PROCEEDINGS

OF

THE ROYAL SOCIETY

OF

EDINBURGH.

VOL. XIV.

NOVEMBER 1886 TO JULY 1887.

EDINBURGH:
PRINTED BY NEILL AND COMPANY.
MDCCCLXXXVIII.

CONTENTS.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. XIV. 1886–87. No. 123.

The 104th Session.

GENERAL STATUTORY MEETING.

Monday, 22nd November 1886.

The following Council were elected:—

President.

Sir WILLIAM THOMSON, F.R.S.

Vice-Presidents.

A. Forbes Irvine, Esq. of Drum.
David Milne Home, Esq. of Milne-Graden.
John Murray, Esq., Ph.D.
Professor Sir Douglas Maclagan.
The Hon. Lord Maclaren.
Rev. Professor Flint, D.D.

General Secretary—Professor Tait.

Secretaries to Ordinary Meetings.

Professor Sir W. Turner, F.R.S.
Professor Crum Brown, F.R.S.

Treasurer—Adam Gillies Smith, Esq., C.A.

Curator of Library and Museum—Alexander Buchan, Esq., M.A.

Ordinary Members of Council.

Professor Chrystal.
Professor Dickson.
Professor Shield Nicholson.
T. B. Sprague, Esq.
Professor Butcher.
Professor M'Kendrick, F.R.S.
Thomas Muir, Esq., LL.D.
Professor M'Intosh, F.R.S.
Robert Gray, Esq.
Dr Arthur Mitchell, C.B.
Stair Agnew, Esq., C.B.
R. M. Ferguson, Esq., Ph.D.

By a Resolution of the Society (19th January 1880), the following Hon. Vice-Presidents, having filled the office of President, are also Members of the Council:—

His Grace the DUKE of ARGYLL, K.T., D.C.L.
The Right Hon. LORD MONCREIFF of Tulliebole, LL.D.
THOMAS STEVENSON, Esq., M. Inst. C.E.

Monday, 6th December 1886.

JOHN MURRAY, Esq., Ph.D., Vice-President, in the Chair.

1. Chairman's Opening Address.

It is my privilege to welcome you at the commencement of this new Session, which, from many indications, promises to be one of great activity among the Fellows of the Society.

Since the close of last Session our esteemed President, Mr Thomas Stevenson, has placed his resignation in the hands of the Council, and, although asked to reconsider his decision, he urged, after consultation with his physician, that, on account of his failing health, he could not continue to hold a post, the duties connected with which he was quite unable efficiently to discharge. I feel sure the Fellows will join with me in hoping that Mr Stevenson may soon be blessed with a return of good health, and that he may long continue to be a contributor to the work of the Society.

Our new President, Sir William Thomson, is not a stranger to the office; when he last occupied the Presidential chair he conferred many lasting benefits on the Society, and his re-election augurs well for the future.

The vote recently taken among the Fellows, with reference to the proposal to change the hour of the ordinary meetings of the Society, has resulted in a majority for the usual hour of meeting being retained. Still, as there was a large minority in favour of some change, the Council will most probably arrange to hold some meetings at four o'clock in the afternoon during the present Session.

Whether we look at the membership of the Society, the extent and value of its publications, or the general activity of the members with reference to scientific investigations, we have every reason to congratulate ourselves on its prosperous condition, and to cherish the notion that the Society has entered on the second century of its existence with a vigour and prospect of usefulness unknown even at any previous period of its career. This very prosperity, however, brings with it new duties and responsibilities. Some new matters

of vital importance to the welfare of the Society are now forced on the consideration of the Council and Fellows: it is to some of these that I propose to refer this evening.

The membership of the Society, including Foreign and Honorary Fellows, numbers at present 507, which is just about the strength of the Royal Society of London. The number of ordinary Fellows is, however, increasing at a somewhat rapid rate, and it is freely discussed, both among the Fellows and in the Council, whether the time has not arrived when only a limited number of Fellows should be elected each year;—after the manner of the election to the fellowship of the Royal Society of London and some foreign societies.

At present, when a candidate is proposed by four or more Fellows, the application remains for several weeks under the consideration of the Council, and thereafter, if the Council be of opinion that the candidate is likely to be a useful member of the Society, he is recommended to the Fellows for election. It would be a mistake to suppose that all the names submitted to the Council are, as a matter of course, recommended for election; it not unfrequently happens that names are withdrawn while under the consideration of the Council, and some never pass the Council.

I have no hesitation in saying that I believe it would be a great mistake to depart from this method of election, which has worked so well in the past, and has secured as Fellows of the Society representative men of all social positions, and from every department of human activity and effort.

Why should we seek to limit the membership? Every energetic scientific man, and every man who is able and willing to assist in any way in the discovery of new facts, new principles, new processes, new knowledge, is a new strength to the Society, and should be welcomed. If thirty or forty such men become candidates in each year, let us have them all as Fellows; the day has passed when it is possible to number the elect either in science or literature!

If we were to adopt the system of selecting a definite number from the candidates of each year—this means the placing of these candidates in a sort of competition with each other for the vacancies, a most objectionable thing among grown-up men—we place a very disagreeable duty on the Council;—canvassing would arise; dis-

satisfaction would follow, however carefully the selection be made; some men who would be most desirable Fellows would be prevented from ever becoming candidates, and offence would be given to the unsuccessful, whose names would ever afterwards be pilloried in the *Proceedings* of our Society. A less objectionable plan would be for the Council to invite a certain number of representative men to become Fellows each year, but it is doubtful if this would work well so long as fellowship involves the payment of fees. There seems to me nothing to fear from an increase in the number of Fellows from year to year. The larger the membership, the more are we likely to be in sympathy with the rapidly increasing number of the general public who interest themselves in the acquisition of new knowledge. We will be all the more powerful when we approach the Government on matters of public interest, and none the less able to give advice in Scottish scientific affairs. In his anniversary address, a few days ago, to the Royal Society of London, the President suggests that that Society should be strengthened by the election of a number of distinguished literary men. This appears to be an admission that the election of fifteen from among candidates each year does not secure that diversity which it is desirable to have in a representative Society, and furnishes an additional argument against the adoption of such a method of election in our Society.

One of the best evidences of the prosperity of the Society is to be found in the great increase in the size and value of the Society's publications. If we include the extra volumes on the Ben Nevis observations and on the Botany of Socotra, which will shortly be issued to the Fellows, then the *Proceedings* and *Transactions* of the Society during the past three years probably surpass in bulk and importance those of any other Society in the United Kingdom for the same period. This must be gratifying to the Fellows, for the money value of the publications in these years is greater than the sums paid in annual fees. The illustrations for these papers have, it is true, been a great drain on the funds of the Society, but the money has been well spent. Just as it is the function of a Society like this to publish in great detail new observations and discoveries, which it would not pay an ordinary publisher to undertake, so should the illustrations of these papers be kept up to a high standard

of artistic merit. It is too often the case that the funds of Scientific Societies demand that the cheapest and not the best shall be undertaken in the matter of illustrations.

I commend it to some of the richer Fellows, if they might not see their way to giving or bequeathing to the Society a fund from which the Council might draw to assist in the careful and artistic execution of such illustrations as may be inserted in the Society's publications.

The magnificent and valuable library of the Society, which Mr Gordon informs me now approaches to twenty thousand volumes, has been acquired chiefly by obtaining the *Transactions* and *Proceedings* of the other learned societies in exchange for our own. Foreign societies have always paid us the compliment of placing a high value on the work done by our Fellows, and published by us. As a consequence we have not only the publications of the great academies which have been long established in the great capitals and other cities of Europe and America, but contributions have flowed into the library from the remotest parts of the world: from Shanghai, Hong-Kong, Japan, and Java in the far east, as well as from our great dependencies and colonies of India, Australia, and New Zealand. Again, we get *Transactions* and *Proceedings* from the far west: from Rio de Janeiro, Buenos Ayres, Bolivia, and Mexico, as well as from many rising cities and towns in the United States and Canada.

At every meeting of the Library Committee fresh proposals of exchange from Societies and Universities, not in scientific communication with us, have to be considered. As education and enlightenment penetrate the various countries of the world, and extend their vital influences to new centres of population, new societies are formed, and, with the same desire for exchange on the part of the new societies daily springing into life and activity, the acquisitions of our library must go on increasing at a rapid rate, not only from old but from new sources. In addition there are the donations from Fellows and others, and the purchases which are made annually. Unfortunately, the space at our disposal is now so limited and inadequate that a considerable part of the library cannot be referred to or consulted. This fact cannot be too widely known, in order that active steps may be taken to provide a remedy.

That remedy is not far to seek; in the new arrangements which must follow the removal of the Antiquarian Museum, the Society's library should be spread over the greater part of this noble building, which could not be devoted to a higher or better purpose than accommodating, for the behoof of the leading scientific society of Scotland, the literature of the learned Societies of all nations,—the accumulated records of the scientific researches of the world.

The Council has considered it expedient to lay these facts regarding the library before the Board of Manufactures, and has requested the favourable consideration of the claims of the Society for more space in this building.

For the prosecution of scientific investigations the *Transactions* and *Proceedings* of learned societies are not only invaluable, but indispensable. They fulfil the twofold purpose of showing what has been accomplished in each department of science, and of serving as starting points, on our part, of new advances into the unknown.

Mere scientific compilations, as distinguished from monographs, however useful to the student, are comparatively of little service to the investigator who is striving to extend the boundaries of his science; and it may be affirmed that many original researches, which have conferred much honour on this Society, could not have been successfully prosecuted without the aid derived from the literature received from similar institutions showing what had most recently been done in the same departments by workers in other parts of the world.

It could be wished that the funds at the disposal of the Society would permit of the acquisition of the records of scientific voyages, which embody valuable original observations and investigations, as well as numerous special monographs. Meanwhile, it is gratifying to know that among the large and varied collection of memoirs existing in this library, there are some which were searched for in vain in the greatest of the metropolitan libraries, although that is annually supported by large Government grants.

Let me give an illustration of the value of this Society and its library to the community.

When some six years ago I succeeded to the direction of the work connected with the publication of the scientific results of the "Challenger" Expedition, there was then no reason why the work

should be continued in Edinburgh, and I was on the point of recommending the Government to transfer the office to London, chiefly on account of the difficulty connected with the access to a fully equipped scientific library. Had the recommendation been made it would, without much doubt, have been adopted. It was not made, because I was assured by the Council of this Society that every facility would be given to me, my assistants, and strangers who might be engaged on "Challenger" work, for consultation of books in the Royal Society library, and at the same time the University conferred certain privileges of a somewhat similar kind.

I suppose it is not altogether a matter of indifference to Scotsmen to know that the work connected with the largest scientific publication ever issued by any country or age, has been chiefly carried out in Edinburgh, or that Edinburgh would have liked it to be said that, having once been commenced here, it was impossible to carry it on in this city. But over and above the mere sentimental aspect of the matter, the retention of the "Challenger" Office in Edinburgh has been a distinct material advantage to the country, for the work which has been given to printers, binders, lithographers, artists, wood engravers, and others, represents the expenditure of many thousands of pounds annually. Then, there is the indirect advantage of having many scientific men from abroad coming and carrying on work here for short periods of time, and I am bound to say some of them would have remained much longer had the library facilities been better. Again, some special industries, such as lithography, have in consequence been greatly developed in our midst. When the "Challenger" work was first commenced, it was believed to be impossible to have the finest kind of lithographic and engraving work done in the United Kingdom; some authors even stipulated that their illustrations should be done abroad. But now this lithographic work can be done as well here as anywhere in the world, if not better; and I frequently receive from abroad requests to have this kind of work undertaken by Edinburgh firms. It seems to me, then, that anything that can be done to increase the completeness or accessibility of the Royal Society library is to be regarded, not as a favour conferred upon a small body of savants, but as a direct boon to the city and the country of which it is the metropolis.

A Society like ours has a very special interest in seeing a truly

complete national library established in Scotland, for to scientific and literary investigators such a library is the most important instrument of research. We must all of us rejoice that there is to be a Free Library in Edinburgh, and it is to be hoped that it will be established so as to give the public the freest possible access to all the ordinary standard works and current literature. It may even be hoped that the directors of the institution will be able to specialise to some extent in the reference library, so as to embrace works not now to be had in Edinburgh.

It must be remembered, however, that this Free Library will mainly be a duplication of books already in Edinburgh libraries. It in no way solves the question of a National Scottish Library, which is the great desideratum for all engaged in the study of science and literature.

There is a great wealth of libraries in Edinburgh, but they are to a large extent inaccessible from want of room or from the antiquated machinery connected with their proper consultation. There is a popular belief that the Advocates' Library contains every English book. As a matter of fact, it is very defective in the literature of some periods; it lacks many important provincial publications, and is of course very deficient in Indian, Colonial, and American works, which are every year becoming more important. There are relatively few foreign works in the Advocates' Library, and were it not for the excellent series of foreign treatises in the University Library, Edinburgh would be very poorly supplied in this respect.

If I wish to consult all the authorities who have described the ice of the Antarctic regions, I can find only some of the books in Edinburgh. If I wish to consult the original authorities who have described the desert of Sahara, I can find only one or two of them in the Edinburgh libraries. Gaps like these are not confined to scientific or geographical works. Socialism is a subject not in the background at present, yet one will seek in vain among our libraries for some of the works of the best-known continental exponents of the theory.

Every true Scotsman has an interest in seeing these defects speedily remedied.

It is much to be regretted that there is no organisation by which reference to the various libraries in Edinburgh could be facilitated

to those engaged in scientific and literary work. I have never received a book from the Advocates', Physicians', and some other libraries without being indebted to the courtesy of some member of these bodies; and although this be always willingly given, it becomes irksome to all parties when repeated week after week. In connection with our "Challenger" work, we often find it more convenient and expeditious to get books from London than from certain of the Edinburgh libraries. It is not for the credit of the city that this should be the case. I cannot but think that it would be a great advantage to form a central board, composed of representatives of the different library-owning societies and corporations of the city. Such a board would do good service by preventing the duplication of purchases among the various libraries where unnecessary. It might have the power of granting the privilege to investigators of consulting all the libraries to a limited extent; but, more important still, it might draw up a scheme for a National Library—a scheme which, while allowing existing libraries to develop on their present lines, would yet erect one of them, say the Advocates', into a National Library, whose function it would be to supplement or fill up in those departments not embraced by the other libraries. It seems to me that some such scheme would command support.

When we remember the large sums of public money that are annually spent on national libraries in London, and that in addition to the cost of buildings and maintenance, about £18,000 has been spent during the past ten years on salaries and purchase of books for the National Library in Dublin, then surely the claims of Scotland deserve some consideration. Were a good workable scheme drawn up by some of our leading men, and supported by the public generally, then even Scottish Parliamentary representatives might awake to the fact that there are some Scottish questions worthy of their attention and combined action.

Some time ago the Council of the Society drew the attention of the Government to the fact, that no bathymetrical survey of the Scottish freshwater lochs existed, except those of Loch Lomond and Loch Awe; and urged the importance, in many branches of scientific inquiry, of knowing the depth and form of such basins as Lochs Morar, Maree, Lochy, Assynt, Linn, Tay, Ericht, with many others,

and expressed the hope that these surveys would be undertaken at an early date, and, at all events, before the completion of the Ordnance Survey of the country. The reply from the Treasury was that these surveys could not be sanctioned, because they did not come within the function of the Board of Admiralty or of the survey department of the Office of Works. This matter was subsequently brought up in Parliament by Lord Balfour of Burleigh, but no steps seem to have been taken to carry out the survey. It may be hoped that this matter will not be allowed to drop. Quite recently soundings of 175 and 180 fathoms have been obtained in Loch Morar. This is the greatest depth that has hitherto been found on the plateau on which the British Islands are situated; to get depths equal to this we must go towards the deep gut off the coasts of Norway, or beyond the 100-fathom line off the coasts of Ireland. There are also geological and biological problems of great interest connected with the depths of these lochs.

Should these surveys not be undertaken a very important part of the survey of the United Kingdom will be left untouched; for it cannot be denied that it is at least as important—sometimes much more important—to know the depth of a lake than to know the height of an adjoining mountain. It would be a matter for great regret if the admirable surveys, which are now drawing to a close, and which reflect so much credit on the officers who conducted them, and honour on the scientific reputation of the country generally, should be lowered in value by the great omission here pointed out.

The Council has recently had before it the subject of Antarctic exploration, and has drawn up and printed a number of suggestions as to the investigations which should be undertaken or attempted in the event of such an expedition being fitted out. There can be little doubt that a thorough exploration of these unknown regions would enrich almost every branch of science with valuable observations; the Antarctic appears to exert a controlling influence on the atmospheric and oceanic circulation and magnetism of the globe, and presents many interesting physical, geological, and zoological problems. No steam vessel, protected for ice, has ever penetrated these seas, and there are good reasons for believing that such vessels would be able to find a place for wintering close to the land of the Antarctic

continent. We have at present no knowledge of the condition of the Antarctic regions except during the summer months of January, February, and March, and, although the first endeavour to pass a winter in these regions would doubtless be accompanied with considerable risk, still it must be attempted, and the duty of attempting it lies heavier on Great Britain than any other nation. If Great Britain is to hold her proper position among the family of nations she must explore the Antarctic, and it is to be hoped that the numerous learned Societies of the United Kingdom will before long press the matter on the attention of the Government.

During the past few years there has been great activity in the examination of the biological conditions of the coasts, lochs, and estuaries of Scotland, and some of the more important results have appeared, or are about to appear, in the *Transactions* and *Proceedings* of the Society.

In connection with the Scottish Marine Station, carried on under the auspices of the Scottish Meteorological Society, there has been conducted during the past three years a very valuable series of investigations into the physical and chemical conditions of the Firths of Forth and Clyde, and various rivers and estuaries, which are of great importance to a right understanding of the general meteorology of the country.

The little steamer of the Station, fitted with the most approved apparatus, has been constantly at work at all times of the year. Three years' observations on the Forth have given the general conditions with regard to temperature and salinity for all seasons, with the laws of their changes.

About the time of the vernal equinox all the water of the Firth is of a uniform temperature; there is a gradient of temperature from river to sea and from surface to bottom in summer, the warmest water being on the surface and towards the land. In winter this state of matters is entirely reversed, at the autumnal equinox there being again a uniform distribution of temperature.

Nearly a year's observations have been completed on the much more complicated and varied region of the Clyde. Here a vast amount of heat is stored up in the waters of the deep lochs during summer, and slowly given out again to the air during the winter months, thus greatly modifying the climate of the West Coast of

Scotland—a condition of things to which there is apparently nothing analogous on the East Coast.

This influence is due to the great depth of the lochs and their exposure to summer heat, and does not appear to be entirely independent of the effects produced by the warm Atlantic water, which flows over the shallow ridge stretching from Cantyre to the Ayrshire coast. Some of the preliminary results of these researches have been presented to the Society by Dr Mill, but all the observations are now in course of preparation for publication, and will furnish physicists and meteorologists with many much-needed data.

I have already referred to the extra volume of the Society's *Transactions*, containing the Ben Nevis Observatory observations, which will shortly be in the hands of the Fellows. Among meteorologists in all parts of the world there has long been a desire expressed to be furnished with copies of the Ben Nevis observations, and as the Directors have no funds for the purpose, the Council have in the circumstances considered it a duty to undertake their publication.

The Fellows are reminded that the Observatory buildings and the road thereto are the property of the Royal Society of Edinburgh, that several donations from the Society's funds were made towards covering the expenses of the preliminary observations before the erection of the permanent Observatory, and that the direction of the Observatory is largely in the hands of the representatives and Fellows of this Society. The Society has, therefore, a deep interest in all that concerns the welfare of this unique high-class Observatory.

As you are aware, the Observatory was erected in the summer of 1883, and formally opened by Mrs Cameron Campbell of Monzie, the proprietrix of the land, on 17th October; immediately thereafter Mr Omond and two assistants went into residence, and the regular work of hourly observations began in the end of November; the Observatory was equipped with the best instruments that could be obtained, several of these being of a novel character, suited to the peculiar climate of the Observatory and to the new lines of observation it was proposed to carry out.

Ben Nevis was selected not merely as the highest mountain in

Great Britain, and situated in the very track of the south-westerly winds from the Atlantic, which exercise so preponderating an influence on the weather of Europe, but because it rises to a height of 4406 feet so close to the sea that a sea-level station may be placed at about 4 miles from the summit—a consideration which gives a value altogether unique to the observations.

The low-level station was established at Fort William, under the charge of Mr Livingstone, of the Public Schools, at which observations are made five times each day, and with these are conjoined a barograph and thermograph for the continuous record of these important elements of climate—atmospheric pressure and temperature. Three years' observations at this pair of stations have already been made, and it is these which are now in the hands of the printer, and will shortly appear as a volume of the Society's *Transactions*.

The climatic difficulties were great, but they were successfully surmounted (1) by the skill of the architect, Mr Sydney Mitchell, who constructed the buildings, (2) by the heroic endurance and fertility and readiness of resource displayed by Mr Omond and his staff of assistants in meeting emergencies as they arose.

At most, if not all, other observatories only a comparatively few of the observations are made by the observers personally, the usual course being to use continuously recording instruments from which the omitted hours are interpolated. But on Ben Nevis every recorded observation is actually noted by the observers. Further, it usually happens that the observations of the temperature, humidity, and movements of the air, and the rainfall are automatically recorded. But on Ben Nevis, during the larger portion of the year, owing to the snow-drifts and ice-incrustations formed on the instruments as well as on everything outside the Observatory exposed to the wind, these observations cannot, and, it may be confidently predicted, never can, be made by self-recording instruments. Hence, if meteorology and weather prediction are to make advances in those fundamental inquiries, which can be successfully prosecuted only by the help of high-level observatories, the time will never come when such heroic services as are now rendered to science by Mr Omond can be dispensed with. Even barometric observations could not be utilised in these inquiries, unless there be conjoined with them observations of the temperature and the movements of the atmosphere.

The high expectations formed of the great importance of this Observatory have already been more than realised. The following are among the more prominent of the results obtained from this pair of high and low level stations:—

1. The rate of decrease of temperature with height has been more correctly ascertained.

2. The rate of decrease of atmospheric pressure with height for the different sea-level pressures and air-temperatures has been ascertained with a high degree of accuracy.

3. The relations of the readings of the dry and wet bulb hygrometer to the vapour of the atmosphere have been worked out, under the extraordinarily dry states of the air which are of such frequent occurrence on the top of the mountain, from observational data to a degree of accuracy not hitherto attained.

These results will, doubtless, in future be incorporated in all books on meteorology and general physics.

As regards weather forecasting, the Ben Nevis observations contribute information of such a value as no low-level observatory, however efficiently equipped and superintended, can for a moment lay claim to; and it is to be enforced here, that when the Directors are placed in a position to raise the five daily observations at Fort William to twenty-four, the value of the Ben Nevis observing system will be immensely enhanced. In other words, the Directors of the Observatory attach the greatest importance to the establishment of a low-level observatory at Fort William, at which observations could be made with the same fulness as at the Observatory on the top of the mountain.

The great value attached to the observations of high-level observatories is attested by the continued additions made to these stations in different parts of the world, and by the observations made at these stations many of the more important questions of meteorology may doubtless be investigated. But there is no other high-level observatory, and its conjoined low-level station, so happily situated as Ben Nevis Observatory and the station at Fort William for supplying physicists with observational data. This is due to the fact that the former is on a peak and the latter close to the sea, the ground sloping down to it, by which the effects of solar and terrestrial radiation are minimised.

It is only a few months ago that an inquiry, conducted on scientific principles, into the bearing of the observations at the Observatory and at the base of the mountain could be entered upon, and its completion will necessarily occupy some time, owing to the complex character of the problems to be dealt with.

The erection and maintenance of the Observatory during the past three years have cost over £7000, the funds having been obtained solely from private subscriptions and learned Societies. During this time the Directors have had to pay to the post office department a rental of £133 per annum for the use of the telegraph wire from Fort William to the top of the mountain. In addition to this, the post office have received considerably over £100 for tourist messages forwarded from the Observatory by Mr Omond and his assistants. The daily despatch of the observations to the press throughout the country is also a source of considerable income to the post office department. It appears then that this Observatory, established solely for the purposes of scientific investigation, instead of being assisted by the Government, is actually a source of revenue to a Government department.

You are aware that a sum of £15,000 is voted annually by Parliament for general meteorological purposes, but more particularly the meteorology of the British Islands. This grant is administered by a committee nominated by the Royal Society of London, called the Meteorological Council. Although Ben Nevis Observatory is certainly the best and most important meteorological observatory in the United Kingdom, yet it has received no support from this grant, if we except the annual sum of £100 which is paid on condition of being supplied with a complete set of all the observations,—a bare equivalent for the mere clerk work required. In 1884 an application for £300, from the £15,000 grant, towards the expenses of the Observatory was refused.

The Meteorological Council also intimated to the Directors that they did not propose to have the observations at Ben Nevis wired to London for use in making up the weather forecasts, until the result of Mr Buchan's discussion of the observations was completed; but they offered no assistance towards carrying out that discussion, although £1000 of the £15,300 is expressly stated to be for the purposes of original investigation

An application to the Government Grant Committee of the Royal Society of London for aid towards the expenses of the Observatory has likewise been unsuccessful.

The refusal of assistance by the London Committees may be partly due to the fact that there are many claims on the funds which they administer, but it appears also to be very largely due to a want of proper knowledge of what has been done, and what may be reasonably expected to be done by the Observatory, there being no observatory in these islands that can compete with the Ben Nevis Observatory for the accuracy and intrinsic value of the hourly observations; and absolutely no pair of stations anywhere in the world that can be named alongside the Observatory and the station at Fort William, as contributing data in furtherance of our knowledge of storms and the science of weather generally.

The Directors have given much time and thought to the affairs of the Observatory during the past four years, and to many of them it has been a cause of considerable personal expenditure, for the expenses connected with frequent visits made to Ben Nevis for the selection of the site, during the building and equipment of the Observatory, and its subsequent inspections, have in no case been charged against the Observatory funds, but have been borne by the individual Directors. Not only so, but it has happened more than once that some one of the Directors has placed considerable sums to the credit of the Observatory in the bank, to enable the work to go on without an hiatus.

The time has now arrived when it is necessary to place the Observatory on some permanent footing, but before another appeal is made to the public, the Council of this Society has resolved to urge the claims of the Observatory on the consideration of the Government, and to ask for a substantial donation towards the annual expenses. In this, I feel sure, the Council will have the support, not only of the Fellows of the learned Societies of Scotland, but of the general public.

In the infancy of science it was possible, with simple and inexpensive appliances, to discover important facts which were lying as it were on the surface, but no discoveries can now be expected except by rising high above the surface, as in the case of Ben Nevis, or descending far below it, as in the case of the exploration of our

deep lochs by means of a steam vessel and the most recent apparatus for sounding, dredging, and taking temperatures; the consequence is that such scientific investigations cannot be undertaken even by scientific men with private fortunes, and hence follows the necessity for Government assistance.

From the estimates it appears—

1. That the Royal Society of London and five or six other learned societies are accommodated in Burlington House free of rent.

2. That the Royal Geographical Society receives a grant of £500 annually.

3. That there is an annual grant of £15,000 for meteorological purposes, administered by a committee of the Royal Society of London, £1000 of which is to be devoted to original investigations.

4. That a sum of £4000 is administered by the Government Grant Committee of the Royal Society of London for scientific research, and although that society is, in a sense, the representative society of the United Kingdom, still its Council and Committees are of necessity composed of Fellows resident in, or within accessible distance of, London, the Fellows resident in Scotland being practically excluded from active participation in the management of the Society.

5. That the Marine Biological Association has recently received £5000 from the Government, and the promise of an annual grant of £500 for five years, towards the establishment of a laboratory of research in England.

6. That in Ireland the Royal Irish Academy receives a grant in aid of £2000 annually, in addition to free accommodation and about £400 annually for allowances and maintenances; the Royal Zoological Society of Ireland receives a grant in aid of £500 annually; and the Royal Dublin Society appears, during the past ten years, to have received considerable sums of money from public funds; in addition there is a large sum granted annually for a national library in Dublin.

7. That, with respect to Scotland, the only grant for scientific purposes in aid of learned societies is £300 annually to the Royal Society of Edinburgh, which is repaid to a department of the Government in the form of rent.

One might well ask what Scotland had done that her learned

Societies and scientific men should be treated so niggardly as compared with those in England and Ireland. It cannot be because she does no scientific work. It is sometimes said, indeed, that in literary matters Scotland, and especially Edinburgh, is a mere shadow of her former self; but in science this is not the case, and it is towards scientific matters that the great ploughshare of human thought and activity is, in this age, directed. I question if any country in the world, taking into consideration its size, can show a better record of scientific work, or a more excellent volume of scientific literature, than Scotland, during the past ten or twenty years.

There can be no doubt that Scotland has a great grievance in the fact that her learned Societies do not receive the same consideration from Government as do similar societies in other parts of the United Kingdom, and in the fact that the administration of all the grants, in which she may be supposed to have a right to participate, is centralised in London and controlled by London Committees. Scotland was refused a representative on the Meteorological Council, where the expenses of the members are paid. Two representatives of this Society are allowed on the committee of sixty members, which distributes the grant of £4000 for research; but when two members of the Council are sent twice a year to London it costs this Society £50 annually,—a considerable charge against its funds. But there is a unanimous opinion among Scottish scientific men that there should be a grant in aid of the learned societies of Scotland, for the purposes of scientific research, analogous to the Government grant of £4000 to the Royal Society of London, which is distributed by a committee in London. Several of the best qualified men in Scotland hesitate to apply for aid from the London committee, and, especially as many of the younger men have been frequently disappointed, there has sprung up a firm belief that the only satisfactory arrangement for the scientific men resident in the northern part of Great Britain is that there should be a grant for scientific research to be distributed by a committee in Scotland, which would be fully conversant with the nature of the investigations to be undertaken, and personally acquainted with the applicants. There can be little doubt that such a grant distributed locally would have the effect of removing many of the barriers which at present impede the progress of science in Scotland.

It is the primary object of our Society to promote the interests of science and literature in Scotland, more especially in all that relates to the extension of the boundaries of knowledge by the discovery of new truths, as distinguished from the making known of old truths. It claims, therefore, the right to memorialise the Government on scientific affairs which it considers of national importance, and in purely Scottish matters it holds that its President and Council, and not the President and Council of the Royal Society of London, should be the advisers of the Government.

The Council has, at various times in the past, called the attention of the Government to the disadvantages under which Scotland laboured in respect of aid to research, but the result has never been satisfactory. In the attempt which the Council is now about to make I would bespeak the co-operation and support of all Scotsmen who believe it to be for the honour and well-being of the country that our scientific institutions should not languish, or our scientific men be discouraged, but that both should be urged to new advances and greater conquests.

Hume Manuscripts.

Before the reading of the papers the Chairman made the following statement:—

Some reference should, I think, be made to the concluding paragraph of a review of the life of Hume, which recently appeared in the *Scotsman*. It is as follows:—

"Dr Knight has spared no pains to add by his own independent research to the information given in J. Hill Burton's work. He mentions, however, that 'he has not been able to obtain access to the volume of Hume MSS. in the custody of the Royal Society, the Secretary being of opinion that Mr Hill Burton had sufficiently examined these.' Has the Secretary of the Royal Society any such right to bar the way to further genuine research? How does he know that nothing has been omitted and nothing has to be verified? This volume was not given as a present to the Royal Society to keep as a curiosity in a glass case for its peculiar benefit, but given to them as custodiers for the public. The decision of Professor Tait amounts to asserting that this volume is never more to be seen by mortal eye. It is well known that Hume entrusted to the Society

his correspondence with Rousseau (after offering it to the British Museum), not merely in order that it might be preserved, but that the world might be able to learn from it the true story of a famous feud. If MSS. of purely literary or philosophical interest are to be locked up jealously in the archives of a Society which is not literary at all, but scientific, the purpose of the depositors is defeated, and the insistance on the private right of the Society becomes a public wrong."

As this article, if unanswered, might give rise to serious misapprehension as to the action of the Society and of our General Secretary, it is advisable to state—

1. The Royal Society of Edinburgh is, by its constitution, quite as much a literary as a scientific society.

2. The General Secretary is not responsible for the decisions of the Council of the Society, except in so far as he is, *ex officio*, one of the twenty-seven members of Council.

3. The Hume MSS. were bequeathed to the Society by Baron Hume in 1838, and they apparently contain papers which Hume in his last will left particular instructions should be destroyed.

4. The Council of the Society has on several occasions since 1838 appointed committees to report as to the access that should be given to these MSS.

5. The Council, recognising the duty of making public the contents of the Hume MSS., and at the same time being anxious to prevent the dissemination of mere hearsay scandal, affecting the characters of men whose descendants are still among us, requested the late Dr Hill Burton—whose competence no one will question—to examine the MSS. and publish them so far as should seem at once necessary and prudent.

6. More recently a committee of experts appointed by the Council (on which are some of the most distinguished literary men in Edinburgh) has for a considerable time been engaged in the laborious work of reperusal of these MSS., with the view of deciding how far further publication of their contents may now be possible.

7. Pending the final decision of this committee, the Rousseau portions of the MSS. have been for some time open to the inspection of, and have been consulted by, investigators.

The following Communications were read:—

2. Astronomical Tables for facilitating the computation of Differential Refraction, for Latitudes 56° and 57°·30. By the Hon. Lord M'Laren.

3. On the Foundations of the Kinetic Theory of Gases. By Professor Tait.

In a former paper, printed in *Trans. Roy. Soc. Edin.*, 1886, I showed that the recovery of the "special" state by a gas supposed to consist of equal hard spheres takes place, at ordinary pressures and temperatures, in a period of the order of 10^{-9} seconds, at highest.

This forms the indispensable preliminary to the present investigation. For it warrants us in assuming that, except in extreme cases in which the causes tending to disturb the "special" state are at least nearly as rapid and persistent in their action as is the tendency to recovery, a local "special" state is maintained in every region of the space occupied by a gas or gaseous mixture. This may be, and in the cases now to be treated is, accompanied by a common translatory motion of the particles (or, of each separate class of particles) in the region—a motion which at each instant may vary continuously from region to region, and may in any region vary continuously with time.

A troublesome part of the investigation is the dealing with a number of complicated integrals which occur in it, and which (so far as I know) can be treated only by quadratures. All are of the form

$$\int_0^\infty \frac{\nu v^r}{e};$$

where ν is that fraction of the whole number of particles of one kind per cubic unit whose speeds (relatively to those of the same kind, in the same region, *as a whole*) lie between v and $v+dv$; and $1/e$ is the mean free path of a particle whose speed is v. Throughout the paper regard has been had to the fact that e must

be treated as a function of v. So long as the particles are of the same kind, or at least of equal mass if of different diameters, such integrals are easy to evaluate; but it is very different when the masses differ in two mixed gases. In what follows, the merely numerical factor of the expression above will be denoted by C_r, so that the value of the expression is, when the masses and diameters are equal, $C_r/n\pi s^2 h^{r/2}$, and the introduction of different diameters merely introduces another factor. Here $3/2h$ is the mean square speed, n the number of particles per cubic unit, and s their common diameter.

When the masses are unequal there will, in general, be different mean free paths for particles of two different kinds, and the integrals cannot be simplified in the above way. In this case the integrals will be expressed as ${}_1\mathfrak{C}_r$, ${}_2\mathfrak{C}_r$.

(1) In my former paper I showed that the Virial equation is, for *equal* hard spheres exerting no molecular action other than the impacts,

$$n\mathrm{P}\overline{v^2}/2 = \tfrac{3}{2}p(\mathrm{V} - 2n\pi s^3/3),$$

where n is the number of particles, P the mass of one, s its diameter, $\overline{v^2}$ the mean-square speed, p the pressure, and V the volume. The quantity subtracted from the volume is four times the sum of the volumes of the spheres; and I pointed out that this expression exactly agrees in form with Amagat's experimental results for hydrogen, which were conducted through wide ranges of pressure and between 18° C. and 100° C.

In a mixture of equal numbers of two kinds of particles, of diameters s_1, s_2, I find that for s^3 in the above formula we must put

$$\tfrac{1}{4}(s_1{}^3 + 2s^3 + s_2{}^3),$$

where $s = (s_1 + s_2)/2$. Thus the "ultimate volume" is increased if the sizes of the particles differ, though the mean diameter is unaltered.

(2) For the coefficient of viscosity in a single gas the value found is

$$\frac{\mathrm{P}n\mathrm{C}_1}{3\pi n s^2\sqrt{h}} = \frac{\rho\lambda}{\sqrt{h}}\,0{\cdot}412,$$

where ρ is the density, and λ the mean free path. The product $\rho\lambda$ is the same at all temperatures, so that the viscosity is as the square root of the absolute temperature.

(3) The steady linear motion of heat in a gas is next considered, temperature being supposed to be higher as we ascend, so as to prevent complication by convection. It is assumed, as the basis of the inquiry, that :—

Each horizontal layer of the gas is in the "special" state, compounded with a vertical translation which is the same for all particles in the layer.

The following are the chief results :—

(*a*) Since the *pressure* is constant throughout, we have

$$p=\frac{\mathrm{P}n}{2h},$$

so that n/h is constant.

(*b*) Since the motion is steady, no *matter* passes (on the whole) across any horizontal plane. This gives for the speed of translation of the layer at x,

$$a=\int_0^\infty v\left(\frac{dn}{dx}\Big/n+\frac{dv}{dx}\Big/v\right)v/3e.$$

(*c*) Equal amounts of *energy* are (on the whole) transferred across unit area of each horizontal plane, per unit of time. The value is

$$\mathrm{E}=-\frac{\mathrm{P}}{6}\int^\infty nvv^3\left(\left(\frac{dn}{dx}\Big/n+\frac{dv}{dx}\Big/v\right)\Big/e-5a/v\right).$$

By the above value of p, and its consequence as to the ratio n/h, these expressions become

$$a=\frac{dh}{dx}h^{-\frac{5}{2}}\frac{\mathrm{P}}{6p\pi s^2}\left(\frac{5}{2}\mathrm{C}_1-\mathrm{C}_3\right)=\frac{dh}{dx}h^{-\frac{5}{2}}\frac{\rho\lambda}{p}\,0\cdot06,$$

$$\mathrm{E}=\frac{dh}{dx}h^{-\frac{5}{2}}\frac{\mathrm{P}}{6\pi s^2}\left(\frac{25}{4}\mathrm{C}_1-5\mathrm{C}_3+\mathrm{C}_5\right)=\frac{dh}{dx}h^{-\frac{5}{2}}\rho\lambda\,0\cdot45.$$

Since E is constant, by the conditions, we see that a also must be constant. Hence as $h\tau$ (where τ is absolute temperature) is constant, we have $\tau^{\frac{1}{2}}\frac{d\tau}{dx}$ constant, or

$$\tau^{\frac{3}{2}}=\mathrm{A}+\mathrm{B}x,$$

which, when the terminal conditions are assigned, gives the steady distribution of temperature. The motion of the gas is analogous to that of liquid mud when a scavenger tries to sweep it into a heap. The broom produces a general translation which is counteracted by the gravitation due to the slope, just as the translation of the gas is balanced by the greater number of particles escaping from the colder and denser layers than from the warmer and less dense.

In thermal foot-minute-centigrade measure, the conductivity of air, at one atmosphere and ordinary temperatures, appears from the above expressions to be about

$$\sqrt{\frac{\tau}{274}}3 \cdot 10^{-5},$$

or about 1/28,000 of that of iron. No account, of course, is taken of rotation or vibration of individual particles.

4. Fog Bow observed on Ben Nevis, 22nd October 1886. By R. T. Omond, Supt. B.N.O.

(See *Proceedings* for June 20, below.)

5. Temperatures at Different Heights above Ground at Ben Nevis Observatory. By R. T. Omond, Supt. B.N.O.

During part of the recent summer (1886), in addition to the ordinary temperature observations here, a set of readings were taken in a Stevenson screen, at a height of 112 inches above ground—that is, at about two and a half times the standard elevation of 48 inches. The high level screen was mounted at the top of a stand used to carry the thermometers in winter, and consisting of two stout upright posts or battens, with cross bars between them at every 2 feet or so; the screen is placed with the lower edge of the back resting on one bar, and is tied to the one above it or to the side posts. The screen used on this stand is smaller than the standard Stevenson screen, but it is constructed in exactly the same manner, with double louvred sides and the bottom open. It measures inside 10 inches broad by 6 inches deep and 15 inches high. The low level screen, which is mounted on four legs in the

usual manner, is about the ordinary size; it measures 15 by 10 by 15 inches. Readings of both sets of thermometers (high and low) were taken hourly during part of July and the whole of August. During the month of August, as well as these shade temperatures, readings were taken of a black bulb *in vacuo*, belonging to Mr H. N. Dickson, and kindly lent by him for the purpose. In this instrument the thermometer inside the glass globe, instead of being a maximum, is a common thermometer, with the bulb blackened in the usual way. The hourly readings of this instrument indicate the solar radiation at the time of observation, instead of, as in the usual maximum black bulb, giving only the greatest intensity since being last set. In the first column of the table, at the end, the mean hourly values of this black bulb for the month of August are given. It is noteworthy that the maximum is almost exactly at twelve noon; but, as Greenwich time is used, it must be borne in mind that the mean time of the sun's meridian passage is about 12 hours 20 minutes, the longitude being some 5° west. In the second and third columns respectively of the table are the high and low level shade temperatures, and in the fourth the difference between them, for the month of August also. The values of the fourth column are shown graphically in the highest line of the diagram. The average temperature for the whole day is the same in both cases, but the upper thermometer has a slightly less daily range, reading about one-tenth of a degree F. higher at night, and from one to two tenths lower in the afternoon. The result is exactly what would be expected, the upper thermometer being further removed from the ground and less exposed to its radiation, while the smallness of the differences between the two can be accounted for by the exceedingly bad weather of last August: during the whole month there were only two fine days. A comparison of the readings on these two days (the 19th and 22nd), shows a much greater difference. In the fifth column of the table, the mean differences of the high and low level thermometers on the two days is given, and these numbers are also shown in the lower part of the diagram by the line which has the greatest range. Two days is of course far too short a period to give a satisfactory average and a smooth curve, but the general aspect of the line shows that, from about sunrise till noon, the

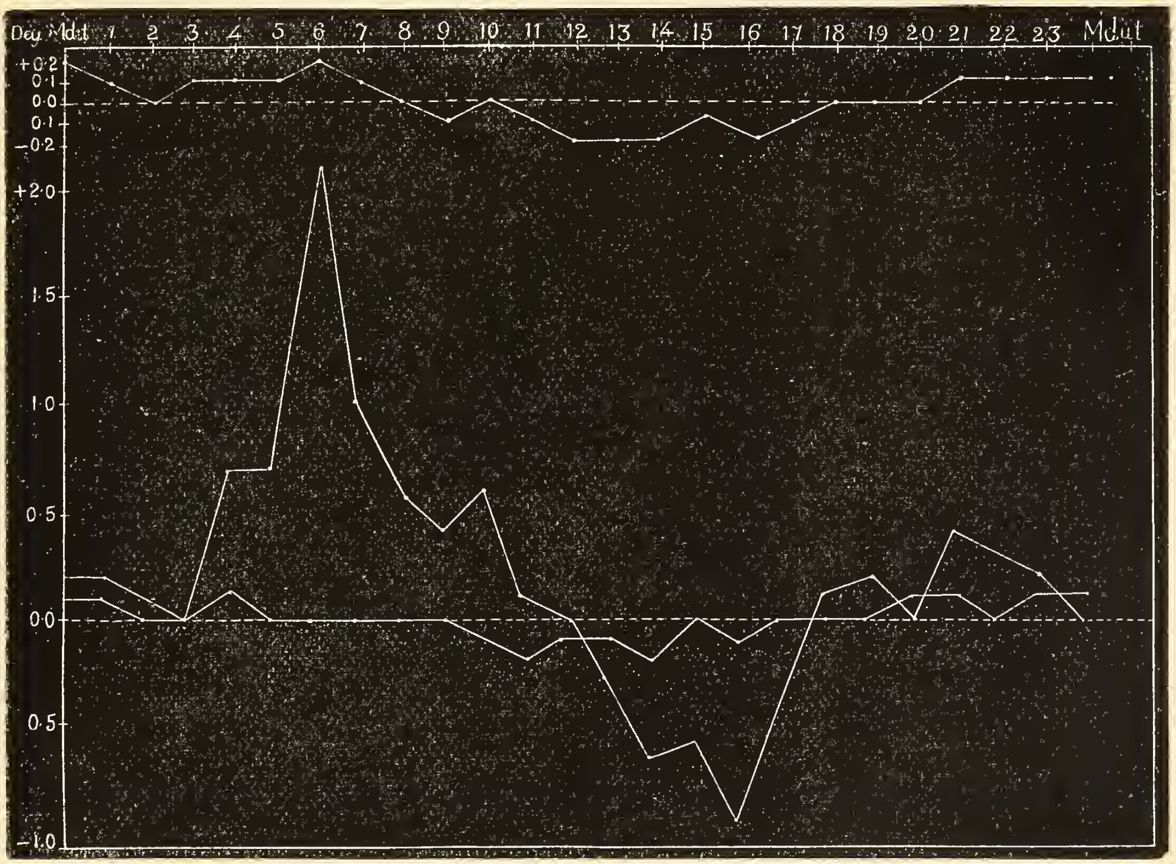
upper thermometer reads distinctly above the lower, from noon to sunset distinctly below it, and during the night slightly above it again. An examination of the black bulb temperatures for these two days shows nothing calling for special note. The range is greater than in the monthly average, but the curve is substantially the same, except for a slight lagging of the maximum point, but the

Hours.	Black Bulb *in vacuo.*	Temp. 48 inches above ground.	Temp. 112 inches above ground.	Diff. of high from low temp.	Mean diff. of 2 fine days.	Mean diff. of 9 foggy days.
Mdnt.	38·8	39·2	39·4	+0·2	+0·2	+0·1
1	38·7	39·1	39·2	+0·1	+0·2	+0·1
2	38·4	38·8	38·8	0·0	+0·1	0·0
3	38·4	38·7	38·8	+0·1	0·0	0·0
4	38·1	38·5	38·6	+0·1	+0·7	+0·1
5	38·8	38·4	38·5	+0·1	+0·7	0·0
6	43·6	38·4	38·6	+0·2	+2·1	0·0
7	47·4	38·9	39·0	+0·1	+1·0	0·0
8	53·6	39·2	39·2	0·0	+0·6	0·0
9	57·9	39·6	39·5	−0·1	+0·4	0·0
10	60·7	39·8	39·8	0·0	+0·6	−0·1
11	64·5	40·3	40·2	−0·1	+0·1	−0·2
12	67·0	40·9	40·7	−0·2	0·0	−0·1
13	64·3	41·3	41·1	−0·2	−0·3	−0·1
14	62·3	41·7	41·5	−0·2	−0·7	−0·2
15	62·2	41·8	41·7	−0·1	−0·6	0·0
16	58·1	42·0	41·8	−0·2	−1·0	−0·1
17	52·5	41·7	41·6	−0·1	−0·4	0·0
18	46·8	41·2	41·2	0·0	+0·1	0·0
19	43·1	40·7	40·7	0·0	+0·2	0·0
20	40·7	40·6	40·6	0·0	0·0	+0·1
21	39·9	40·4	40·5	+0·1	+0·4	+0·1
22	39·8	40·3	40·4	+0·1	+0·3	0·0
23	39·5	39·9	40·0	+0·1	+0·2	+0·1
Mdnt.	39·2	39·6	39·7	+0·1	0·0	+0·1
Mean.	49·0	40·1	40·1	0·0		

period is too short to say anything definite about this. As a contrast to these fine days, I have computed the mean differences on nine days of continuous fog or mist. These differences are given in column six of the table, and are shown by the line in the lower part of the diagram with the least range. Here, though there is still a tendency of the upper thermometer to read higher at night and lower in the afternoon, the differences are very small;

at only two hours do they exceed one-tenth of a degree. This small difference on the foggy days fully bears out what I have formerly observed on Ben Nevis, that in a saturated atmosphere—with mist or fog present, it makes no practical difference how the thermometers are placed, so long as the air can reach them at all and they are shaded from the direct rays of the sun.

Differences of High from Low Thermometers.



As the surface of the hill top consists entirely of broken rock without soil or vegetation, it seems probable that a large amount of the difference between the two thermometers is directly due to ground radiation. On a calm day with bright sunshine the stones get so heated as to be disagreeable to handle, but there were unfortunately no such days last August; still, the radiation of heat during the day and of cold at night during ordinary clear weather must be considerable, and the lower thermometer was much nearer this source of heat and cold than the upper. It should, however, be borne in mind that, as the screens are only open below and the bulbs of the thermometers raised about four inches above the lower

edge of the sides, they can only receive and transmit radiation from and to that part of the ground that would be visible to an eye placed where the bulbs are; and that in theory the greater distance of the upper thermometer from the ground would be exactly counterbalanced by the larger area capable of acting on it, and the radiation effects would be the same in both cases. The thermometers were so mounted that the difference in the size of the screens made little difference in the exposure to ground radiation. If the screens themselves get heated and cooled by ground radiation, they would correspondingly heat and cool the air as it passed through the louvres, and thus affect the thermometers. In this latter case the heating and cooling ought to be inversely as the square of their distances from the ground—that is, the lower screen should be affected nearly six times as much as the upper.

The afternoon difference may also be due to convection currents arising from the heated ground; these would affect the lower thermometer more than the upper, but it is difficult to see how any such cause could give the morning difference, in which the upper thermometer reads above the lower. It is possible, however, that in fine weather the layer of air next the ground is so much cooled by contact with the ground that there is a continuous gradient of temperature rising with height above ground, at least as high as the level of the upper screen. I hope to repeat the experiment under more favourable conditions of weather, and also, if possible, when the ground is covered with snow.

PRIVATE BUSINESS.

Messrs Asutosh Mukhopadhyay, M.A., &c., Joseph James Coleman, and John James Burnet were balloted for, and declared duly elected Fellows of the Society.

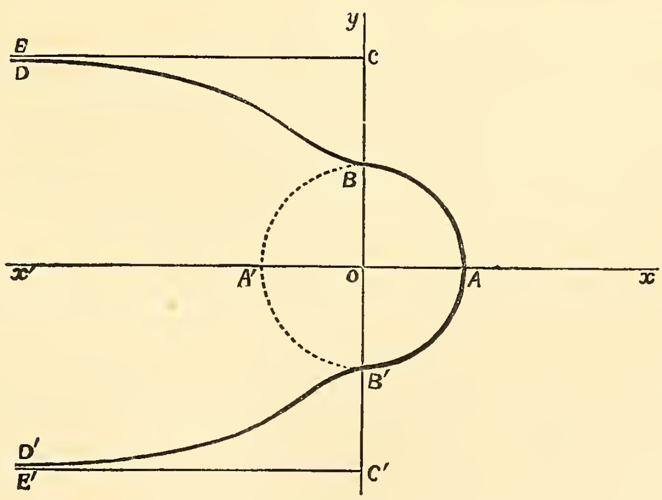
Monday, 20th December 1886.

Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. Motion of Compound Bodies through Liquid. By the Rev. H. J. Sharpe, M.A. Communicated by the President.

It is well known that the solution of the mathematical problem of the motion of liquid arising from the motion through it of solids has been effected in comparatively few instances, and in these, such as the sphere and the ellipsoid, the surface is defined by a single equation. In the following investigation, however, we seem naturally to come across cases where the solid might be described as "a compound body," consisting as it does of parts defined by different equations. It is possible that the method employed might lead to the solution of analogous problems in Sound, Heat, or Electricity.



DBAB′D′ is the trace on the plane of the paper of a material cylindrical surface, which is supposed to move through an infinite mass of water in direction OA with a velocity which at the instant

considered is c. BAB′ is a semicircle with centre O, and the other parts of the curve are symmetrical with respect to OA. If $OA = a$, the polar equation of BD, referred to OA as prime radius, is supposed to be

$$\theta - \frac{\pi}{2}\frac{r}{a}\sin\theta + \frac{2}{1.2.3}\frac{a^2}{r^2}\sin 2\theta - \frac{2}{3.4.5}\frac{a^4}{r^4}\sin 4\theta$$
$$+ \frac{2}{5.6.7}\frac{a^6}{r^6}\sin 6\theta - \&c. = 0 \quad . \quad (1)$$

Then the resulting irrotational motion of the liquid, supposed to be in two dimensions only, can be completely determined. It may be remarked that the series in (1), as well as all the like series in this paper, can be presented in a finite form, but, as the results are rather complicated, perhaps it will be better to leave them as they are. It does not matter whether we suppose the solid moving with velocity c through the liquid at rest at infinity, or whether we suppose the solid at rest, and the liquid moving past it with a velocity which at infinity is $-c$. For simplicity, we shall suppose the latter case. First, considering the reflection of the liquid motion only from the semicircle BAB′, let u and v be the velocities of the fluid at any point $P(x, y)$ or (r, θ) of the fluid in the plane of the paper. Let us assume

$$u = \Sigma\frac{a_m}{r^m}\cos m\theta - c\,, \qquad v = \Sigma\frac{a_m}{r^m}\sin m\theta\,,$$

where a_m, &c., are arbitrary constants. It is well known that these expressions satisfy the hydrodynamical equations.

Then the velocity in the directions OP is $u\cos\theta + v\sin\theta$, and

$$u\cos\theta + v\sin\theta = \Sigma\frac{a_m}{r^m}\cos(m-1)\theta - c\cos\theta\,.$$

Now expand $\cos\theta$ by Fourier's theorem in a series of cosines of even multiples of θ between the limits 0 and $\frac{1}{2}\pi$ of θ, which expansion we observe will be true even *at* both limits. Then

$$u\cos\theta + v\sin\theta = \Sigma\frac{a_m}{r^m}\cos(m-1)\theta - \frac{2c}{\pi} + \frac{4c}{\pi}\sum_{n=1}^{n=\infty}\cos 2n\theta\,.\,\frac{\cos n\pi}{4n^2-1}\,.$$

Now determine a_m, &c., by the condition that $u\cos\theta + v\sin\theta$

shall vanish when $r=a$; we thus get $m=2n+1$ and the following series of equations :—

$$\frac{a_1}{a}-\frac{2c}{\pi}=0\,,\qquad \frac{a_5}{a^5}+\frac{4c}{\pi}\frac{1}{3.5}=0\,,$$

$$\frac{a_3}{a^3}-\frac{4c}{\pi}\frac{1}{1.3}=0\,,\qquad \frac{a_7}{a^7}-\frac{4c}{\pi}\frac{1}{5.7}=0\,,\ \&c.$$

We thus get $u+c=$

$$\frac{2c}{\pi}\left\{\frac{a}{r}\cos\theta+\frac{2}{1.3}\frac{a^3}{r^3}\cos 3\theta-\frac{2}{3.5}\frac{a^5}{r^5}\cos 5\theta+\frac{2}{5.7}\frac{a^7}{r^7}\cos 7\theta-\&c.\right\}$$

$$v=\frac{2c}{\pi}\left\{\frac{a}{r}\sin\theta+\frac{2}{1.3}\frac{a^3}{r^3}\sin 3\theta-\frac{2}{3.5}\frac{a^5}{r^5}\sin 5\theta\right.$$

$$\left.+\frac{2}{5.7}\frac{a^7}{r^7}\sin 7\theta-\&c.\right\}.$$

It is well known that if we transform from polar to rectangular co-ordinates, the above equations can be expressed in the form

$$u=f'(x+iy)+f(x-iy),$$

$$v=i\{f'(x+iy)-f'(x-iy)\},$$

and when presented in this form we know that the equation of the stream lines is

$$f(x+iy)-f(x-iy)=\text{constant}.$$

Determining in the particular case considered the form of the function f, and then transferring back again to polar co-ordinates, we find for the equation of the stream lines

$$\theta-\frac{\pi}{2}\frac{r}{a}\sin\theta+\frac{2}{1.2.3}\frac{a^2}{r^2}\sin 2\theta$$

$$-\frac{2}{3.4.5}\frac{a^4}{r^4}\sin 4\theta+\frac{2}{5.6.7}\frac{a^6}{r^6}\sin 6\theta-\&c.=\text{constant}.$$

For the particular stream line which passes through the point B, the above must be satisfied by $r=a$, $\theta=\frac{1}{2}\pi$, and in this case the constant $=0$, and we get equation (1), which is strictly a stream line, but which may evidently be considered as a material curve joined to AB at B. It is evident that CE is an asymptote to this

curve, where $OC = 2a$, touching the curve at the point $r = \infty$, $\theta = \pi$. We are concerned with values of θ only from 0 to π. We can readily show that the curve BD touches the circle at B. For in (1) putting $a + \delta r$ for r, and $\frac{1}{2}\pi + \delta\theta$ for θ, we get

$$\delta\theta - \frac{\pi}{2a}\delta r - 2\delta\theta\left(\frac{1}{1.3} + \frac{1}{3.5} + \frac{1}{5.7} + \&c.\right) = 0,$$

or $\delta r = 0$. If we retained terms involving $\delta\theta^2$, we could readily prove that a is the radius of curvature at B, and that C is the centre of curvature, so that there must be a point of contrary flexure between B and D. As before remarked, DBAx is a stream line. Thus, as we pass along DB, (1) is its equation. When we get on to the circle BA, we *should* get, putting $r = a$,

$$\theta - \tfrac{1}{2}\pi\sin\theta + \frac{2}{1.2.3}\sin 2\theta - \frac{2}{3.4.5}\sin 4\theta + \frac{2}{5.6.7}\sin 6\theta - \&c. = 0,$$

and this *should* be true from $\theta = 0$ to $\theta = \frac{1}{2}\pi$. But if we were to expand $\theta - \frac{1}{2}\pi\sin\theta$ by Fourier's theorem in a series of sines of even multiples of θ between the limits 0 and $\frac{1}{2}\pi$ of θ, we should get exactly the above series, which verifies the work, and we may observe that the expansion would be true even *at* the limits, for $\theta - \frac{1}{2}\pi\sin\theta$ vanishes when $\theta = 0$ and $\theta = \frac{1}{2}\pi$.

It will be found on examination that if AB, instead of being a quadrant, were any part of the semi-circumference, a similar proposition would hold good.

It can also be shown that if AB, instead of being a quadrant of a circle, were a quadrant of an *ellipse*, a like proposition could be established. I will give a slight sketch of the proof. Suppose AB to be part of an ellipse, and suppose it to be moving parallel to OA with a velocity V. Let u, v be the resulting absolute velocities of the fluid at any point (x, y). Then we may take

$$u = \frac{d\phi}{dx}, \qquad v = \frac{d\phi}{dy}, \qquad \frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} = 0 \quad . \quad . \quad . \quad . \quad (2)$$

Now put $x = c\cosh\beta\cos\alpha$, $y = c\sinh\beta\sin\alpha$,
where c is a constant, sinh and cosh are the hyperbolic sine and cosine, and α, β are new so-called curvilinear co-ordinates. Then (2) becomes

$$\frac{d^2\phi}{d\alpha^2} + \frac{d^2\phi}{d\beta^2} = 0.$$

Assume for a solution of this equation

$$\frac{d\phi}{d\alpha} = \Sigma a_m \epsilon^{-m\beta} \sin m\alpha, \qquad \frac{d\phi}{d\beta} = \Sigma a_m \epsilon^{-m\beta} \cos m\alpha + \mathrm{C},$$

where C is a constant, and a_m, &c. are constants to be determined. We may observe that the velocity at infinity normal to the ellipse $\beta = \text{const.}$ is $d\phi/\mathrm{P}d\beta$ where $\mathrm{P}^2 = \frac{1}{2}c^2(\cosh 2\beta - \cos 2\alpha)$, so that whether C is zero or finite, with the above assumption, the whole velocity at infinity is zero. Now suppose $\beta = \beta_1$ gives the solid cylinder, then it will be found that the condition for reflection of the fluid motion at $\beta = \beta_1$ is

$$\Sigma a_m \epsilon^{-m\beta_1} \cos m\alpha + \mathrm{C} = \mathrm{V}c \sinh \beta_1 \cos \alpha \,. \quad . \quad . \quad . \quad . \quad (3)$$

If the ellipse is complete, (3) must hold for all values of α from 0 to π, and then we shall find that we must have $\mathrm{C} = 0$ and $m = 0$. This leads to the solution given in Art. 88 (d) of Lamb's *Motion of Fluids.* But if the reflection takes place at only a quadrant of the ellipse, (3) admits of another solution. Expand in (3) $\cos \alpha$ by Fourier's Theorem in a series of cosines of even multiples of α from $\alpha = 0$ to $\alpha = \frac{1}{2}\pi$, and we get

$$\Sigma a_m \epsilon^{-m\beta_1} \cos m\alpha + \mathrm{C} = \frac{2}{\pi} \mathrm{V}c \sinh \beta_1 \left\{ 1 - \Sigma_1^\infty \cos 2n\alpha \frac{2 \cos \pi n}{4n^2 - 1} \right\}.$$

Now let $\qquad \mathrm{C} = \frac{2}{\pi} \mathrm{V}c \sinh \beta_1, \qquad m = 2n,$

and

$$a_{2n} \epsilon^{-2n\beta_1} = -\frac{4}{\pi} \mathrm{V}c \sinh \beta_1 \frac{\cos n\pi}{4n^2 - 1},$$

and we have a solution exactly analogous to the case of the circle. It will be found also that instead of taking a quadrant of an ellipse, we might take AB to be any arc and still get a similar proposition.

We will next take a case of motion in three dimensions. We will suppose a solid hemisphere BAB′ of radius a moving through an infinite mass of liquid in direction OA, with a velocity which at the instant considered is V, the velocity of the liquid at infinity being zero. The motion of the liquid is supposed to be in planes through OA, and to be symmetrical round it. We will suppose a

velocity $-V$ in direction Ox to be impressed upon the solid and fluid, so as to bring the former to rest, and let ϕ be the velocity potential of the fluid motion *relative* to the solid at any point P whose polar coordinates are r, θ. Then ϕ satisfies Laplace's equation $\Delta^2\phi = 0$, which changed to polar coordinates becomes

$$\frac{d^2\phi}{dr^2} + \frac{2}{r}\frac{d\phi}{dr} + \frac{1}{r^2 \sin\theta}\frac{d}{d\theta}\left(\sin\theta\frac{d\phi}{d\theta}\right) = 0 .$$

We may take for a solution of this equation

$$\phi = -Vr\cos\theta + a_0\frac{a}{r} + a_2 P_2 \frac{a_3}{r^3} + a_4 P_4 \frac{a_5}{r^5} + \&c.,$$

where P_2, P_4, &c., are Legendre's coefficients of even order, and a_0, a_2, &c., are constants to be determined. The first term is introduced because the velocity at infinity parallel to OA must be $-V$. We can determine a_0, a_2, &c., by the condition that $d\phi/dr$ must vanish, when $r = a$.

This gives us

$$V\cos\theta = -\frac{1}{a}(a_0 + 3a_2P_2 + 5a_4P_4 + \&c.) \quad . \qquad (4)$$

This must be true from $\theta = 0$ to $\theta = \frac{1}{2}\pi$.

Now $\cos\theta$ can be expanded in a series of Legendre's coefficients of even orders between the above limits. Putting $\cos\theta = x$,

$$\int_0 P_m P_m dx = \frac{1}{2m+1} \text{ and } \int_0^1 P_m P_n dx = 0 ,$$

which last is true if both m and n are even. We then get

$$a_0 = -\tfrac{1}{2}aV , \qquad a_2 = -\tfrac{5}{24}aV ,$$

and generally

$$a_r = (-1)^{\frac{1}{2}r} aV \frac{2r+1}{r+1} \times \frac{3.5\ldots(r-3)}{2.4\ldots(r+2)}$$

It will be found that the differential equation to the stream lines can be expressed in the form (putting $\frac{a}{r} = \rho$ for shortness),

$$(1-x^2)\left\{-V\frac{a}{\rho^3} - a_2\rho\frac{dP_2}{dx} + a_4\rho^3\frac{dP_4}{dx} + \&c.\right\}d\rho$$

$$-\left\{\frac{1}{\rho^2}Vax + a_0 + 3a_2P_2\rho^2 + 5a_4P_4\rho^4 + \&c.\right\}dx = 0 . \quad . \quad (5)$$

Remembering the known relation

$$\frac{d}{dx}\left\{ 1-x^2)\frac{dP_n}{dx}\right\} + n(n+1)P_n = 0,$$

and examining the general term of (5), we see that (5) is an exact differential. Therefore integrating we shall get for the equation of the stream lines

$$(1-x^2)\left\{\frac{aV}{2\rho_2} + a_2\frac{\rho^2}{2}\frac{dP_2}{dx} + a_4\frac{\rho^4}{4}\frac{dP_4}{dx} + \&c\cdot\right\} - a_0x = C$$

For the particular stream line, which passes through the point B, the above must be satisfied by $\rho=1$, $\theta=\frac{1}{2}\pi$ or $x=0$, and therefore $C=\frac{aV}{2}$, so that its equation is

$$(1-x^2)\left\{\frac{aV}{2\rho^2} + a_2\frac{\rho^2}{2}\frac{dP_2}{dx} + a_4\frac{\rho^4}{4}\frac{dP_4}{dx} + \&c.\right\} - a_0x = \frac{aV}{2}\ . \quad (6)$$

For distant points ρ is small, therefore for such points neglecting all the positive powers of ρ in the above, we can readily prove that $y=\sqrt{(2)}a$ is an asymptote, and that the curve lies below it. It is interesting to see whether at B the curve goes up or down. To this end, in (6) put $a+\delta r$ for r and $\frac{1}{2}\pi+\delta\theta$ for θ, expand and retain not beyond the first power of δr and the second power of $\delta\theta$. It will be found that we shall get

$$2\delta r - a\delta\theta^2 + a\left[-1+\tfrac{5}{8}+\tfrac{9}{64}+\&c.\right.$$

$$\left.+\frac{1}{n}\frac{4n+1}{2n+1}\frac{3.5\ldots(2n-3)}{2.4\ldots(2n+2)}\frac{3.5\ldots(2n+1)}{2.4\ldots(2n-2)}+\&c.\right]\delta\theta = 0\ . \quad (7)$$

In order to get the terms after the 3rd in this series, we must take n from 3 to infinity. There seems to be no doubt that the value of the series in the square brackets is zero, although it is rather difficult to prove it algebraically, for the expansion (4) is true even *at* the limits. Moreover the expansion (4) is never differentiated, therefore $d\phi/dr$ is zero for $r=a$ even *at* the point B, therefore, as in the case of the circular cylinder, the stream line through B must *touch* the circle at B; assuming therefore that the series in (7) is zero, the remaining terms show us that the curvature at B is upwards, and the radius of curvature equal to a. The same figure therefore as was employed for the circular cylinder will do for this case, provided that we make $OC=\sqrt{(2)}a$ instead of $2a$.

2. Note on Knots on Endless Cords. By A. B. Kempe, Esq. Communicated by Prof. Tait. (Plate I.)

1. Each crossing divides the cord into two loops.

2. Any other crossing either lies on both these loops, say is *linked* to the former crossing, or on one loop only, say is *not linked* to the former crossing.

3. It can be shown without difficulty that if crossing a is linked to crossing b, then crossing b is linked to crossing a; and therefore, also, if crossing a is not linked to crossing b, crossing b is not linked to crossing a.

4. Hence *pairs* of crossings are of two sorts, viz., linked and unlinked.

5. We have the two following fundamental laws :—

(*a*) The number of crossings linked to each crossing is even.

(*b*) If two crossings are not linked to each other, the number of crossings linked to both is even.

6. We may represent a knot diagrammatically thus :—

Represent the crossings by small circles or *nuclei*.

Join pairs of nuclei which represent pairs of linked crossings by lines or *links*.

No lines are to be drawn in the case of unlinked pairs of crossings.

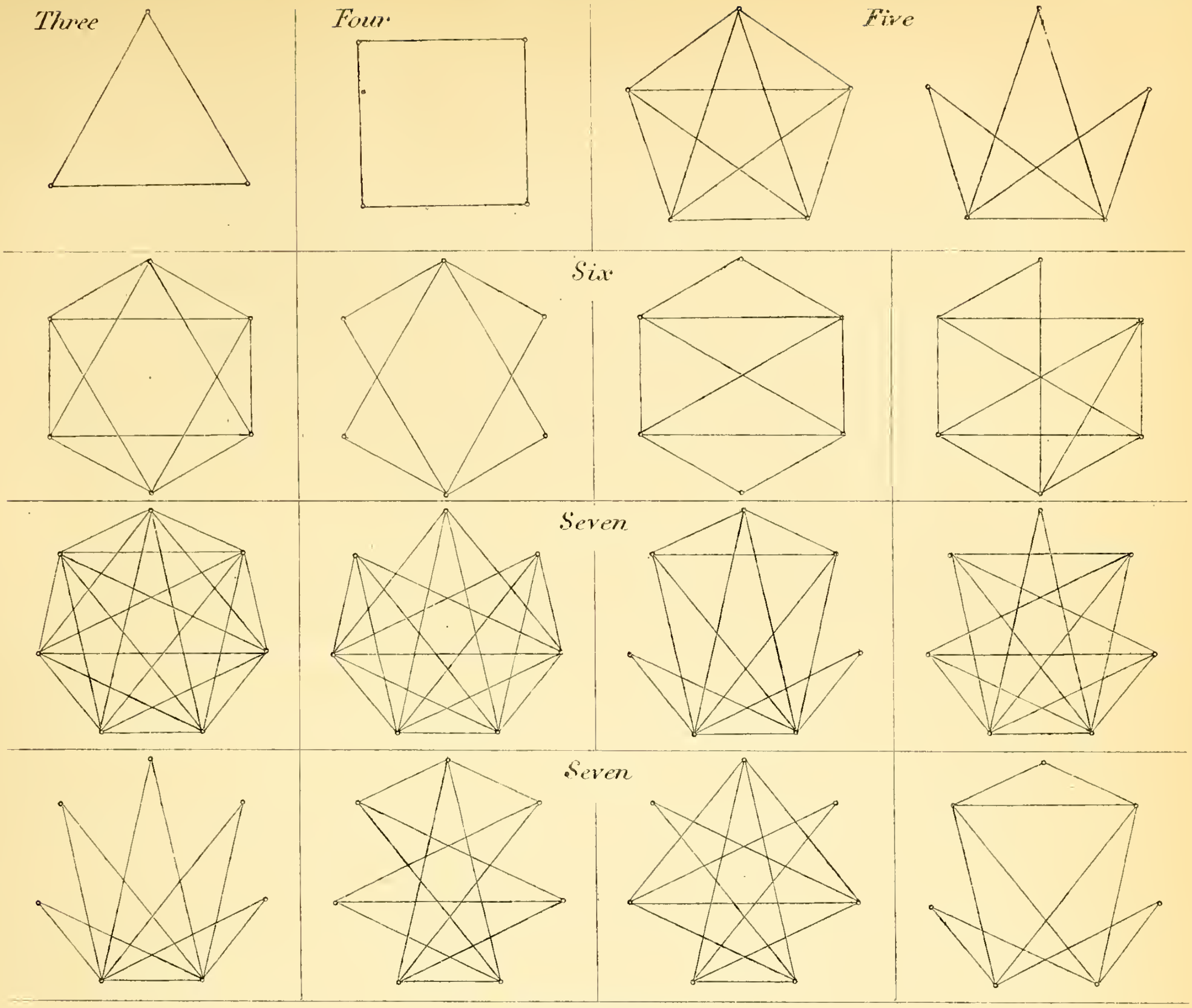
7. In these diagrams, in conformity with sec. 5, the number of links proceeding from a nucleus must be even, and if two nuclei are not joined by a link, the number of nuclei joined by links to both must be even.

8. This mode of representing knots has the advantage of indicating the degree of complexity of the various knots. Thus, the diagram representing two distinct knots on the same string will consist of two entirely detached portions, and nugatory crossings will be represented by nuclei having no links proceeding from them.

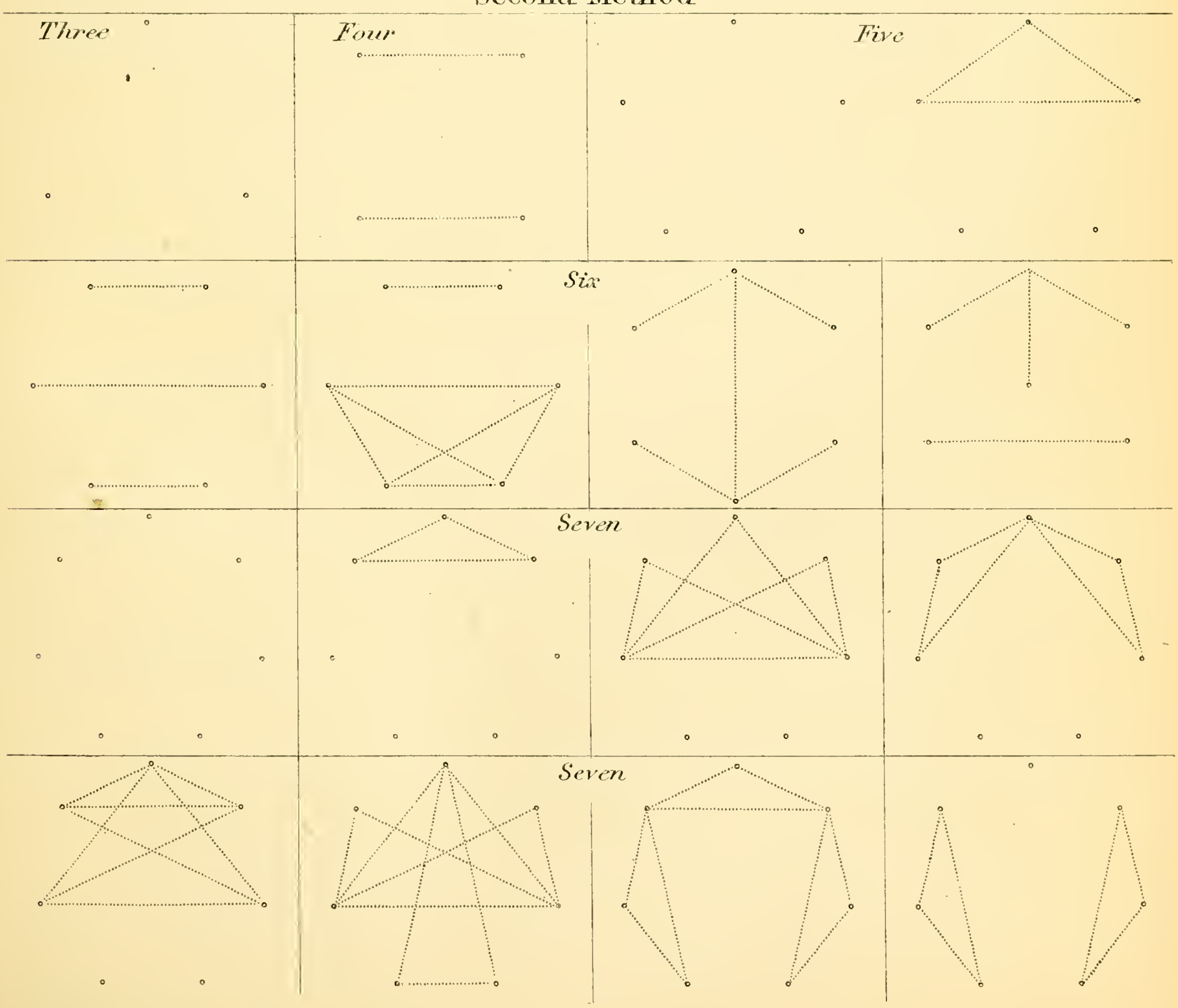
9. In the plate the various knots of *three*, *four*, *five*, *six*, and *seven* crossings are indicated in outline on this plan.

10. It will occasionally be convenient to represent pairs of crossings which are *unlinked* by pairs of nuclei joined by a dotted line or link, pairs of crossings which are linked not being joined at all.

First Method



Second Method



The advantage of this mode of representation is that it involves a smaller number of lines than the former mode, and thus the diagrams are simpler and clearer.

11. In the lower half of the plate the same knots are indicated again, but on this new plan. [By an oversight the last, and the last but two, of the seven-folds have been interchanged in this part of the diagram.]

3. On the Ring-Waves produced by throwing a Stone into Water. By Sir W. Thomson.

(Printed in full in the *Philosophical Magazine*, Jan. 1887.)

4. On the Waves produced by a Ship advancing uniformly into Smooth Water. By the Same.

(*Phil. Mag.*, Jan. 1887.)

5. Expansion of Functions in terms of Linear, Cylindric, Spherical, &c., Functions. By P. Alexander, M.A. Communicated by Dr T. Muir.

(This paper is printed in the *Transactions.*)

6. On Even Distribution of Points in Space. By Prof. Tait.

(*Abstract.*)

The question raised is very closely connected with § 6 of my paper on the *Kinetic Theory of Gases* (*Trans. R.S.E.* xxxiii. 71). The result arrived at is that, when (in the thin layer) some particles prevent others from doing their full duty, the formula should be

$$\varepsilon^{-n_1 \pi s^2 \delta x}, \text{ instead of } 1 - n_1 \pi s^2 \delta x$$

as given in the text.

Friday, 7th January 1887.

SIR DOUGLAS MACLAGAN, Vice-President, in the Chair.

At the request of the Council, Mr J. J. Coleman gave an Address on Processes of Refrigeration.

The following Papers were laid on the Table:—

1. On the Front and Rear of a Free Procession of Waves in Deep Water. By Sir William Thomson, F.R.S.

PRELIMINARY.

General Problem of Deep-Sea Wave-Motion in two dimensions. (Infinitesimal Motion.)

Taking x horizontal, and y vertically downwards; let $(x+\xi, y+\eta)$ be, at time t, the position of the particle whose position at time 0 is (x, y); let Φ denote the velocity-potential at (x, y, t); and let P denote its time-integral, $\int_0^t dt\Phi$. We have

$$\xi = \int_0^t dt\frac{d\Phi}{dx} = \frac{dP}{dx}\,; \quad \text{and} \quad \eta = \int_0^t dt\frac{d\Phi}{dy} = \frac{dP}{dy} \quad . \quad . \quad . \quad (1).$$

Let p be the pressure at $(x+\xi, y+\eta)$. (The motion being infinitesimal,) we have

$$p = C + g(y+\eta) - \frac{d\Phi}{dt} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2),$$

or, in virtue of (1),

$$p = C + gy + g\frac{dP}{dy} - \frac{d^2P}{dt^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3).$$

The kinematical conditions are, the equation of continuity,

$$\frac{d^2P}{dx^2} + \frac{d^2P}{dy^2} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4);$$

and the boundary equation, in two parts—one relating to the upper

surface, the other to the bottom. The latter, for our present case of infinitely deep water, is simply

$$\mathrm{P}=0 \text{ when } y=\infty \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5).$$

To find the former, or upper-surface kinematical equation, at time t, let it be $y=0$ at time 0, and let $\mathfrak{h}$ be the height at time t above the level $y=0$, of the upper-surface particle whose coordinates at time 0 are $(x, 0)$. Remembering that y positive was taken as *downwards*, we have by (1),

$$\mathfrak{h}=-\left(\frac{d\mathrm{P}}{dy}\right)_{y=0} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6).$$

The most general upper-surface dynamical condition which can be imposed is

$$p_{(y=0)}=f(x, t) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7),$$

where f denotes an arbitrary function of the two independent variables.

Suppose now the water to be at rest at time 0. It is clear from dynamical considerations that the solution of (4), subject to the conditions (5), (7), (3), is fully determinate: and when it is found, (1) gives the position at time t of the fluid-particle which at time 0 was in any position (x, y); and so completes the solution of the problem.

The particular solution which we are now going to work out to represent a uniform procession of waves commencing at time 0, and produced and maintained by the application of changing pressure to the surface in the neighbourhood of the zero of x, must, as its appropriate form of (7), fulfil the condition

$$p_{(y=0)}=\mathfrak{F}(x) \sin \omega t+\mathrm{F}(x) \cos \omega t \quad . \quad . \quad . \quad . \quad (8),$$

where $\mathfrak{F}(x)$ and $\mathrm{F}(x)$ denote functions which vanish for all large positive or negative values of x.

If we wish to make only a single procession, in the direction of x positive for example, we may take

$$\mathfrak{F}(x)=\mathrm{F}(x-\tfrac{1}{2} g \pi / \omega^{2}) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9).$$

A perfectly general formula is easily (by the Fourier-Poisson-Cauchy method) written down to express the value of P; and so,

by (1) and (6), the complete solution of the problem: for $\mathfrak{F}$ and F any given arbitrary functions.

It is obvious that, so far as $\mathfrak{h}$ is concerned, the general solution for x any considerable multiple of $\pm l$, and exceeding $\pm l$ by not less than two or three times the wave-length, $2\pi g/\omega^2$, must, for values of t great enough to have let the front of the procession pass the place x, be

$$\left.\begin{aligned}
&= \mathfrak{A} \sin\left[\omega t - \frac{\omega^2}{g}(x-f)\right] + \mathrm{A}\cos\left[\omega t - \frac{\omega^2}{g}(-\mathrm{f})\right] \\
&\qquad\qquad\qquad\qquad \text{for } x \text{ positive,} \\
&\text{and} \\
\mathfrak{h} &= \mathfrak{A}\sin\left[\omega t - \frac{\omega^2}{g}(-x+f)\right] - \mathrm{A}\cos\left[\omega t - \frac{\omega^2}{g}(-x+\mathrm{f})\right] \\
&\qquad\qquad\qquad\qquad \text{for } x \text{ negative,}
\end{aligned}\right\} (10);$$

where $\mathfrak{A}$ and f denote quantities calculable from the form of $\mathfrak{F}$; and A and f similarly from F. Further, it is obvious that the front of each procession will, for any value of t not less than several times the period and not less than several times the time one of the wave-crests takes to travel through a space equal to l, be independent of the particular forms of $\mathfrak{F}$ and F. From the theory of Stokes, Osborne Reynolds, and Rayleigh, we know that it advances at half the speed of a wave-crest; but their theory, so far as hitherto developed, does not teach us the law according to which the front, as it advances, becomes longer and longer in proportion to $\sqrt{t}$, nor even the fact that it does become longer and longer. All the details of this interesting question are exquisitely given in what follows: having been found with great ease for the particular case,

$$\mathrm{F}(x) = 0, \text{ and } \mathfrak{F}(x) = \left\{\frac{(x^2+b^2)^{\frac{1}{2}}+b}{x^2+b^2}\right\}^{\frac{1}{2}} \quad . \quad . \quad . \quad (11),$$

where b denotes a length of any magnitude, which we shall take to be very small in comparison with $2\pi g/\omega^2$, the wave-length. We shall in fact find that

$$p_{(y=0)} = \mathrm{C} + \left\{\frac{(x^2+b^2)^{\frac{1}{2}}+b}{x^2+b^2}\right\}^{\frac{1}{2}} \sin\omega t \quad . \quad . \quad . \quad (12),$$

in the particular processional case of the general equations (1) . . . (6), which we now go on to work out.

Remembering Cauchy and Poisson's discovery that every surface of particles which are in a horizontal plane when undisturbed fulfils the condition of a free upper-surface (so that if all the water above it were annulled the motion of the water remaining below it would be undisturbed), in the case of free waves of infinitely deep water; we see that when $p_{(y=0)} = \text{const.}$, we have also, in our notation, $p = \text{const.}$, for every constant value of y. Hence, looking to (3) above, we must find, in the case of free waves,

$$\frac{gd\mathrm{P}}{dy} = \frac{d^2\mathrm{P}}{dt^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13);$$

for every value of y, and not only at the upper surface $y=0$. Thanking Cauchy and Poisson for this as a suggestion, but not assuming it without the proof of it which we immediately find; and borrowing now from Fourier* his celebrated "instantaneous plane-source" † solution of his equation $\frac{dv}{dt} = k\frac{d^2v}{dx^2}$ for thermal conduction, assume, as an imaginary type-solution of (4) and (13) for free waves,

$$\frac{1}{(b+y+\iota x)^{\frac{1}{2}}}\epsilon^{\frac{-gt^2}{4(b+y+\iota x)}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14),$$

where ι denotes $\sqrt{-1}$. Whence, as a real solution by adding the values of (14) for ι and $-\iota$, and dividing by $\sqrt{2}$,

$$\left.\begin{array}{l} \phi(t) = \dfrac{1}{r}\left\{ (r+y+b)^{\frac{1}{2}} \cos\dfrac{gt^2x}{4r^2} + (r-y-b)^{\frac{1}{2}} \sin\dfrac{gt^2x}{4r^2} \right\}\epsilon^{\frac{-gt^2(y+b)}{4r^2}} \\ \text{where} \qquad\qquad r = [(y+b)^2 + x^2]^{\frac{1}{2}} \end{array}\right\} (15).$$

Curves representing calculated results of this solution for free waves were shown at the meeting of the British Association (Section A) at Birmingham in September, and at the last meeting (December 20) of the Royal Society of Edinburgh. To build up of it a solution for a uniformly maintained procession of waves (a double procession it shall be, of equal and similar waves travelling in the two directions from $x=0$) take

$$\mathrm{P}(t) = \int_0^t dt\phi(t) \quad . \quad . \quad . \quad . \quad . \quad (16);$$

* *Théorie Analytique de la Chaleur.*

† Sir W. Thomson's Collected Papers, vol. ii. p. 46.

and

$$\mathrm{P} = -\int_0^t dt \sin \omega t' \mathbf{P}(t-t') = -\int_0^t dt \sin \omega(t-t')\mathbf{P}(t') \quad . \quad (17).$$

Since $\phi\ (t)$, as we have seen, satisfies (13), $\mathbf{P}\ (t)$ must satisfy it also. Hence

$$g\frac{d\mathbf{P}(t)}{dy} = \frac{d^2\mathbf{P}(t)}{dt^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (18),$$

for all values of x, y, and t. Now by differentiation of (17) we find, because $\mathbf{P}(0)=0$, and by (16),

$$\frac{d\mathrm{P}}{dt} = -\int_0^t dt' \sin \omega t' \frac{d}{dt}\mathbf{P}(t-t') = -\int_0^t dt' \sin \omega t' \phi(t-t') \quad . \quad (19);$$

and differentiating this, we find, because $\phi(0) = (r+y+b)^{\frac{1}{2}}r^{-1}$

$$\frac{d^2\mathrm{P}}{dt^2} = -\frac{(r+y+b)^{\frac{1}{2}}}{r}\sin \omega t - \int_0^t dt' \sin \omega t' \frac{d}{dt}\phi(t-t')$$

$$= -\frac{(r+y+b)^{\frac{1}{2}}}{r}\sin \omega t - \int_0^t dt' \sin \omega(t-t')\frac{d^2\mathbf{P}(t')}{dt'^2} \quad . \quad (20).$$

From this and the second form of (17) we find

$$g\frac{d\mathrm{P}}{dy} - \frac{d^2\mathrm{P}}{dt^2} = \frac{(r+y+b)^{\frac{1}{2}}}{r}\sin \omega t$$

$$-\int_0^t dt \sin \omega(t-t')\left[g\frac{d\mathbf{P}(t')}{dy} - \frac{d^2\mathbf{P}(t')}{dt'^2}\right] \quad . \quad . \quad . \quad (21);$$

whence, by (18)

$$g\frac{d\mathrm{P}}{dy} - \frac{d^2\mathrm{P}}{dt^2} = \frac{(r+y+b)^{\frac{1}{2}}}{r}\sin \omega t \quad . \quad . \quad . \quad (22);$$

and therefore finally, by (3) above, we have, for the surface pressure,

$$p_{(y=0)} = \mathrm{C} + \left\{\frac{(x^2+b^2)^{\frac{1}{2}}+b}{x^2+b^2}\right\}^{\frac{1}{2}}\sin \omega t \quad . \quad (23),$$

as promised in (12) above.

To work out our solution, remember that $d\mathrm{P}/dt$ is the velocity-potential of the motion; and calling this Φ, we find, by (19),

$$\Phi = -\int_0^t dt' \sin \omega(t-t')\phi(t) \quad . \quad . \quad . \quad . \quad (24);$$

and by (22), (3), and (2) we find

$$\eta = \frac{1}{g}\left\{\frac{d\Phi}{dt} + \frac{(r+y+b)^{\frac{1}{2}}}{r}\sin\omega t\right\}. \quad . \quad . \quad . \quad (25).$$

What we chiefly want to know is the surface-value of η, which we have denoted by $-\mathfrak{h}$; and we shall work this out for the case $b=0$. But it is to be remarked that the assumption of $b=0$ does not diminish the generality of our problem, because the motion at at any depth, c, below the upper surface with $b=0$, is the same as the motion at the surface, with $b=c$.

Put now $b=0$ and $y=0$ in (15): we find

$$\phi(t) = x^{-\frac{1}{2}}\left(\cos\frac{gt^2}{4x} + \sin\frac{gt^2}{4x}\right) = \sqrt{\frac{2}{x}}\sin\left(\frac{gt^2}{4x} + \frac{\pi}{4}\right) \quad . \quad (26).$$

Using this in (24), and putting $\sigma^2 = gt'^2/4x$, we find

$$\Phi = -2\sqrt{\frac{2}{g}}\int_0^{\sqrt{\frac{g}{4x}}} d\sigma \sin\left(\omega t - 2\omega\sqrt{\frac{x}{g}}\sigma\right)\sin\left(\sigma^2 + \frac{4}{\pi}\right) \quad . \quad (27),$$

$$= \sqrt{\frac{2}{g}}\int_0^{t\sqrt{\frac{g}{4x}}} d\sigma\left\{\cos\left[\left(\sigma - \omega\sqrt{\frac{x}{g}}\right)^2 - \omega^2\frac{x}{g} + \omega t + \frac{\pi}{4}\right]\right.$$

$$\left. - \cos\left[\left(\sigma + \omega\sqrt{\frac{x}{g}}\right)^2 - \omega^2\frac{x}{g} - \omega t + \frac{\pi}{4}\right]\right\} \quad (28).$$

Using now the following notation,

$$say\,\theta = \int_0^\theta d\theta\sin\theta^2; \qquad cay\,\theta = \int_0^\theta d\theta\cos\theta^2 \quad . \quad (29),$$

for two integrals which have been tabulated by Airy * through the range from 0 to $5{\cdot}5\sqrt{\frac{\omega}{2}}$ we reduce (28) to

$$\sqrt{\frac{g}{2}}\Phi = \left[cay\left(t\sqrt{\frac{g}{4x}} - \omega\sqrt{\frac{x}{g}}\right) + cay\left(\omega\sqrt{\frac{x}{g}}\right)\right]\cos\left(\frac{\omega^2}{x}x - \omega t - \frac{\pi}{4}\right)$$

$$+\left[say\left(t\sqrt{\frac{g}{4x}} - \omega\sqrt{\frac{x}{g}}\right) + say\left(\omega\sqrt{\frac{x}{g}}\right)\right]\sin\left(\frac{\omega^2}{g}x - \omega t - \frac{\pi}{4}\right)$$

$$-\left[cay\left(t\sqrt{\frac{g}{4x}} + \omega\sqrt{\frac{x}{g}}\right) - cay\left(\omega\sqrt{\frac{x}{g}}\right)\right]\cos\left(\frac{\omega^2}{g}x + \omega t - \frac{\omega}{4}\right)$$

$$-\left[say\left(t\sqrt{\frac{g}{4x}} + \omega\sqrt{\frac{x}{g}}\right) - say\left(\omega\sqrt{\frac{x}{g}}\right)\right]\sin\left(\frac{\omega^2}{g}x + \omega t - \frac{\omega}{4}\right). \quad (30).$$

* "Tracts" (Undulatory Theory of Optics, last page).

The interpretation of this is eased by putting it into the form

$$\Phi = \sqrt{\frac{2}{g}}\left\{ Q \cos\left(\frac{\omega^2}{g}x - \omega t - e\right) - R \cos\left(\frac{\omega^2}{g}x + \omega t - f\right)\right\}. \quad (31).$$

where

$$Q = \left\{\left[cay\left(t\sqrt{\frac{g}{4x}} - \omega\sqrt{\frac{x}{g}}\right) + cay\left(\omega\sqrt{\frac{x}{g}}\right)\right]^2 \right.$$
$$\left. + \left[say\left(t\sqrt{\frac{g}{4x}} - \omega\sqrt{\frac{x}{g}}\right) + say\left(\omega\sqrt{\frac{x}{g}}\right)\right]^2\right\}^{\frac{1}{2}}. \quad (32);$$

$$e = \tan^{-1}\frac{say\left(t\sqrt{\frac{g}{4x}} - \omega\sqrt{\frac{x}{g}}\right) + say\left(\omega\sqrt{\frac{x}{g}}\right)}{cay\left(t\sqrt{\frac{g}{4x}} - \omega\sqrt{\frac{x}{g}}\right) + cay\left(\omega\sqrt{\frac{x}{g}}\right)} - \frac{\mu}{4} \quad . \quad . \quad . \quad (33);$$

$$R = \sqrt{\frac{2}{g}}\left\{\left[cay\left(t\sqrt{\frac{g}{4x}} + \omega\sqrt{\frac{x}{g}}\right) - cay\left(\omega\sqrt{\frac{x}{g}}\right)\right]^2 \right.$$
$$\left. + \left[say\left(t\sqrt{\frac{g}{4x}} + \omega\sqrt{\frac{x}{g}}\right) - say\left(\omega\sqrt{\frac{x}{g}}\right)\right]^2\right\}^{\frac{1}{2}} \quad . \quad (34);$$

and

$$f = \tan^{-1}\frac{say\left(t\sqrt{\frac{g}{4x}} + \omega\sqrt{\frac{x}{g}}\right) - say\left(\omega\sqrt{\frac{x}{g}}\right)}{cay\left(t\sqrt{\frac{g}{4x}} + \omega\sqrt{\frac{x}{g}}\right) - cay\left(\omega\sqrt{\frac{x}{g}}\right)} - \frac{\pi}{4} \quad . \quad . \quad (35).$$

Now, remembering that $cay\ (\infty) = say\ (\infty) = \sqrt{\frac{\pi}{8}}$, we see that if $\omega\sqrt{\frac{x}{g}}$ is large, and $t\sqrt{\frac{g}{4x}} - \omega\sqrt{\frac{x}{g}}$ is large positive, we have

$$R \doteq 0, \quad Q \doteq \sqrt{\pi}, \quad e \doteq 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (36);$$

and therefore

$$\phi \doteq \sqrt{\frac{2\pi}{g}}\cos\left(\frac{\omega^2 x}{g} - \omega t\right) \quad . \quad . \quad . \quad . \quad . \quad (37)$$

whence, and by (25) with $b = 0$,

$$\mathfrak{h} \doteq -\frac{1}{g}\omega\sqrt{\frac{2\pi}{g}}\left[\sin\left(\frac{\omega^2 x}{g} - \omega t\right) - \frac{1}{\omega}\sqrt{\frac{g}{2\pi x}}\sin\omega t\right] \quad . \quad (38);$$

or, since $\omega\sqrt{(x/g)}$ is very large,

$$\mathfrak{h} \doteq -\frac{\omega}{g}\sqrt{\frac{2\pi}{g}}\sin\left(\frac{\omega^2 x}{g} - \omega t\right) \quad . \quad . \quad . \quad . \quad . \quad (39).$$

This represents a uniform procession of free waves, of which the wave-length, λ, and the wave-velocity, U, are as follows :—

$$\lambda = 2\pi g/\omega^2, \quad U = g/\omega \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (40).$$

To explain the meaning of "very large" as we have just now used it, let

$$x = n\lambda, \text{ which makes } \omega\sqrt{\frac{x}{g}} = \sqrt{2\pi n}, \text{ and } \frac{1}{\omega}\sqrt{\frac{g}{2\pi x}} = 1/4\pi\sqrt{n} \quad (41).$$

Hence the term of (38) omitted in (39) is $1/4\pi\sqrt{n}$ of that retained. And the value of the R, omitted by (36) in (37), is of the order $1/2\sqrt{2n}$ of the Q which is retained, because

$$cay(\infty) - cay(\sqrt{2\pi n}) \doteqdot -\frac{\sin(2\pi n)}{2\sqrt{2\pi n}},$$

and
$$say(\infty) - say(\sqrt{2\pi n}) \doteqdot \frac{\cos(2\pi n)}{2\sqrt{2\pi n}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (42),$$

when n is very large.

In (36) and its consequence (31), we supposed t so large that $t\sqrt{\frac{g}{4x}} - \omega\sqrt{\frac{x}{g}}$ is large positive; let us next suppose t so small that it is large negative; that is to say, let

$$t = 2\omega x/g - m\sqrt{\frac{4x}{g}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (43),$$

where m is a large positive numeric. Thus, remarking that $cay(-\theta) = -cay(\theta)$, and $say(-\theta) = -say(\theta)$, we have, by (43) and (41) in (32),

$$Q = \sqrt{\frac{2}{g}}\{[cay(m) - cay(\sqrt{2\pi n})]^2 + [say(m) - say(\sqrt{2\pi n})]^2\}^{\frac{1}{2}} \quad (44);$$

and therefore, when m and n are each very large, $Q \doteqdot 0$. Because n is large we still, as in (36), have $R \doteqdot 0$; and therefore the motion is approximately zero, at any considerable number, n, of wave-lengths from the origin, so long as m in (43) remains large. As time advances, m decreases to 0, and on to $-\infty$: and, watching at the place $x = n\lambda$, we see wave-motion gradually increasing from nothing, till it becomes the regular procession of waves represented by (39); and continues so unchanged for ever after. When $m = 0$, that is to say, at the time

$$t = 2\omega x/g \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (45),$$

Q has attained half its final value. The point x where this condition is fulfilled at time t may be called the mid-front of the procession. It travels at the velocity $\frac{1}{2}g/\omega$, or half the wave-velocity; which agrees with the result of Stokes.

We may arbitrarily define "the front" as the succession of augmenting waves which pass between the times corresponding to $m = +10$ and $m = -10$ (or any other considerable number instead of 10). Thus the time taken by the front, in passing the place $x = n\lambda$, is $40\omega^{-1}\sqrt{2\pi n}$. The space travelled by the mid-front in this time is $20g\omega^{-1}\sqrt{2\pi n}$, which may, arbitrarily, be defined as the length of the front. It increases in proportion to $\sqrt{n}$; and therefore in proportion to $\sqrt{t}$, as said above. The effect upon phase of the changing waves in the front; due to the fluctuations of e, and to the law of augmentation of Q from zero to its final value; is to be illustrated by calculations and graphic representations, which I hope will be given on a future occasion.

The rear of a wholly free procession of waves may be quite readily studied after the constitution of the front has been fully investigate, by superimposing an annulling surface-pressure upon the originating pressure represented by (12) above, after the originating pressure has been continued so long as to produce a procession of any desired number of waves.

2. Numerical and other Additions to his Paper, read on 6th December 1886, on the Foundations of the Kinetic Theory of Gases. By Professor Tait.

In the case of diffusion, in a long tube of unit section, suppose that we have, at section x of the tube, n_1P_1s and n_2P_2s per cubic unit, with translational speeds α_1 and α_2, respectively. If G_1 be the whole mass of the first gas on the negative side of the section, it is shown that the rate of flow of that gas is

$$\frac{dG_1}{dt} = -P_1\left(n_1\alpha_1 - {}_1\mathfrak{C}_1\frac{dn_1}{dx}\Big/3\right), \text{ \&c.}$$

Obviously

$$\frac{dG_1}{dx} = P_1n_1, \text{ \&c.}$$

The motion of the layer of P_1s at x is (if approximately steady) given by the equation

$$\frac{d}{dx}\left(\frac{P_1 n_1}{h_1}\right) = -\frac{8}{3} n_1 n_2 s^2 \sqrt{\frac{\pi(h_1+h_2)}{h_1 h_2}} \frac{P_1 P_2}{P_1+P_2} (\alpha_1 - \alpha_2),$$

where the right-hand side depends on the collisions between the two kinds of gas in the layer, s being the semi-sum of the diameters. From these we obtain

$$\frac{dG_1}{dt} = \left(\frac{3}{16s^2} \frac{P_1+P_2}{\sqrt{h_1 h_2 (h_1+h_2)}} \frac{1}{p} + \frac{1}{3n}(n_{2\,1}\mathfrak{C}_1 + n_{1\,2}\mathfrak{C}_1)\right) \frac{d^2G_1}{dx^2}.$$

In the special case, when the masses and diameters are equal in the two gases, the diffusion-coefficient (the multiplier of $\frac{d^2G_1}{dx^2}$ above) has the value

$$\left(\frac{3}{4}\sqrt{\frac{\pi}{2}} + \frac{1}{3}C_1\right)\frac{\lambda}{0{\cdot}677\sqrt{h}} = \frac{\lambda}{\sqrt{h}} 1{\cdot}8.$$

It is therefore inversely as the density, and directly as the square root of the absolute temperature. And the case of two infinite vessels, connected by a tube of length l and section S, and containing two gases whose particles have equal masses and diameters, the rate of flow of either is $\frac{S\rho\lambda}{l\sqrt{h}}$ 1·8 in *mass* per unit of time.

Other cases are treated; and among these it is shown that with equal masses, and constant semi-sum of diameters, difference of diameters favours diffusion. The remainder of the paper is devoted to the interdiffusion of two gases whose particles have masses in the special ratio 16 : 1, the case of oxygen and hydrogen. The rate of diffusion (in a tube of unit section and of length l, connecting two infinite vessels filled with the gases (the semi-sum s of the diameters being constant) is given by the expression,

$$\frac{A \,.\, P}{\pi l s^2 \sqrt{h}};$$

where A depends, as follows, on the ratio of the diameters :—

Ratio of Diameters.	A.
0 : 1	3·48
1 : 3	3·31
1 : 1	3·46
3 : 1	3·79
1 : 0	4·26

When the masses are unequal it is shown that the temperature must be kept constant to insure a steady state of diffusion.

3. Intimation of an Improvement in Rankine's Formula for Retaining Walls. Given by Professor Armstrong on behalf of Mr Elliott.

Monday, 17th January 1887.

SHERIFF IRVINE, Vice-President, in the Chair.

The following Communications were read:—

1. The Total Rainfall on the Land of the Globe, and its Relation to the Discharge of Rivers. By J. Murray, Esq., Ph.D., *V.P.*

2. Chemical Affinity and Solution. By W. Durham, Esq.

In continuation of my inquiry into the evidence which thermochemistry gives of the truth of my theory of chemical affinity and solution, I would direct attention to the sulphates.

In the first instance, consider the well-known definite compound sulphuric acid H_2SO_4. The heat which is evolved on building up this acid from its elements is 192920 units. When it is dissolved in a large quantity of water the mixture evolves 17850 units, making in all 210770 units. Now, consider how this is made up. First, we have H_2O with a combination heat of 68360 units, then SO_3 with 103240 units. Further, we know that S combines with H_2, evolving 4740 units of heat, and let us assume, according to my theory, that all the affinity of the S for O is not exhausted on the combination SO_3, but that part remains in a less intense form which can act on the O of the water, and in conjunction with H_2 can evolve 34413 units more, this being its average action on the three atoms of O already combined. Add these numbers together,

and we have almost exactly the heat of formation and solution of H_2SO_4. Thus—

$[H^2O]$	$= 68360$	$[H^2, S, O^4]$	$= 192920$
$[S, O^3]$	$= 103240$	$[H_2SO_4, Aq]$	$= 17850$
$[S, H^2]$	$= 4740$		
$[SO^3, O]$	$= 34413$		
	210753		210770

Now, of course, I have assumed that S will develop 34413 units in addition after combining with three atoms of O. Let us see, however, how this way of looking at the combination helps us as we go on. Take next a very different sulphate, viz., $BaSO_4$. In building this salt up from its elements, 338070 units of heat are evolved. Analysing this in the same manner as in the case of H_2SO_4 we have $[Ba, O] = 124240$ units of heat, $[S, O^3] = 103240$ units, together equal to 227480 units, leaving 110590 units to be evolved on combination of BaO with SO_3. Whence do these 110590 units come? We have the answer at once when we know that $[Ba, S] =$ about 109600. It is evident the S acts upon the Ba with as much energy as if BaS were actually formed, and this is the cause of the combination. In accordance with my theory, the S cannot act to any extent on the O of the BaO owing to the energy with which the Ba holds the O, being represented by 124240 units instead of 68360 as in the case of H_2O. Thus we have—

$[Ba, O]$	124240		$[Ba, S, O^4]$ 338070
$[S, O^3]$	103240		
	227480		
Difference	110590	$= [Ba, S]$ 109600	
	338070		338070

Further, consider how the heat of neutralisation is accounted for. BaO, on being dissolved in water, evolves 34520 units of heat, SO_3 evolves 39153, and the difference between the sum of these and 110590 is the heat of neutralisation. Thus—

$$
\begin{aligned}
[BaO, Aq] &= 34520 \\
[SO^3, Aq] &= 39153 \\
\text{Neutralisation} &= 36896 \\
\hline
&\ 110569
\end{aligned}
$$

Again, take $SrSO_4$, and proceeding exactly in the same way, we have the following result:—

[Sr, O]	= 128440	[Sr, S, O⁴]	= 330900
[S, O³]	= 103240		
	231680		
Difference	99220 = [Sr, S]		99200
	330900		330900

$$[SrO, Aq] = 29340$$
$$[SO^3, Aq] = 39153$$
$$\text{Neutralisation} = 30710$$
$$\overline{\quad 99203 \quad}$$

Now, both these salts are insoluble in water for the same reason, viz., that the full affinity of the S for the metal is exercised, and nothing is left for the H_2 or O of the water.

Let us take next $CaSO_4$, and we shall find some still more remarkable results. In building this compound up 318370 units of heat are evolved. Tabulated in the same way as in the other cases we have—

[Ca, O]	= 130930	[Ca, S, O⁴]	318370
[S, O³]	= 103240		
	234170		
Difference	84200		
	318370		318370

Now in this case the difference 84200 is not equal to the heat of [Ca, S], which is 92000. The S therefore is not held with the full strength of its affinity for Ca. There are 7800 units to spare. What becomes of them? Consider the following:—

$$[CaO, Aq] = 18330$$
$$[SO^3, Aq] = 39153$$
$$\text{Neutralisation} = 31440$$
$$\overline{\quad 88623 \quad}$$

Now this exceeds 84200 by 4423, which is the heat of solution, viz., 4440, and accounts for so much of the difference; but the remarkable thing now comes in, $CaSO_4$ combines with $2H_2O$, and in doing so evolves 300 units of heat more than the heat of solution, viz., 4740, which is exactly the heat of combination of SH_2. We see, therefore, that owing to the whole affinity of the S for Ca not being exercised in the compound $CaSO_4$, the S can exercise its normal affinity towards H_2 of the water, and as a consequence we have $CaSO_4$ slightly soluble, while the other two analogous salts are insoluble. Further, when this compound $CaSO_42H_2O$ is dissolved in water, the excess of its heat above that of solution appears as a negative quantity, therefore its heat of solution is -300. Now this admirably illustrates the meaning of these negative heats of solution, and also two points to which I drew attention:—First, the lowering of intensity of affinity. We have in this case affinity represented by 300 units of so low a tension that its presence can only be detected when acting on two molecules of water. On a larger quantity it has no effect so far as temperature is concerned. The second point is that every molecule of water exercises affinity on every other molecule, but as the work done and undone must be equal everywhere, there is no change of temperature; but it is entirely different if one or two molecules be bound to another foreign body; the balance is then upset, and the result will be a change of temperature in one direction or the other. There are still about 3000 units of heat to account for. Now, I am not prepared to say exactly where these will be found. According to my theory the affinity of the S is now so reduced in intensity that it cannot make its presence known by evolution of heat in the ordinary way. It may be, however, that here we have the explanation of the facts pointed out in my former papers regarding the precipitation of clay suspended in water, by the addition of a very small quantity of a soluble salt.

Take one more example as extremely interesting for the fresh light it throws on the subject—consider Na_2SO_4. In building this compound up from its elements 328590 units of heat are evolved, and in addition the compound combines with ten molecules of water with an additional evolution of 19220 units. Proceeding as in the other cases we have—

$[Na^2,O]$ =	99760	$[Na^2, S, O^4]$	= 328590
$[S, O^3]$ =	103240	$[Na^2 SO^4, 10H^2O]$ =	19220
	203000		
Difference	144810		
	347810		347810

Now, how is this difference of 144810 units accounted for? We have first $[Na^2S] = 88200$, and then we know that Na_2O on being dissolved in water evolves 55500 units of heat, which on my principles are due to its unexhausted affinity for the O of the water; this affinity acts upon the O of the SO_3. Thus we have—

$$
\begin{array}{ll}
[Na^2S] & = 88200 \\
[Na^2O, O] & = 55500 \\
\hline
& 143700
\end{array}
$$

We have also heats of neutralisation and solution as follows:—

$[Na^2O, \ddot{A}q] = 55500$	$[Na^2, O] = 99760$	$[Na^2, S, O^4] = 328590$
$[SO^3, Aq] = 39170$	$[S, O^3] = 103240$	203000
Neutralisation = 31378		
		125590
		Difference 458
126048		126048

Heat of solution = 460

The balance of 18760 units of heat in the formation of the crystalline salt appears as a negative quantity on solution as in the analogous lime salt.

I have made a few determinations of the quantities of some compounds dissolved in water to see how far these conclusions derived from thermo-chemistry are borne out by actual quantities dissolved.

In my last paper I stated that the heats of solution of chlorides varied *directly* as the affinity of the metal for O, and *inversely* as its affinity for Cl. Consider the following table:—

$MCl_2 - MO$, Aq.	Quantity of MCl_2 dissolved in 100 parts of Water.
Ca = 20560	63 grains.
Sr = 26770	46 ,,
Ba = 35980	35 ,,

Now, it is apparent at once that the quantity dissolved is almost exactly inversely as the difference of heats, which is in complete accordance with the laws laid down in my former paper. With the nitrates the same relationship cannot be made so clear, as the metals are in combination with O, and the data obtainable are insufficient to trace the various affinities. The results, however, are quite in accordance with the above, although the range is much greater. Thus we have—

Quantity of $M(NO_3)_2$ dissolved.

Ca = 111 grains.
Sr = 50 „
Ba = 7 „

The differences are in the same proportion as in the chlorides. The sulphates I have already noticed.

The salts of those metals which form insoluble oxides or hydrates I leave for future treatment, as the data obtainable are defective for my purpose. So far as they go, however, they are in complete accordance with those laws I have stated.

These facts seem to me to prove, without doubt, that solution is entirely due to chemical affinity, and that chemical affinity does not act, as has hitherto been supposed, in units, but in all proportions according to the circumstances, and that in chemical combinations of all degrees every atom acts upon every other atom according to its affinity and the position in which it is placed. This way of regarding chemical affinity reduces a perfect chaos of empirical results into an orderly and systematic arrangement.

3. Thermometer Screens. Part IV. By John Aitken, Esq. (Plates II., III., IV.)

The object of this paper is to describe a new thermometer screen, and to give the results of some trials made this autumn and winter with a Stevenson screen as generally used, and one modified in the way described in a previous part of this investigation.* Also to give comparative readings taken with those screens and with the new one.

*"Thermometer Screens," *Proc. Roy. Soc. Edin.*, Part 117, p. 661.

Before entering on the subject of the paper, I wish to make a few preliminary remarks on the cause of the difference in the readings given by different screens and by other ways of protecting the thermometers; also to call attention to the interpretation we are entitled to put on curves of temperature drawn from readings taken at longer or shorter intervals of time.

All the methods in use for taking the temperature of the air give different results. One cause of this difference is the more or less perfect way in which the thermometer is protected from the effects of radiation; so that, while all tend to read too high during the day, some read higher than others. But in addition to this, there is another reason why the different arrangements give different results. This second disturbing element we will, for want of a special name, call the *inertia* of the apparatus. By the inertia of the apparatus is simply meant the resistance offered by the thermometer and its surroundings to change of temperature. The inertia may, therefore, be measured by the time taken by the thermometer to acquire the temperature of the air for a given amount of change of temperature. For example, suppose the temperature of the passing air to rise one degree, it would almost instantly heat up any small body, such as a cobweb, to its own temperature, but it would take a much longer time to heat up a larger body, though similarly exposed. The time required will depend on the mass and specific heat of the body, and on the shape and amount of surface it presents to the passing air. We see from this, that if the arrangement of apparatus we use to take the temperature of the air has a small inertia, it may, if the temperature is rising, indicate at first a higher temperature than an arrangement having a greater inertia; and, if the temperature does not remain long enough at its highest point, the apparatus with small inertia will indicate a higher maximum temperature than the other. To illustrate this point, let us first consider a purely imaginary case. We know that the temperature of the air during the forenoon of a summer's day is constantly changing. It does not rise regularly, but rises to a certain extent, then falls, then rises and falls again; and though the general tendency may be upwards, there are many breaks in the curve representing the rise of temperature for the day. This results, as we shall see later on, from the manner

in which the air is heated. In Pl. II. fig. 1, the curve A is supposed to represent the changes in temperature of the air, drawn to a scale on which the vertical lines represent half minutes, while the horizontal lines represent half degrees. During one minute the temperature often rises or falls more than one degree. For convenience of illustration, this curve, representing the temperature of the air, is shown as a smooth curve. In reality it is not likely to be so, but in all probability is a very irregular one. Suppose then the curve A represents the temperature of the passing air, then the curve representing the temperature of any very small body, such as a cobweb, will follow this one very closely. But if the body is of any size, then the curve of its temperature will be something like the curve B. Its temperature will rise and fall with that of the passing air, but the two curves will not rise and fall together, because the temperature of the body will go on rising after that of the air has begun to fall, and it will continue to rise so long as the air is the hotter of the two. In the curves, the temperature of B is shown to be rising for more than half a minute after A has attained its maximum, and it is not till A has fallen more than half a degreee, and has the same temperature as B, that the latter ceases to rise, and the curve of its temperature becomes horizontal. After this A and B both fall, but A more quickly than B, and B does not attain its lowest point till after A has passed its lowest, and risen to a certain amount, and acquired the temperature of B, after which both curves rise, but A more quickly than B.

The points to be noted here are: First, that if the top of the curve A had been the maximum for the day, then the inertia of B would have prevented it acquiring the maximum temperature, so that any arrangement of screen having a large inertia will tend to give a lower reading than one with a small inertia.

The second point is, the effect of the inertia in retarding the time of maximum temperature. The curve B does not arrive at its maximum till some time later than the curve A. These considerations help to explain why the daily maximum temperature does not occur about mid-day, when the sun is at its highest, but at a later hour. If we suppose the curve AA′, continued as shown by the dotted lines in the figure, to represent the intensity of solar radiation, then the curve BB′ will represent its heating effect on

the air. When the sun is at its highest the air is receiving its maximum heating effect, but owing to its inertia it does not acquire its maximum temperature till a later hour, till near two o'clock, at which hour the amount of heat received is balanced by that lost. After that the temperature of the air falls, but the curve representing its fall is later than A, owing to the inertia of the air, which affects a falling as well as a rising temperature. The same explanation applies to the yearly maximum, and shows why it does not occur in June, when the sun is highest and the greatest number of hours daily above the horizon, but at a later date, when its heating power has considerably diminished.

The curve C in the figure represents the effect which a still greater inertia has on the rise of temperature in a body heated by the air. As the temperature of A never falls quite to that of C, the curve C never falls, but only varies in the rate at which it rises. It will be as well to note here, that all these effects of inertia in checking and retarding the heating of large bodies are quite apart from the question of radiation and its effects on large and small bodies, which, as has been shown in a previous paper, acts in exactly the opposite way, and tends to heat large bodies to a higher temperature than small ones.

Let us now turn to the practical consideration of the subject, and see what the effect of inertia really is on the readings given by different arrangements of apparatus. On Pl. II. fig. 2, are shown curves of temperatures drawn from readings given by different arrangements, each having a different inertia. The curve FB shows the readings of a very fine bulbed thermometer. This thermometer, as was explained in a previous part of this investigation, was constructed to be used as a standard of air temperatures, with which to compare the readings given by the different screens. The bulb of this instrument is 25 mm. long, but it has a diameter of only about 1·5 mm. When in use, it is exposed under a horizontal sunshade in the manner described in Part II., and the bulb is protected from radiation by means of a sheath of pure silver, which fits it closely, but does not press upon it.

The readings given by this instrument were considered to be nearer the true temperature of the air than those given by any other arrangement, as they always kept lowest while there was any

radiation. It will, however, be evident that its inertia is very small, and when exposed either without its silver sheath or when covered with chemically deposited silver, its sensitiveness is very remarkable, its indications showing constant fluctuations in the temperature of the air, as the mercury is in a continual state of pulsation whenever there is any radiation effect. These changes amount often to more than a degree in less than a minute, even in an October day, and are much greater in bright summer weather. The fluctuations do not seem to be due to variations in the radiation, as they are observed even when radiation appears to be constant, either under a clear sun or after it has been under a dense cloud for a time. These changes of temperature would seem to be due to the air that is heated on the ground and on other radiation heated bodies not being perfectly mixed with the colder air, one part of the eddy formed by the passing air having more heated air in it than another.*

The curve FB, fig. 2, is drawn from temperature given by this fine-bulbed thermometer without any silver covering. The curve LB shows the readings of another thermometer with a larger bulb, exposed bare, alongside the fine-bulbed one. Its bulb is 22 mm.

* This conclusion seems to be confirmed by observations made by Mr Dickson with this instrument at the top of Ben Nevis. He informs me that he never observed any of these rapid fluctuations at that station. The air heated on the slopes of the mountain will be carried away sideways by the wind, and the small amount of heating effected by the limited area of ground at the top does not seem to be sufficient to give rise to these changes.

Professor Langley, in his celebrated researches with the bolometer, has observed certain fluctuations in the intensity of the solar radiation from minute to minute. As these fluctuations were ten times the instrumental errors, he is satisfied they have a real existence. He says the solar radiation would have been constant, but that the amount transmitted varied from minute to minute even in what appeared a cloudless sky. It is evident that the fluctuations observed by Professor Langley and those indicated by the fine-bulbed thermometer are not of the same order, although they take place at about the same intervals. The variations given by the thermometer are much too great to be due to variations in the amount of transmitted heat. As we have a satisfactory explanation of the fluctuations indicated by the thermometer, it seems possible that Professor Langley's fluctuations may be due to the same cause. It is very difficult to suppose a want of uniformity in the upper air so great as to cause these fluctuations in such short intervals of time. It seems more probable that the variations observed with the bolometer are due to the imperfectly mixed hot and cold, moist and dry air near the surface of the earth, which will affect the readings of that instrument by absorption and by radiation.

long and 7 mm. in diameter. It has therefore a greater inertia than the other. A and B are the curves of temperature for two Stevenson screens, one with the bottom open and the other with it closed; while C is the curve of temperature given by the new screen presently to be described. The temperatures shown in fig. 2 were taken on the 18th October. There were a few passing clouds at the time, and a strongish north-east wind was blowing. The readings were taken simultaneously by two observers, at intervals of one minute from 11·59 to 12·20, when they were stopped on account of the radiation effect falling to zero, and all the different thermometers reading nearly alike.

It will be seen from an examination of the curves that the fine-bulbed thermometer moved much more rapidly than any of the others. This is not shown so well in the curves as it might be, as the interval of one minute is much too large to show the fluctuation of this instrument, for in the interval between two observations it often indicated temperature much above or below the recorded readings. In the first rise of the curves from 12 to 12.2, there is no very great difference in their steepness. This is probably due to no observations being taken at 12.1; but take the rise beginning at 12.4, and here the effect of the inertia of the different arrangements is very marked. From 12.4 to 12.5 the fine-bulbed thermometer rose 0°·8, and the large bulb 0°·3, while the screens only rose about 0°·1. During the next minute the rate of increase of temperature of the fine bulb greatly diminished, as it was near the temperature of the air, while the rate of the others increased. It will be noticed that the two exposed bulbs FB and LB arrived at their maximum and began to fall before the screens A and B got to their maxima, and so long as the fall continued they kept falling in advance of the others. In the rapid fall which began at 12.11, in one minute the fine bulb fell 0°·9, the large one 0°·7; while the screens A and B fell only about 0°·3, and they took four minutes to fall the 0°·9 lost by the fine bulb in one minute. One effect of this is that while the fine bulb, if read at very short intervals, would give a curve like the edge of a saw, with irregular teeth set at intervals of less than one minute, the inertia of the screens causes them to smooth over these irregularities, and to give a curve with fewer and less abrupt changes.

It will be observed that while these curves show fairly well the effect of inertia on the rate of the heating and cooling of the thermometers, yet they do not show that the arrangement with smallest inertia gives the highest readings, as we concluded from a consideration of the curves in fig. 1. The reason for this is that the curves in fig. 2 are complicated by radiation effects. The large bulb and one Stevenson screen read higher than the fine-bulbed thermometer. This was caused by those arrangements being more affected by radiation than the fine bulb. If we take readings given by the screen C and the fine bulb—the two which are least affected by radiation—it will be observed that the fine bulb rises above and falls below C, somewhat in the manner indicated in the imaginary curves fig. 1, only it rises too high, owing to its being more heated than C by radiation.

It may be as well to note here that the two thermometers exposed under the sunshade did not give more correct temperatures than the screens, but it must be remembered that they were not coated with silver when these readings were taken. It may be interesting to note that the fine bulb gave lower readings than the large bulb, though similarly exposed; this will be seen from an examination of the curves FB and LB. All the curves in this figure follow each other more closely than they would under many conditions. The reason for their comparative closeness on this occasion was doubtless the amount of wind that was blowing at the time the readings were taken. It is evident that a quick circulation of air will have great influence in reducing the time necessary to heat or cool the screens, and will have very much less effect on the exposed bulbs. I much regret I was unable to return to this investigation till late in the season, by which time the radiation effects were greatly reduced, and, owing to the amount of bad weather, very few observations were made. The curves shown in fig. 2 are the result of the only one-minute observations I have been able to make. If these readings had been taken in spring or summer, and when there was sunshine and little wind, the fluctuations would have been much more marked, and the relative steepness of the different curves better brought out.

Turning now to the consideration of the question as to what interpretation we are entitled to put on curves of temperature made

from observations taken at longer or shorter intervals of time, a very little consideration will show us that these curves, as generally constructed, are in no sense curves of temperature.

An examination of the one-minute observations given in fig. 2 shows us that the curve of daily temperature is much too complicated to be capable of being represented in the manner it generally is. A few detached observations at hourly intervals tell us really very little about the matter, and to attempt to draw a curve from these can lead to no good, as the curve appears to give definiteness where in fact almost all is unknown; this is particularly the case when there is any radiation. Observations taken at intervals so wide apart as one hour really tell us nothing about the state of matters in the intervals, and yet by connecting these hourly readings by means of curves we not only indicate the temperatures in the interval, but we may even represent the temperature at the hour of observation to be rising or falling when in reality it may be doing quite the opposite.

To illustrate the small value we are entitled to put on curves drawn from hourly observations of temperature, the curves in Pl. II. fig. 3, are drawn from one of the few sets of observations taken recently at regular intervals. The curve A is drawn from observations taken at five-minute intervals, and it will be seen that during the two hours while the observations were made, there were great fluctuations in the temperature. An examination of fig. 2 will, however, show us that even during five-minute intervals considerable changes may have taken place. The curve A, fig. 3, is therefore not so variable as the actual state of the air was when these five-minute readings were taken. Suppose now that, instead of taking the temperature every five minutes, we had done it at hourly intervals. If, for instance, we had selected, not the hour but five minutes to the hour, for the time for taking our readings, then we should have got the curve B. If, however, we had selected five minutes past the hour for our observations, we should have got the curve C, while if the readings had been taken five minutes later the curve would have been D. This latter curve would have shown a higher maximum for the day of 2°·75 above that given by curve B. This difference might have been brought about, as stated, by the hour at which the observa-

tions were made, or it might have resulted from the hour at which the high maximum happened to be reached, or it might even have been produced by an error in our time-keeper, either advancing or retarding the time of observations, while keeping them at hourly intervals. It is evident, therefore, that curves of temperature drawn from readings taken at intervals, unless very short, have but little value, and may be most misleading. It would be better therefore, instead of curving the results, simply to connect them with straight lines, so as to enable the eye easily to catch the successive readings, it being understood that these lines give no information as to the state of matters between the observations.

New Screen.

In Part III. of this communication (*Proc. Roy. Soc. Edin.*, No. 121) reference was made to some attempts made to check the entrance of radiant heat through the air passages into the draught tube screen. The most successful results were got by introducing small screens between the bulb of the thermometer and the source of radiation, and so arranging the air circulation that all air that had touched the radiation-heated surfaces was drawn away through side passages, and only the central core of unheated air allowed to pass on to the centre of the screen, and come into contact with the bulb and its surroundings. The plan, however, which was found to work well in a room, in still air, was found to be quite unsuitable for observations in the open air, owing to the wind causing eddies inside the screen which interfered with the proper circulation of the air, and mixed the air heated on the screens with that entering the centre chamber; an attempt was therefore made to see if the principle could not be modified to enable it to be applied in a form suitable for open-air observations.

A number of complicated forms suggested themselves, but as all of them seemed likely to give rise to eddies inside the screen, they were abandoned without trial. At last the simple form shown in fig. 1 was designed, and a screen of this form was constructed at the beginning of October. The figure shows a vertical section through the centre. As will be seen, it is of extremely simple construction. The circulation of air through this screen is entirely affected by the natural movements of the air. No modifi-

cation of it with draught tube has yet been attempted. The screen consists of two distinct parts,—the lower, or screen proper, surrounding the bulb of the thermometer, is constructed to protect the

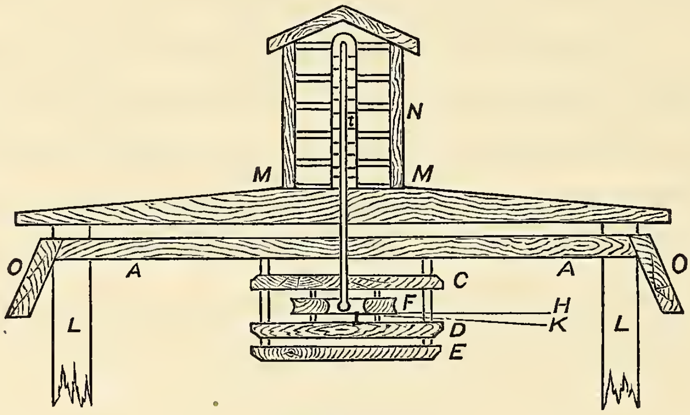


Fig. 1.

thermometer from all radiation from below, while the upper part protects the lower screen from the direct rays of the sun. The upper part consists of a square sunshade AA made of wood, and supported in a horizontal position at the proper height from the ground by means of four wooden supports LL attached to the corners, and fixed firmly in the ground. If the direct rays of the sun fell on AA it would get highly heated, and would heat the air on the under side of it, which might affect the readings of the thermometer. To prevent this, another piece of wood MM is placed over AA parallel to it, but with an air space between the two, to check the passage of heat downwards. The lower part of the screen consists of the three plates C, D, and E, fixed parallel to each other and to the lower side of the sunshade AA in the position shown. The plates are held in their places by long screw nails passing through the four corners. These plates may be made of any substance that is a non-conductor of heat; wood is the only material yet tried. The bulb of the thermometer *t* is placed in the space between the two plates C and D, where it is protected from all radiation both from above and below, and to protect it from the horizontal radiation, it is surrounded by the annular piece F shaped in the manner shown. The stem of the thermometer *t* passes upwards through the sunshade, and is protected by means of the louvred box N.

When the screen was first used, the sunshade part consisted only of the two pieces AA and MM, but when the sun got very low in winter it was found to affect the correctness of the readings, and

the small vertical sunshades OO were added to the screen. These pieces of wood are fixed in a sloping position, to prevent any air heated on them from passing downwards towards the thermometer. As these pieces will slightly interfere with the air circulation, it is possible the screen will act better without them, if observations do not require to be made when the sun is very low. They are not, of course, required on the north side of the screen, or where houses or trees shut out a low sun. The best size for the sunshade has yet to be determined. If made larger than shown, say 3 feet square, then only a very low sun could have any effect on the readings. This screen for future reference is called screen C.

Let us look at the action of this screen when placed in the open air. It will be seen that the air has a very free circulation through it; the plates being horizontal and placed at a distance from each other, the air has a perfectly free passage through it from whatever direction it may blow. It will be further noticed that the bulb is perfectly protected from radiation from all bodies outside. Turning now to the manner in which the heat absorbed by the screening surfaces is prevented from affecting the readings of the thermometer: First, the large sunshade AA prevents any part of the screen proper from being heated by the direct rays of the sun, and it has thus only the diffused radiation to contend with. The under side of AA will be a good deal heated, but the hot air in contact with it will pass between AA and C, and not come into contact with the bulb. The plate E, with the air space between it and D, prevents the heat radiated from the ground passing upwards to the upper surface of D. If D was a perfect non-conductor, E would be unnecessary. The only hot air that really gets into the screen surrounding the bulb is the air heated on the under surface of C and upper surface of D, and also that heated on the upper and under surfaces of F. As will be seen from the construction of the screen, very little heat falls on the surfaces of C and D, so they will be but little heated; but the air that gets heated on these surfaces does not come into contact with the bulb, but tends to flow straight through and out at the other side of the screen, keeping to the surfaces of the plates. This will not be the case when the wind is strong, but when the conditions are trying, that is, with little wind, it seems probable that the heated air will pass through without mixing much with the

cold. It will be seen from this that the radiant heat which will affect the thermometer will be that absorbed by the two horizontal surfaces of F, and the heat absorbed at the outer surfaces of F, and conducted through the wood into the inner chamber surrounding the thermometer bulb. The amount of heat received from the first of these sources will be very small, as the horizontal surfaces of F receive most of their heat at second hand, that is, after being radiated and reflected from C and D. The amount received by direct radiation from without is very small, and is represented by the angle KIH in the figure; from which it will be seen that these surfaces have a verv limited exposure to outside objects, and the amount conducted through F is probably very small. It will be seen from the sketch that the annular piece F has its outer surface groove-shaped. The object of this groove is to prevent the air heated on it from flowing into the inner chamber, the groove conducting it round the outside. How far this groove is necessary, or to what extent it improves the readings, I cannot say, nor can I say whether the double bottom is necessary, or whether the passage between F and D might not be abolished, and the screen and sunshade thus made smaller. It was thought advisable to take all these precautions, as there was not time this season to work upwards from the simpler to the more complicated; these points were therefore left for future consideration.

It may be mentioned that this screen has been tried without the annular piece F, and it was found to work very well, but did not give quite such low readings as with it in, and its inertia was also much less without the ring. When the piece F is out, the bulb is freely exposed to radiation from all surrounding objects; but as the space between C and D can then be reduced, the bulb does not get a very wide view of the outside. Its readings without the annular piece were very much more correct than the Stevenson screen. This at first may seem strange, as the bulb of the thermometer in it is much more freely exposed to radiation than the one in the Stevenson screen. The reason for its lower readings would appear to be that it is exposed to the radiation from trees and other objects high up, freely exposed to the wind, and therefore cooler; whereas the bulb in the Stevenson screen is exposed to the highly heated grass. This new screen without the ring has not been tried in a

situation exposed to walls and other large surfaces that get highly heated in the sunshine.

Owing to the lateness of the season, no trials with this screen have been made under severe test conditions. In the beginning of October it was fitted up on the lawn, and near it was placed a horizontal sunshade under which was placed the fine-bulbed thermometer with its silver sheath, and a considerable number of comparative readings were taken. This was done on a number of days and in different conditions of weather, and the screen has proved itself to be considerably in advance of all the others. Its readings being quite as good as those given by the fine silvered bulb, I shall not attempt to say which gives the lowest readings, as the conditions of the trials have not been sufficiently severe or varied enough to bring out any decided difference; but under all conditions yet tried the screen was quite as low as the silvered bulb. It is unnecessary here to give any detailed account of these trials by themselves, but I shall presently give some comparative readings taken with this screen, with the Stevenson screen with the bottom open, and with it closed, also with the silvered bulb, showing that the new screen gives much lower and more correct readings than either of the other screens, and, as the circulation through it is very free, these lower temperatures cannot be due to the high inertia of the new screen.

Trial of Screens.

In the first and second parts of this communication the results were given of some trials of different methods of protecting the thermometer against the effects of radiation. The conclusions arrived at were, that the most correct readings were given by the thermometer placed in a strong current of air produced by a suction fan, or by a fine-bulbed thermometer exposed under a sunshade with its bulb protected by a silver sheath. Readings taken by these two methods agreed very well with each other, and either of them was taken as a standard of temperature with which to compare the readings given by the different screens. Compared with these standards, the ordinary Stevenson screen was found to give readings of from 1°·3 to 2°·8 too high, when there was much radiation and little wind. Further, it was found that when the Steven-

son screen was closed with either a louvred or solid bottom the error was greatly reduced.

After these tests were made, the Scottish Meteorological Society took up the matter, and made a number of comparative trials with the apparatus. The first of these tests were made at Granton in the summer of 1885 by Mr H. N. Dickson. After the Granton work was concluded, Mr Dickson took some of the screens to the top of Ben Nevis, continued the investigation there, and produced a most careful and elaborate set of observations under the conditions existing at the top of the mountain. The Ben Nevis work has not yet been published, but Mr Dickson communicated some of the results of his work at Granton to the Royal Society of Edinburgh.* In this communication he gives curves of the temperatures for two days. These temperatures were taken by the fan apparatus, the silvered bulb, and one Stevenson screen with bottom open and another with it closed. On examining these curves I was much astonished to find that they confirmed none of the conclusions arrived at from trials made here. At Granton the fan and silvered bulb did not give the best results; the silvered bulb read highest throughout the whole of the second day, and further there was little to choose between, in the readings of the Stevenson sereens with open or closed bottoms.

These results obtained by Mr Dickson at Granton are so different from mine, that I thought it necessary to reconsider the matter, and again go over my work, under the conditions existing here. In my first trials I had only one Stevenson screen; its readings were compared with those given by the fan apparatus and the thermometer with silvered bulb; and, comparing the readings with the standards when the bottom was out and when it was in, the result was greatly in favour of the observations made with the bottom closed. Mr Dickson made his trials with two screens,—one sent by me with louvred bottom and double top, and another of the ordinary pattern, that is, with bottom open.

In the autumn, my old screen with its louvred bottom was returned from the trials at Granton and Ben Nevis, and I obtained a new one of the standard pattern and exactly similar, only with open bottom and single top; the latter of these screens in the

* *Proc. Roy. Soc. Edin.*, No. 120, p. 199.

following is called screen A, and the old one B. The screens were fitted upon the lawn at a distance of about 15 feet apart, in as nearly as possible similar exposures to sun and wind. This was done on the 14th September, and in order to make sure that both screens gave the same temperature, comparative trials were made with them, the bottoms of both being open. Though the day was not a very suitable one for the test, as there was not much radiation, still the old screen was found to give higher readings than the other by about half a degree. This was owing to the paint being dirty, and the surface of its louvres being better absorbers of heat than the louvres of the new and clean one. The screens were now painted white, after which they looked nearly alike in whiteness, and on trial with both bottoms open were found to give readings nearly alike.

As it would be almost impossible to set up the two screens in positions exactly alike with regard to exposure to wind, radiation, &c., both screens were provided with movable bottoms, so that either could be worked with the bottom closed while the other was open. When testing the screens, sometimes the same screen was kept closed throughout the whole day, at other times first the one then the other would be closed, while the external conditions remained constant, so as to check any difference in temperature due to position or condition of screen, direction of wind, &c.

In these trials the ordinary thermometer used for taking wet and dry bulb observations was employed. The wet bulb with its apparatus was removed, and the thermometer in each screen was placed where the wet one usually is, the index of the instrument being turned slightly round, so that it could be read by opening the door to only a very small extent. The object of this was to prevent radiant heat entering and altering the readings while they were being taken; also to prevent radiation heating the inside of the screen. The thermometers with which most of the observations were made had round bulbs about 8 mm. diameter, but others with smaller bulbs were occasionally used. All the thermometers were graduated on the stems, had wide scales, and were easily read. In addition to the Kew corrections, they were all carefully compared with each other in water at intervals of 5 degrees or less. The room where these comparisons were made was heated to the temperature of the water, so that the temperature of the large volume of water surround-

ing the thermometers might be kept constant, and the errors due to imperfect mixing be as small as possible. It may be also mentioned that the same thermometers were not kept in the same screens, but they were changed from time to time, to check any error that might arise from any unknown difference in the thermometers, and every precaution that could be thought of was taken to check the results.

The maximum thermometers used were all graduated on the stems and placed vertically in the screens with their bulbs in the usual position. They were held in their places by spring clips, to prevent the position of the index being altered by shaking, a source of error to which the metal-framed instruments loosely hung are very liable. The instruments were carefully tested with their stems vertical. The common maximum thermometers with metal frames and placed horizontally were discarded, as they were found to give most uncertain results, and never agreed with the others in the same screen, owing to the index in these instruments moving too easily, and more or less easily at different parts of the bore, thus forming a longer or shorter air space at different parts of the scale. Further, under certain conditions they gave different readings from the ordinary thermometers with freely exposed bulbs. They read too low if the high temperature remained only a short time, owing to their greater inertia; and in the open Stevenson screen they read too high when radiation remained strong for any length of time, owing to their larger surface causing them to be much more heated than the thermometers with freely exposed bulbs, as the frame and thermometer really act as one surface. The maximum thermometers were changed from screen to screen, generally every day or second day, as an extra check on possible errors.

A great number of observations were made, extending from the 15th of September to the end of November. On many days when there was much radiation, a great number of readings were taken at short intervals, and on as many other days as possible the temperature was taken by maximum thermometers. The result has been entirely to confirm my first conclusions. The closed screen always gave lower readings than the open one.

The fan apparatus was not put on trial, as there were too many readings to be taken without it. In a trial like this the readings of

all the instruments ought to be taken at the same moment, owing to the constant changes of temperature in the air. In practice it is therefore desirable to make the number of thermometers to be read as few as possible. The silvered bulb was, however, again put on trial, and as before, it gave readings much below the Stevenson screen; considering the season, its readings were as much below the screen as was observed in the first trials already mentioned.

On 21st September a number of readings were taken from time to time during most of the day with the two Stevenson screens, the bottom of A being closed, while B was open. These readings are marked off at the top of Pl. III., and the different readings connected by straight lines. The day was fine, with passing clouds, a little wind, and radiation fairly strong for the time of the year. It will be observed, that till after mid-day there was but little difference in the readings of the two screens; this was owing to there being but little radiation before that hour. At a little before 12 o'clock the open screen was only 0°·3 above the closed one. A little before 1 o'clock the bottom was taken out of A, and by 1 o'clock both screens read nearly alike.

The bottom was again put in A, and when the next reading was taken at 1.15 the open screen read 0°·5 higher than the closed one, and during the whole day the open one gave the highest readings, the amount varying according to the radiation at the time. The black parts at the top of the diagram represent sunshine; they cannot, however, be very correct, as they are drawn from the notes taken at the hour the readings were made, and thus only represent the condition of matters at that time, no intermediate record being taken. A sunshine recorder would have enabled me to put in these curves more correctly. The general result is, however, very easily seen from the record given. It will be noticed, that whenever there was sunshine the open screen read much higher than the closed one, and that during the absence of sunshine they tended to read alike, but it was not till after radiation had entirely ceased that they read quite alike. At the 2 o'clock and the 2.5 readings the open screen was 0°·9 higher than the other, while a little before 3 o'clock the difference was as much as 1°·1.

While this trial was in progress, in addition to the thermometer placed where the wet bulb usually is, and which gave the readings

shown on Pl. III., another thermometer was hung up at the back of each screen, with its bulb in the place where the bulb of the maximum thermometer is usually situated, and readings of these thermometers were taken at the same time as the others. These readings showed that the thermometers placed at the back of the screens indicated on this occasion a greater difference than those placed at the sides. The one placed at the back of the screen with open bottom was as much as 1°·4 higher than the one at the back of the closed screen.

The readings given in the middle of Pl. III., taken on the 25th September, show that the open bottom affects the readings not only on fine sunshiny days, but also on dull ones. On the 25th, the sky was uniformly covered all over with a dense mass of clouds, through which the sun was never visible. But though clouded, there was a good deal of heat reflected and radiated from the sky, the surfaces of all exposed bodies were hotter than the air, and the temperature of the grass rose as much as twelve degrees above the temperature of the atmosphere. At the beginning of this trial screen A was open, while B was closed. At 11 o'clock, when the first readings were taken, there was but little difference in the temperatures given by the two screens, but at 11.35 the open screen read 0°·5 higher than the closed one. After the 11.35 reading, the bottom was taken out of B, and put into A. After which it will be seen that the lines connecting the temperatures cross each other, and B, which at first was closed and read lowest, now that it is open reads higher than the closed one by 0°·6. These readings show that even on a dull day there may be a considerable difference in the readings given by the two screens.

The third set of observations at the foot of Pl. III. show a series made on the 7th October with the two screens A and B and with the new screen C. In these curves, as well as in those taken on the 25th September, there is a curve marked G. This curve represents the temperature of the grass, and was taken by means of a thermometer placed with its bulb on the grass underneath the open screen. This curve is not drawn to the same scale as the others, as there would not be room for it; the temperature rises so high it could not be represented within the limits of the plate. For this curve each space between the lines represents 1 degree, instead of 0°·2 of a degree, and as the disturbance produced by the hot grass will be in

proportion to the excess of its temperature above that of the air, the lowest curve—namely C, in the 7th October observations—is taken as the base line, and the excess of the temperature of the grass above C is marked off. For instance, at 11.30, the temperature of the air as given by C was 57°, while the grass was 70°, or 13 degrees above C. Read in this way, the G observations show us that at 12.10 the grass was 18·5 degrees above the temperature of the air.

No curve is given of the silvered-bulb observations, though they were taken at the same time. The reason for this is that they would only confuse the figure, as they were practically the same as those of the screen C, only they were sometimes a little higher and at other times a little lower than C, owing to the smaller inertia of the silvered bulb.

The day on which this trial was made was fine, with some wind from the south-west till near mid-day, when it fell, the air was clear, and there were a few passing clouds. It was not thought advisable to take readings till near 11 o'clock, as the Stevenson screens were quite wet in the morning, and tended to read low, owing to the evaporation. At 10.45, when the readings were begun, the bottom of screen A was open, and B closed. Readings were taken with the screens in this condition till 11.45. After which screen A was closed, and B opened. At 12.30 the condition of the screens was again reversed, B being closed and A open.

It will be noticed that from 10.45 to 11.45 there was not much difference between the open screen A and the closed one B, even though the radiation effect was strong, as will be seen from the amount of sunshine indicated by the black area below the curves and by the curve G. The maximum difference between A and B amounted to only 0°·5; this was probably due to the strongish wind blowing at the time. After 11.45, when the bottom of A was closed, and B opened, the lines connecting the temperatures cross each other, and B now, instead of being the lower, becomes much the higher, the open screen showing a maximum difference of 1°·5 at 12.10. This great error in the readings of the open screen was doubtless due to the wind dying away at this hour. After the bottoms were again reversed, the lines again cross each other; but as the sun did not again come out, and the radiation curve G fell very low, there was not much difference in the two screens; but it will

be observed, that as long as the readings were taken, the open one always read a little higher than the closed one.

The curve C shows the temperatures given by the new screen, fig. 1. It will be observed that its readings are very much below those of either of the Stevenson screens. This difference varied from time to time, according to the amount of radiation, but it always remained during the whole time of the observations a good deal lower than the others. At 12.10 it attained its maximum difference, being at that hour 2°·3 lower than the ordinary Stevenson, and 0°·8 lower than the closed Stevenson.

In addition to the observation shown on Pl. III. a number of others have also been taken, but the particulars need not be given here. The general result has always been the same. The Stevenson screen with closed bottom always read lower than the open one, and the C screen lower than either, the amount depending on the radiation, wind, &c., at the time. The observations on Pl. II. fig. 2, show the readings of the three screens on the 18th October. On this occasion the difference in the screens was not great. It will be seen that the open Stevenson only went 0°·5 higher than the C screen, while the closed one was only 0°·2 higher. This small difference was due to the lateness of the season, the small amount of radiation at the time, together with the strength and direction of the wind. The wind was fresh, and from the north-east; it therefore entered by the cold side of the Stevenson screens, and tended to keep the side of the screen on which the sun was shining colder than if it had come from the opposite direction.

Further, in addition to these observations, made at short intervals of time with the ordinary thermometers, the temperature has been taken almost every day in the three screens by maximum thermometers, and the result is always the same. On very few days do they all read alike; only when there is stormy weather and dense masses of clouds; at all other times there are differences, but the three screens always keep the same relative position, C being lowest, and the closed screen lower than the open. It may be mentioned, that in these trials with the maximum thermometers, the same instruments were not always kept in the same screens, but were generally changed every morning, to equalize any instrumental errors, and screens A and B were worked alternately open and closed.

Even so late as the beginning of November, I was astonished to find that the three screens gave very different readings, as will be seen from the following maximum temperatures observed on 31st October and 1st and 2nd November:—

Date.	Stevenson Closed.	Stevenson Open.	Screen C.
October 31	55·4	56·1	55·0
November 1	51·5	51·8	50·6
,, 2	52·8	53·0	52·0

On these three days the weather was very fine, with sunshine and little wind, and on all of these the ordinary Stevenson gave readings of from 1° to 1°·2 higher than the C screen.

These differences in the readings astonished me greatly at the time; and when on the 16th of November a difference of more than a degree was again recorded in the readings of the Stevenson and the C screen, I began to have some doubts as to the correctness of the results, as I thought that long before November arrived radiation would be so weak that it would not interfere seriously with the correctness of the readings given by the Stevenson screen; and yet the observations showed that on some days in this month the error was very considerable. It was, therefore, thought advisable to make some more trials of the screens by means of ordinary thermometers, and taking readings at short intervals. The morning of the 17th November being fine, with little wind and a cloudless sky, readings were taken at five-minute intervals. On this occasion it was not found possible to make a comparison between the ordinary Stevenson screen and the modified form, because, owing to the lowness of the sun, the shadows of the tops of one or two distant trees passed across the screens, sometimes one and sometimes the other being in the shadow; and as the screen in the shade always read lowest, it was impossible to compare the open with the closed one. They were therefore both worked open, and the readings taken of the one in sunshine. These readings, with those given by screen C, are shown in the middle series of observations in Pl. IV. The readings were begun at 11.15 A.M., but the Stevenson screens not being dry, no readings were taken with them

till 11.45, at which hour there was a difference of 0°·9 between screen C and the Stevenson. At 12.5 the difference was as much as 1°·2, after which it fell to about 1°·0 at 12.30; after this hour the weather changed, the sky clouded all over, the radiation effect gradually fell, and the readings having no further interest were stopped.

The following day, the 18th, being a most perfect day, the trials were continued. On this occasion the sun rose in a cloudless sky, and shone brightly till it set. The sun was warmer, and the wind less than on the previous day. All the conditions were thus favourable for a trial of this kind, as they tended to bring out in a marked manner any differences due to radiation. As on the previous day, the readings were taken at five-minute intervals, with two short breaks. They were begun at 10.15 A.M., and continued till 3.15 P.M.; the temperatures are all shown in the lowest series of curves in Pl. IV. It will be seen that on this occasion the difference was often more than 1°·0, and attained a maximum of 1°·85 at 2.20 P.M. The wide separation of the curves at this hour was due to the wind dying quite away, so that, though the louvres were not exposed to so strong a radiation at that hour as they had been at an earlier part of the day, yet they got more highly heated, as there was no wind to cool them.

It may be mentioned that the fine silvered bulb was also on trial on these two days. It is, however, impossible to enter its readings on Pl. IV., as they were almost exactly the same as screen C, scarcely ever varying from it more than 0°·1 ; only once it was observed to vary 0°·2, but generally the two readings were the same. We may, therefore look on the readings given by the C screen as nearly correct on those days, and those given by the Stevenson as too high.

We now come to the consideration of the cause of this very great error in the Stevenson screen, so late in the year, when radiation is so much reduced. As the error is principally due to the heating of the louvres by solar radiation, and as the temperature to which they are raised depends on the amount of heat received by them, and the rate at which this heat is carried away by the air, we shall consider these two points separately. First, as to the radiation. On the two days on which these trials were made, two different kinds of radiation thermometers were exposed to the sunshine. One of them was an ordinary vacuum black-bulb thermometer,

and the other was one of my radiation thermometers having a plate 14 inches square. The following are the maximum readings recorded :—

Date.	Vacuum Thermometer.	14″ Black Plate.
November 17	81°·5	96°·5
,, 18	91°	113°·5

These radiation temperatures seem high for so low an elevation as the sun attained at that date, for we must remember that the temperature of the air was low at the time. Suppose it had been a warm summer day, and the temperature of the air 75°, then the same solar radiation as that of the 18th would have raised the temperature of the vacuum thermometer to 121°, and the other one to 143°·5. Still stronger radiation effects were observed on the 2nd December; the air on this day was 32°, vac. 81°, and black surface 111°. This seems to point to a wonderful diathermancy of the air in winter compared to summer. I write without sufficient observations, but I imagine an equally low sun in summer would not have anything like this heating effect.* Then again, the louvres of the Stevenson screen are exposed almost perpendicularly to the rays of a low sun; they therefore receive more heat than from an equally hot but higher sun in summer.

As stated, owing to shadows passing over the screen, it was not found possible to make a comparison during the winter months between the ordinary Stevenson and the modified form. This, however, does not seem to be a matter of much importance, as we can scarcely expect to find much difference in their readings at this season. The daily maximum readings up to the 16th November do indicate an advantage in favour of the closed form; but this

* The greater diathermancy of the air in winter than in summer has been observed by M. Soret at Geneva, and Professor Langley has carefully measured it. He finds the greater transparency in winter to be chiefly for rays of short wave lengths, and he finds a close relation between the diathermancy and the amount of vapour in the air. I much regret I have no low-sun summer observations with which to compare my winter ones, but the difference in this climate seems to be very much greater than that observed by M. Soret or Professor Langley. This difference will probably be due to the position of my observations being much further north than Geneva or Mount Whitney, and subject to greater extremes of dryness and moisture.

may possibly be the result of accident, and due to the open screen being more frequently in sunshine than the other, at the time the maximum temperature was attained. The sun at this season being very low, its rays strike so nearly horizontally they scarcely heat the ground, all their heat being received by trees and other vertical surfaces. There will therefore be but little heat radiated into the screen from the ground, and there will not be much reflected. While the observations were being made on the 17th and 18th November, no thermometer was placed on the grass, as its indications would have been valueless, owing to its showing a great difference in temperature according to the situation of the bulb. It would have read high and above the temperature of the air if it happened to be in sunshine, but if shaded by the grass it would have read much below the temperature of the air, as it was observed that the small hollows in the grass which had got frozen during the night remained frozen all day. It is evident from this that a thermometer placed on the grass would not enable us to say whether an excess or deficiency of heat was radiated in through the open bottom of the screen; but of course some heat would be reflected inwards, however cold the grass.

We may here refer to a point of some interest observed when making these trials in November. In the previous parts of this communication frequent mention has been made of the quick fluctuations in the temperature of the passing air, as revealed by the constant pulsations of the fine-bulbed thermometer. It was observed in these last trials that these quick changes were almost entirely absent. The mercury in the fine-bulbed thermometer rose and fell nearly as steadily as the one in the C screen, seldom varying from it more than 0°·1. The apparent dulness in the movements of this instrument would seem to be caused by the entire change in the manner in which the air is heated by solar radiation in winter compared to summer. When the sun is low the ground is but little heated by its rays, and there is no layer of hot air near its surface swept along by the wind, and imperfectly mixed with the colder air above, to give rise to a succession of warm and cold parts; and the heating received by vertical surfaces is distributed through a much greater depth of atmosphere, and is more easily and perfectly mixed with the passing air. The air passing the thermometer has therefore a much more uniform temperature

with a low than with a high sun. If this is the correct explanation, then it confirms our conclusion that the fluctuations observed in this thermometer in summer were due to imperfectly mixed hot and cold air, and not to variations in the radiation.

We now come to the consideration of the second point, namely, the power of the passing air to check the effects of radiation. It is very evident that this will depend on the rate at which the air passes over the radiation-heated surfaces. Now, in summer, we have—apart altogether from what we call wind—an unstable condition of the atmosphere which acts quite locally. Owing to the heating of the ground, the air over it is rarefied and tends to rise; its stability is thus constantly disturbed by slight movements. But in winter we have a totally different condition of matters. Radiation from the earth being now in excess of that towards it, the surface of the ground gets cooled, and the air on it tends to become denser, and so keep closer to the ground, and if the country is flat, there is no tendency as in summer to local movements, but on the contrary, the tendency is towards stability. We thus see that the solar radiation effect in summer is to cause—in addition to winds proper—slight local airs, which prevent the surface of the louvres in the screen from being highly heated; whereas in winter the tendency owing to the lowness of the sun is the other way, and we have degrees of calmness in winter, quite unknown and impossible in summer. With these two things, namely, the high heating power of even a low winter sun, and the great calmness of air possible and frequent at this season, we seem to have the explanation of the somewhat unexpectedly great error of the Stevenson screen observed during the winter season. The error at this season is due principally to the heating of the louvres, and but little to heat radiated in through the open bottom.

In connection with the more perfect calms which take place in winter than in summer, and as showing their effects on the comparative cooling produced by them at the different seasons, we may here refer to the readings given by the two forms of radiation thermometers used in this investigation, namely, the ordinary black-bulb thermometer *in vacuo*, and the flat black surface thermometer having an area of 14 inches square. The ordinary black bulb gives a very complicated result, indeed it is extremely difficult to interpret

its readings. It measures, however, chiefly the intensity of the solar radiation that is received at the surface of the earth, as modified by the surroundings of the bulb. The flat black surface, on the other hand, gives the intensity of the solar radiation as modified by air currents; the stronger the wind the lower the temperature recorded for a given intensity of radiation; whereas the black bulb *in vacuo* is not much affected by wind, though it is affected by other and rather obscure influences; the flat black surface therefore gives a better indication of the climate than the black bulb. In fine summer weather it was found that the flat black surface generally read about 12 per cent. above the black bulb; but in winter this difference has been found to be very much greater, on account of the more perfect calms at that season permitting the exposed surface to be more highly heated.

But further, it is found that there is a close relation between the difference in the readings of the two radiation thermometers and the errors of the Stevenson screen. An examination of the readings taken in December show that as the ratio of the temperature of the black surface to that of the black bulb increased, the error in the screen increased along with it. When the ratio of the black surface to the vacuum black bulb was under 1·5, the error of the Stevenson screen was about 1 degree, and as the ratio increased, the error increased along with it. This error attained its maximum, as far as was observed, about mid-day on the 13th of the month; at that hour the temperature of the air was 34°, the vacuum black bulb 64°, and the flat black surface 93°. The black bulb was thus 30° above the temperature of the air, while the black surface was 59°, or in the ratio of nearly two. At that hour the Stevenson screen reached its maximum error for the day, being 2°·9 above the standard. It may be noted that when the error of the screen was at its maximum the vacuum thermometer did not register the maximum temperature for the month; indeed it was one of the lowest on the bright days, but owing to the entire absence of wind at the time, the difference in the heating effect of the sun's rays on the two forms of radiation thermometers, and the error of the Stevenson screen attained their maximum.

While on this subject, it may be as well to consider an effect of these two tendencies of air when heated and when cooled,—the one to instability and currents, the other to stability and calmness. Given

a condition of weather in which there is no wind, then in summer the effect of heating the air on the ground is to cause ascending currents of hot air. There is thus an influence at work in summer tending to keep the air near the ground at nearly the same temperature at all places within a considerable distance; while the effect of cooling the air in winter is precisely the opposite, as the cooled air tends to sink down into hollows and remain there, where its temperature is further reduced by contact with radiating surfaces. The cooling of the air in winter, therefore, in the absence of winds, will tend to give rise to differences of temperature, which may amount to some degrees within a limited area.

From the above we can see that on a day on which there is no wind, the temperature of the air in an exposed situation, and where the sun is shining, may be some degrees warmer than the air in a cup-shaped hollow, into which the sun's rays do not penetrate, provided always that the sky overhead is clear and radiation into space strong. Take the case of the 18th November: on that day the temperature of the air was 45°, while the grass that was not exposed to the sunshine was under 32°. We can easily imagine conditions in which the air resting on grass at 32° in a hollow into which the sun does not shine might easily be cooled some degrees below the temperature of the air above. These observations are suggested by the peculiar condition of matters reported from different parts of London on the 24th November. In one part of London the maximum temperature was 40°, while at another it did not rise above 32°, a difference of 8°, which might be accounted for in the way above stated, as there was no wind at the time.

A record having been kept for some time of the maximum temperatures recorded by the three screens, the readings taken from the 23rd September to the 25th November are marked off at the top of Pl. IV. The temperatures were not taken every day, but every observation taken is marked. Most of these are from the readings of the maximum thermometers, but on those days on which the screens were under trial, the readings taken at short intervals were used, and the maximum recorded readings selected. As it was impossible within the limits of the plate to record the actual temperatures, only the differences between the readings are

shown. The readings of the open Stevenson screen are used as a base line, and the difference between its readings and those of the closed Stevenson and the screen C are marked off, each series being connected by straight lines. It will be seen that on only two occasions did the screen C give the highest readings, and on these exceptional occasions it was only 0°·1 higher. As the days on which these occurred were dull and windy, the differences were probably errors of observation, such as are quite to be expected. It will be observed that the close screen generally held an intermediate position between the open one and screen C, its readings being better than the former, but not so good as the latter.

Accepting screen C as our standard, which we may do for the present, its readings being the same as the fine silvered bulb, an examination of the maximum temperature curves shows us that in autumn and winter the Stevenson screen is frequently more than 1° too high. In the first half of October it was more than 2°·0 too high on two occasions, and in November it was a degree or more wrong eight times—in addition to the seven times shown on plate there was an error of 1°·1 on the last day of the month—and on one occasion in that month it was as much as 1°·75 too high. The differences shown in Pl. IV. are the differences in the maximum readings, but these are not necessarily the greatest differences for the day. Of course, the differences in the maximum readings are what are practically required, yet it may be interesting to note that this may not be the greatest difference for the day. For instance, on the 21st November the maximum readings for the day were 39°·6 for screen C, and 41°·2 for the open Stevenson, thus giving a difference in the maximum for the day of 1°·6 as shown. The day being very trying, a reading was taken at 1 o'clock, when the index of the maximum thermometer in screen C was at 38° and in the Stevenson at 40°·6, or a difference of 2°·6. This great difference was due to there being a dead calm a short time before.

The weather on the different days is not entered on the plate, as it is quite unnecessary; the curves speak for themselves. Whenever the reading of the screen C was much below the others, the weather was fine, and it was only during cloudy and stormy weather that all three agreed.

The observations for December show that the maximum given

by the Stevenson screen on eight days was one degree or more above the maximum given by screen C. The maximum difference for the month was 1°·75. The screens gave the same maximum on eleven days, while the mean maximum temperature for the month was 36°·78 by the Stevenson, and 36°·25 by the C screen; that is, the C screen gave an average maximum temperature of fully half a degree below the Stevenson. Of course, the average error is determined very much by the number of bright days in the month. Taking the average error for the fine days of the month, it was about 1°·4, and that would have been the error if the month had been bright throughout. The difference for January 1887 promises to be very small. Owing to the dull and clouded weather, the screens have read exactly alike on almost every day of this month. The observations for December and January are not entered in the plates.

Returning now to the consideration of why the result got by the different screens at Granton differ so much from those obtained here, I think I have taken every precaution to ensure the correctness of my results; and yet we find, even so late as the middle of November, that the Stevenson screen with open bottom gave higher readings than the closed one; also that the thermometer with its bulb sheathed in silver gave, as in the previous trials, readings much lower than either of them, and yet the observations made at Granton show no such differences; how then are we to account for the difference in the results obtained at the two places?

The first thing that suggests itself as a possible cause of the differences is the condition of the louvres in the two screens. Was the one dirtier than the other, or were the absorbing powers of the paints on the two screens different? We have seen that, on a not very trying day, in September, the absorbing powers of the two screens used in my last trials caused a difference of about 0°·5. On a bright day, such as those on which the Granton trials were made, this difference would be greater; and if the closed screen was in the Granton trials the better absorber of the two, this might have neutralised any advantage arising from its being closed. With regard to the explanation of the high readings given by the silvered bulb at Granton, I have great difficulty. No doubt, any imperfection in the cleanness of the silver would increase its absorbing powers and so raise the temperature, but doubtless care

would be taken to keep the polish in as perfect a condition as possible.

In addition, however, to these possible sources of error, there is in the situation of the two places where the observations were made an essential difference which would affect the results. The site of the screens at Granton was very freely exposed to every breath of wind, being on a knoll in the middle of a field near the sea shore, perfectly open to the west, north, and east, while the land rose a little to the south, and the screens were at a great distance from trees or anything that could obstruct the free circulation of the air. The site on which the screens are placed here is very different, and not so good in many respects, though I think it will compare favourably with the site of many screens in daily use. Here the screens are on a lawn, and surrounded at no very great distance by trees. As the surroundings of the screens seem to be a matter of greater importance than might at first be thought, it will be as well that I state more fully the conditions surrounding the screens here. Standing at the screens, the view in the different directions is closed in principally by trees. The view to the south and round by west to north-west is closed in by a narrow line of trees running north and south, at a distance of 26 yards at the point where it comes nearest to the screens. From the north-west to the north-east, at a distance of about 24 yards, there is a holly hedge 8 feet high, and also a few trees. From north-east to east are trees at a distance. From east to south-east a holly hedge running north and south, and coming at its north end to within 12 yards of the screens. From south-east to south are stables and kennels at a distance of 35 yards. The ground slopes slightly down to the north. The screens are placed east and west of each other, and all the ground in view is under grass. It will be seen that, while the position is somewhat sheltered from winds blowing from east to south-east, it is fairly open to winds from south to south-west, as well as from north-west and north-east. But, on the whole, it is evident the site is much more confined than the Granton one.

An evident result of the opener exposure of the Granton site is, that the Stevenson screens would be kept cooler there, on account of the freer circulation of the air through the louvres, the screens would thus be both more nearly correct, and therefore nearly agree

with each other and with the silvered bulb, at the more exposed Granton site than at the more confined one here. Further, on both days on which the Granton trials were made, the wind entered the screens from the cold side. This of itself is a most important point, especially if the sky is clear, as the cold sides of the screens may be below the temperature of the air, and the passage of the air in that direction prevents the advance inwards of the heat from the sun-heated louvres. I have noticed in the trials here that on all occasions on which the wind was north of east or west the errors were comparatively small. It therefore seems possible that the more open exposure of the Granton site, together with the direction of the wind at the time the two sets of the Granton observations were made, are the principal causes of the difference in our results.

Since this paper was given in, Mr Dickson, who has continued his trials with the Stevenson screens at the top of Ben Nevis, has very kindly furnished me with an abstract of the result obtained in that situation. He says the readings for thirty-four days gave the following mean maxima :—

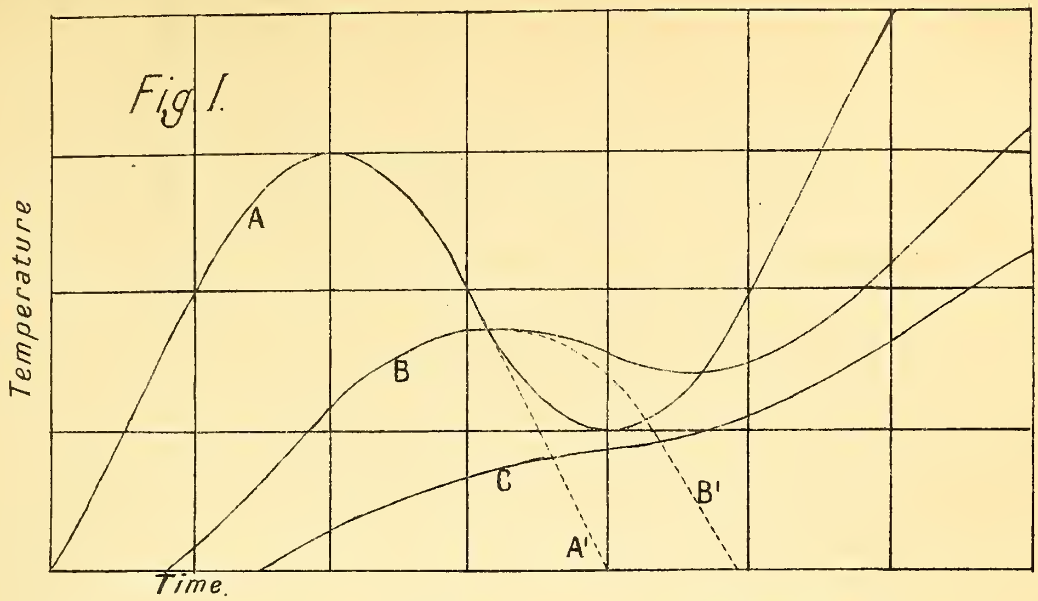
Stevenson,	Open bottom,	41°·45.
„	Closed „	41°·00.

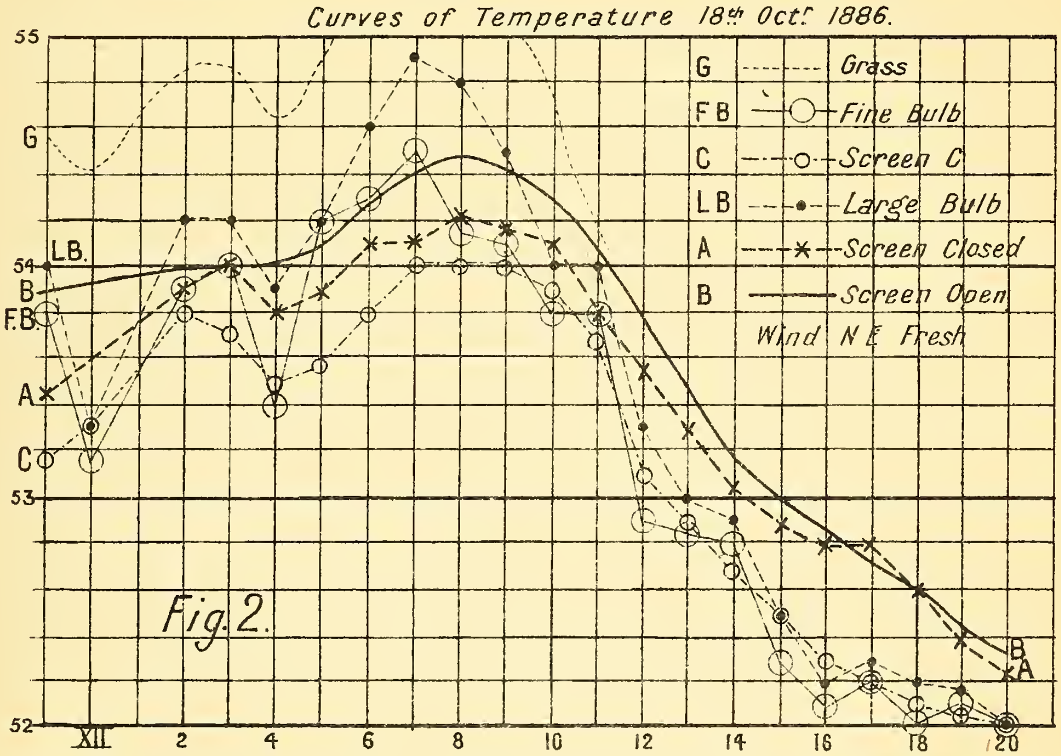
Thus showing a difference in favour of the closed bottom of nearly half a degree, a result confirming the conclusion arrived at in this and a preceding paper.

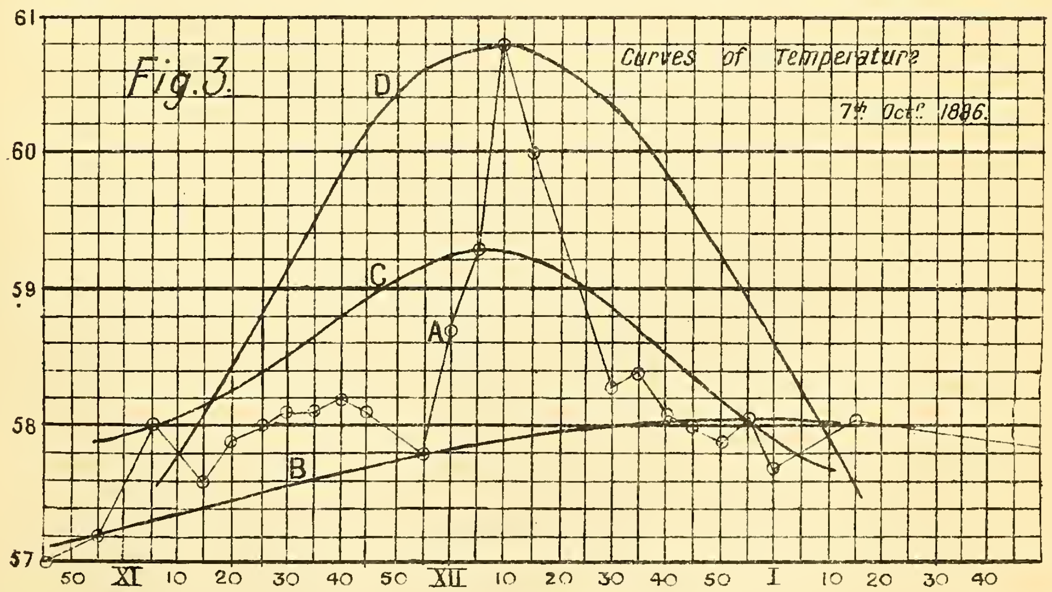
The site here being surrounded by trees in almost every direction, may in part explain the reason why screen C, even when the annular piece F was removed, gave such correct readings. We have seen that on the 7th October the temperature of the grass rose 18° above the temperature of the air. Now that is the temperature to which the thermometers in the Stevenson screen with open bottom are exposed, while screen C, by its construction, cuts off all this radiation and exposes the bulb to the radiation from the trees, which will never be so highly heated as the grass, as they have a freer circulation of air through them. This, combined with the very free circulation of air through the screen, and the method adopted for preventing the heated air touching the thermometer, would seem to account for the low readings given by this screen even when the piece F is removed.

There would appear to be an advantage in favour of the C screen, which may be referred to here. We have seen that when the louvres of the Stevenson screen get dirty, they absorb more radiant heat, and so increase the error of the readings. Many screens in daily use must, from this cause, give too high readings. The screen C is, however, not much affected from this cause; indeed, I am not quite certain but that the screen will act quite as well if certain parts are black. For instance, the sides of the plates C and D which are exposed to the bulb of the thermometer might perhaps with advantage be blackened. I have not yet been able experimentally to determine this point, but many of my observations have been taken with a large black patch in the centre of each plate. My reason for testing this was, that if these surfaces are white, they will reflect to the bulb some of the heat which falls on them; but if they are black, they will absorb this heat; and it seemed possible that the increased amount of heat *radiated* by the blackened surfaces, together with the greater amount to which the air in contact with them is heated, might affect the thermometer less than the heat *reflected* by the white. It was not found possible to settle this point by readings taken with the screen under the two conditions and comparing them with the silvered bulb, as the inertia of the two arrangements is so different, and the effect sought for very small. This, with many other matters connected with this screen, will be best settled by means of two screens similar in all points save the one we wish to test. For these trials, however, we must wait another warm season. Although this new screen has acted very satisfactorily up to the present time, giving readings almost exactly the same as the fine silvered bulb standard, and much below those of the Stevenson screen, yet it would be rash to conclude that it will be superior under all conditions of climate. The various influences to which thermometer screens are exposed are so numerous that the unexpected has many opportunities of happening and upsetting our hopes and expectations.

[A Postscript to this Paper will be found immediately after the *Proceedings* of July 1887.]







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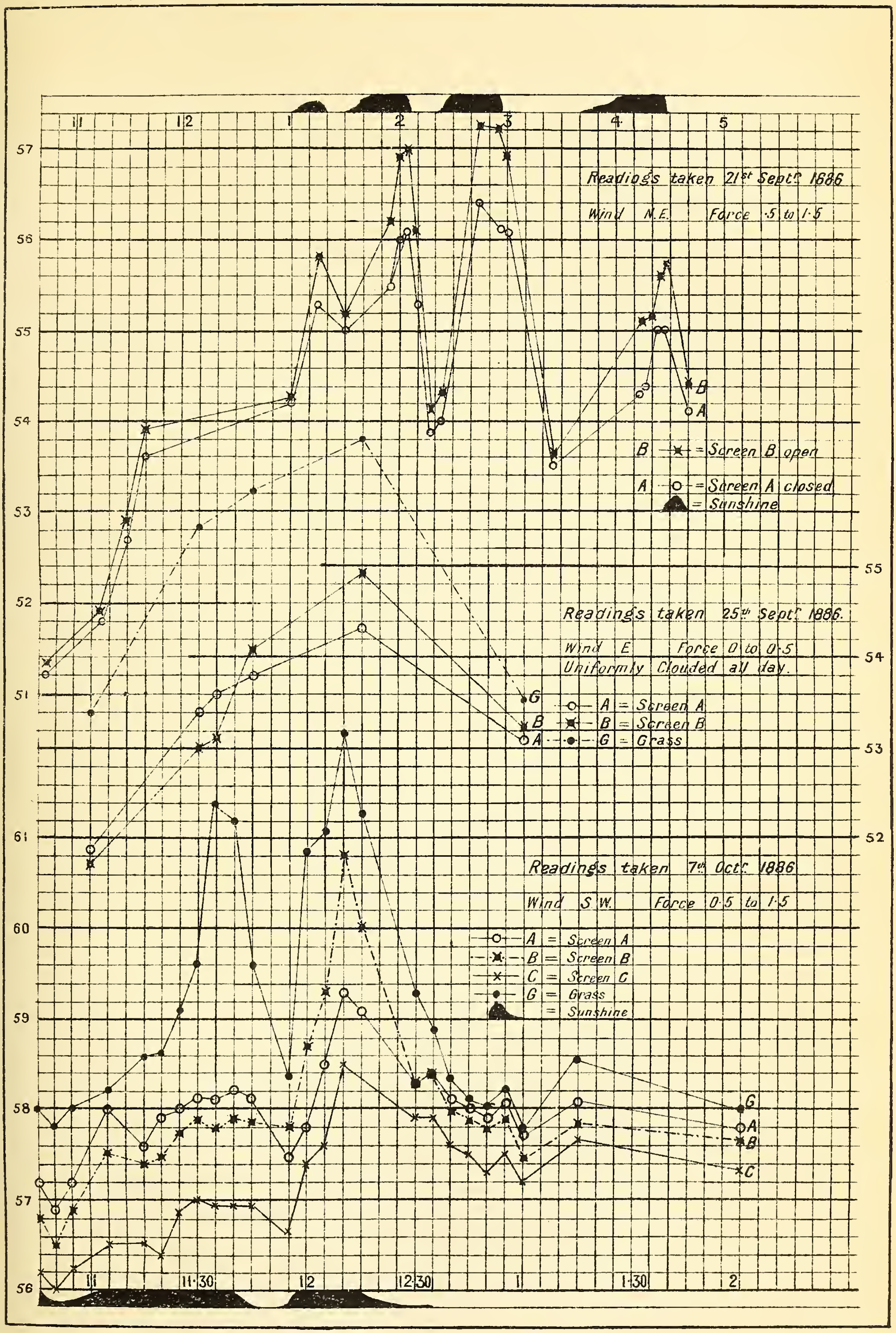

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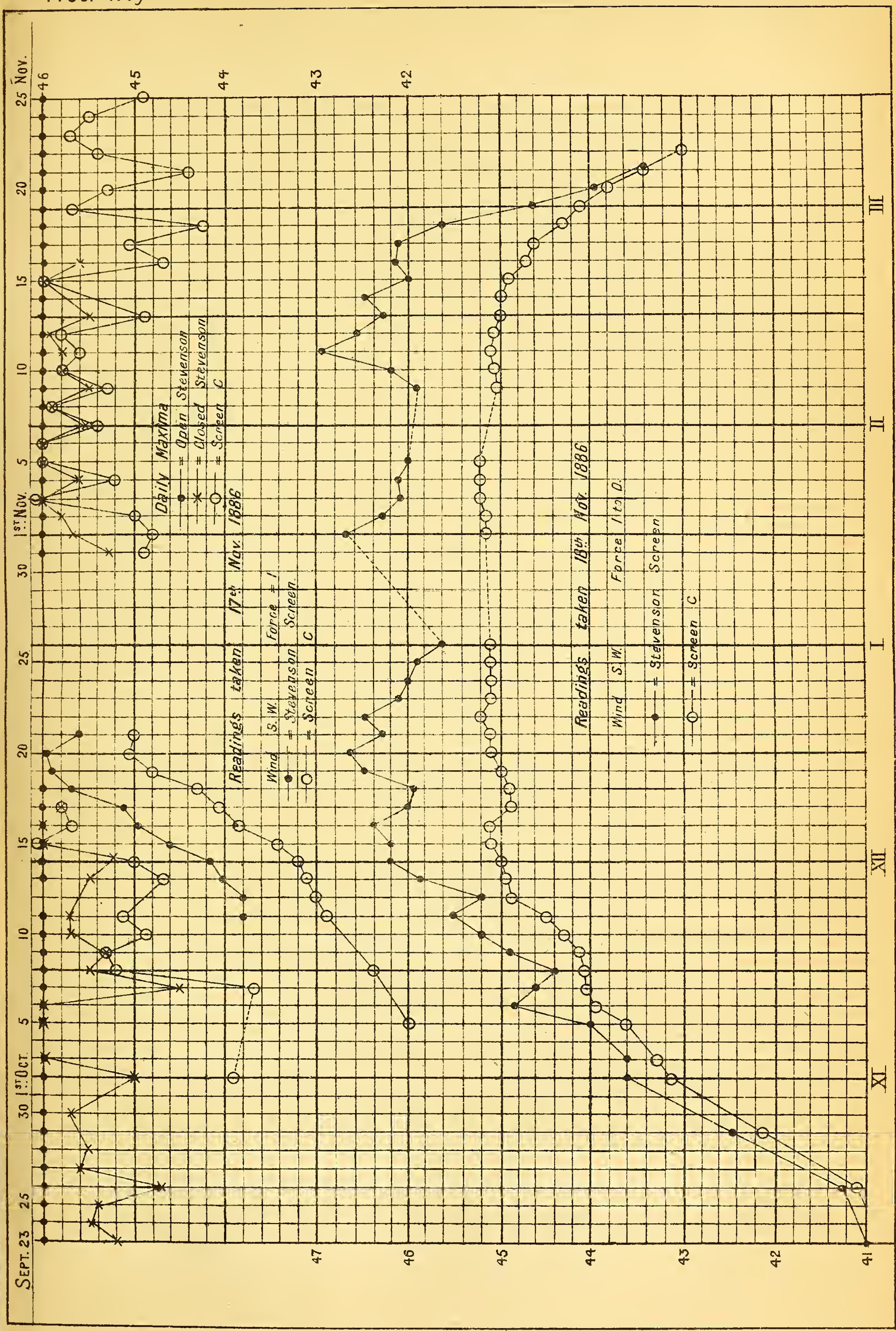

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4. On the General Effects of Molecular Attraction of Small Range on the Behaviour of a Group of Smooth Impinging Spheres. By Professor Tait.

(*Abstract.*)

The present instalment traces some of the consequences of assuming the hard spherical particles of a gas to exert intense molecular forces when at distances comparable with their diameters. The effect of the new term in the virial in counteracting and at last obliterating that due to the impacts, is traced as the gas is gradually compressed.

Next, the spheres (still supposed to attract one another) are regarded as capable of absorbing energy in a vibratory form, and of losing it directly by radiation. In such a case the relative translatory energy may be so reduced that pairs of spheres may *remain* within molecular distance from one another. The bearing of these results upon condensation, dissociation, &c., is given.

PRIVATE BUSINESS.

Mr Nanabhay A. F. Moos, L.C.E., B.Sc., Assistant Professor of Engineering, College of Science, Bombay, was balloted for, and declared duly elected a Fellow of the Society.

Monday, 31st January 1887.

JOHN MURRAY, Ph.D., Vice-President, in the Chair.

The following Communications were read:—

1. On a New Formula for the Pressure of Earth against a Retaining Wall. By A. C. Elliott, B.Sc., C.E. Communicated by Professor Armstrong.

There are two main distinct methods of attacking the problem of the retaining wall. The first in chronological order is due to Coulomb, and is variously named, perhaps most commonly as the method of the Wedge of Least Resistance. Briefly characterised, it

might be said to depend upon the mathematical artifice of finding the resultant force due to the mutual action of the earth mass, and the wall a maximum, the earth being supposed to yield incipiently under the action of its weight, and in opposition to friction and the reaction in question, along an inclined plane determined so as to fulfil that imposed condition. Coulomb's method has been developed by various writers, and may be regarded as complete.

The second method is due to Rankine. It is based upon two general dynamical principles, both of which are really involved in Coulomb's treatment, but which are there drawn upon as it were incidentally rather than appealed to as fundamental principles. Rankine's first principle is merely a statement that the well-known propositions in regard to the laws of static friction apply in the interior of a granular mass of earth; and, in particular, that there is a coefficient of friction for earth upon earth of any given kind. That some sort of physical datum of this nature with respect to any given kind of earth may be properly assumed does not admit of question; but how far it answers to an ordinary physical constant, or even an ordinary coefficient of friction, is by no means certain. However, objections of this kind apply to Coulomb's method with even greater force, and the author proposes to attempt to push Rankine's theory farther on its present bases, rather than to discuss preliminary difficulties. If, therefore, he shall be fortunate enough to arrive by a path not altogether mistaken at certain results, he would merely say that such are the consequences of adopting these fundamental principles.

The first of the principles just referred to enabled Rankine to formulate the conditions of equilibrium in the interior of an earth mass generally, and in terms of certain data for particular cases occurring in practice.

When he comes to deal with the action of the earth on a wall, Rankine refers to his second principle, which was first distinctly laid down by the late Canon Moseley. Briefly it is merely this:—When a system is in equilibrium under a set of forces, those which are called into existence by the action of the others are the least possible consistent with the given conditions; or, among a set of forces, active and passive, in equilibrium, the passive forces are the least possible.

Rankine uses the term granular mass to indicate that the earth is assumed to have no tenacity or cohesion. Some kinds of material might fairly claim to be so described, but in ordinary practice it is quite common to meet with earth which will stand with a vertical face for a considerable time; but since the action of time and weather will inevitably result in the material ultimately assuming a slope more or less constant for that particular kind, it is not only prudent but necessary to allow for the almost total failure of tenacity or cohesive strength with lapse of time. In the retaining wall problem the effect of assuming any degree of tenacity being clearly operative in reducing the resultant force representing the mutual action of the earth mass and the wall, Rankine makes no scruple to discarding tenacity altogether.

It may be remarked that Coulomb's method implicitly takes account of some degree of tenacity. Nearly all direct experiments have shown considerable divergence between the actual overturning moment of the earth pressure on the wall, and that calculated by Coulomb's or Rankine's method (employing the accepted methods for determining the principal constant), the divergence very commonly amounting to upwards of 50 per cent. in excess of the observed value. The discrepancy is, in the author's opinion, in great part clearly due to the ignoring in the mathematical investigation of the effect of tenacity. On the ground that no satisfactory allowance could be made on account of a quantity which is at once a function of time and weather, Rankine, as has been already remarked, expressly rejects the tenacity from consideration, so that, in the case of Rankine's formula at least, one ought not to be surprised if the calculated should exceed the actual overturning moment of the earth pressure to a considerable extent.

But granting this, or at any rate taking leave of it, there still remains the experimental evidence that Coulomb's method in its complete form, though much more unsatisfactory from a physical point of view, gives better results than Rankine's. Attention has consequently been redirected to Rankine's method with the object if possible of removing this anomaly; and, accordingly, it has been pointed out that Rankine simply applies the conditions of equilibrium, obtaining at a point in the interior of the earth mass, to a point situated in the surface of separation, between the wall and

the earth mass, thus tacitly neglecting the boundary conditions. Rankine, in short, neglects the friction between the earth mass and the wall, or supposes the wall to be perfectly smooth. Dr Maurice Levy and Professor Boussinesq have worked at the problem thus presented, but the author is only partially acquainted with Prof. Boussinesq's results, and not at all with Dr Levy's. In 1881 Mr Benjamin Baker read a paper before the Institution of Civil Engineers, "On the Actual Pressure of Earthwork"—a valuable contribution to the literature of the subject, to which reference will afterwards be made. An interesting communication from Professor Boussinesq is printed with the correspondence appended to the paper, which will be found in vol. lxv. of the *Proceedings.*

The author will confine himself to the case where the surface of the earth to be retained is level. Rankine's hypothesis of a granular material and the fundamental principles before mentioned are assumed.

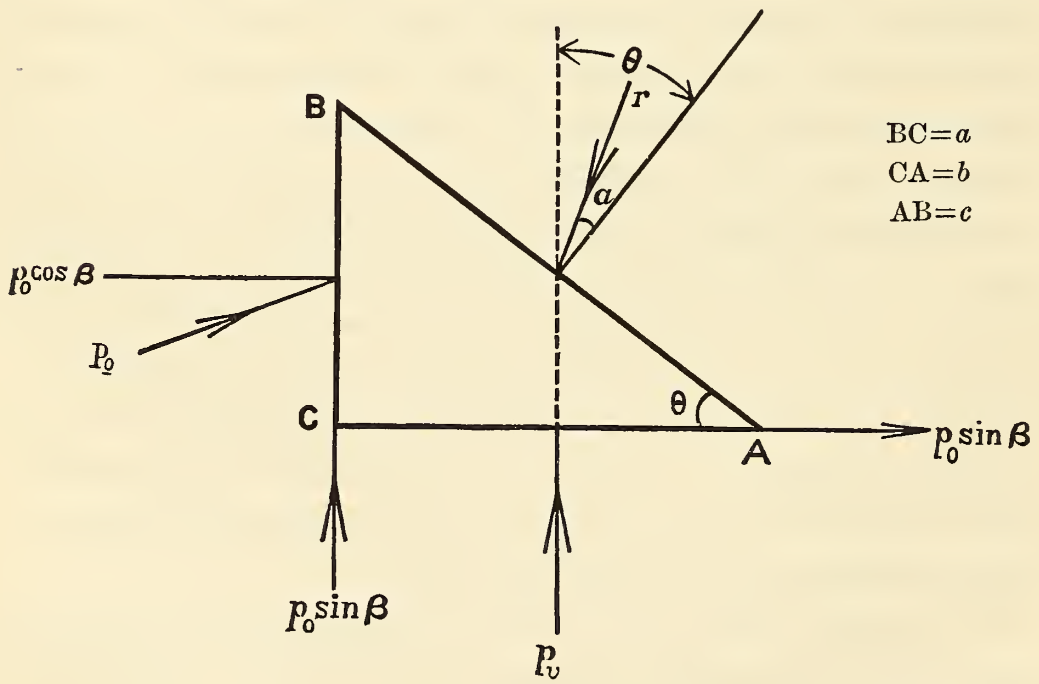


FIG. 1.—Vertical Section of a Right Prism of Earth.

Let ABC be a vertical section of a right prism of earth, whose length-axis is parallel to the inner face of the wall, which will here be assumed to be vertical. Let BC be in contact with the wall, and AC horizontal; also, let the prism be of unit length, and let the transverse dimensions be very small.

Consider the forces acting on the prism :—

(1) Its weight: this may be neglected in comparison with the other impressed forces, which are small quantities of the second order, while the weight is a small quantity of the third order.

(2) The forces acting on the end faces of the right prism :—these are independently balanced as regards the wall, and may henceforth be left out of consideration.

(3) The remaining impressed forces acting parallel to the plane of the section: of these let p_v be the vertical pressure in the neighbourhood of the prism, due to the column of earth; p_0 the inclined pressure exerted by the wall.

β the angle between the direction of p_0 and the normal to the inner face of the wall, drawn outwards. This angle β, in the case where motion is just about to take place along the interface BC, will be equal to the angle of friction for the earth on the wall.

r the pressure of the contiguous earth on the face AB of the prism;

α the obliquity of r; or the angle which the direction of the stress r makes with the normal to AB;

θ the angle BAC.

Considering first the equation of moments, it will appear that the tangential stress on AC must be equal to the tangential stress on BC; but the tangential stress on BC is $p_0 \sin\beta$, which is therefore the value of the tangential stress on AC.

Resolving vertically and horizontally

$$p_v b + p_0 \sin\beta \,.\, a - r\cos(\theta-\alpha)\,.\,c = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$p_0 \cos\beta \,.\, a + p_0 \sin\beta \,.\, b - r\sin(\theta-\alpha)\,.\,c = 0 \quad . \quad . \quad . \quad (2)$$

Dividing out by c and substituting in terms of θ,

$$p_v \cos\theta + p_0 \sin\beta \sin\theta = r\cos(\theta-\alpha) \quad . \quad . \quad . \quad . \quad (3)$$

$$p_0 \cos\beta \sin\theta + p_0 \sin\beta \cos\theta = r\sin(\theta-\alpha) \quad . \quad . \quad . \quad . \quad (4)$$

Eliminating r, and writing s for the ratio p_0/p_v

$$\frac{1 + s\sin\beta\tan\theta}{s(\cos\beta + \sin\beta\cot\theta)} = \frac{1+\tan\alpha\tan\theta}{1-\tan\alpha\cot\theta} \quad . \quad . \quad . \quad . \quad (5)$$

If now θ be regarded as the independent, and α as the dependent variable, to find the condition for α a maximum (5) must be

differentiated with respect to θ and $\frac{d\alpha}{d\theta}$ put equal to zero in the result —*i.e.* (5) must be differentiated partially with respect to θ. There then results the condition

$$\tan \alpha = \frac{s \sin \beta}{s \cos \beta \sin^2 \theta - \cos^2 \theta} \quad . \quad . \quad . \quad . \quad . \quad (6)$$

In strictness, this step ought to be immediately justified by an examination of the sign of $\frac{d^2\alpha}{d\theta^2}$, under the condition (6). This will be found to involve a considerable amount of labour; and, since the operation is essentially of the nature of a verification, the author proposes to accept, for the present, (6) as the condition for α a maximum, and afterwards to justify whatever assumption this may involve, by a process of verification. The mere fact, however, of obtaining a result which does not necessarily imply that α must be zero, indicates the existence of a maximum or minimum condition different from $\alpha = 0$; which is, of course, the condition which α fulfils when it is a minimum as regards mere numerical magnitude. Farther, from the theory of stress, it is known that, corresponding to the maximum value of α, there is a maximum value of equal numerical amount but of opposite sign.

Now the maximum value of α is to be ϕ, the angle of repose; the angle, that is, whose tangent is the coefficient of friction of earth upon earth, for earth of the given kind.

Let θ_1 be the value of θ, when $\alpha = \phi$. Then $\theta = \theta_1$ and $\alpha = \phi$ must satisfy (5) and (6) simultaneously. Therefore—

$$\frac{1 + s \sin \beta \tan \theta_1}{s(\cos \beta + \sin \beta \cot \theta_1)} = \frac{1 + \tan \phi \tan \theta_1}{1 - \tan \phi \cot \theta_1} \quad . \quad . \quad . \quad (7)$$

and

$$\tan \phi = \frac{s \sin \beta}{s \cos \beta \sin^2 \theta_1 - \cos^2 \theta_1} \quad . \quad . \quad . \quad . \quad . \quad (8)$$

(8) may be written

$$\tan^2 \theta_1 = \frac{1 + s \sin \beta \cot \phi}{s(\cos \beta - \sin \beta \cot \phi)} \quad . \quad . \quad . \quad . \quad . \quad (9)$$

Eliminating θ_1 between (7) and (8) there is obtained finally, after solving for s,

$$s = \frac{\cos\beta(1+2\tan^2\phi) \pm 2\sec\phi\sqrt{\cos^2\beta\tan^2\phi - \sin^2\beta}}{4\sin^2\beta\sec^2\phi + \cos^2\beta} \quad . \quad (10)$$

giving the relation between s, β, and ϕ, desired.

(10) may be written in a form more convenient for calculation thus—

$$s = \frac{\cos\beta\{2-\cos^2\phi\} \pm 2\sqrt{\cos^2\beta - \cos^2\phi}}{4-\cos^2\beta\{4-\cos^2\phi\}} \quad . \quad . \quad . \quad (11)$$

Again, when $\beta = 0$ (11) may be written

$$s = \frac{1 \pm \sin\phi}{1 \mp \sin\phi} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

And it will also be observed that, when $\beta = \phi$, the quantity under the radical sign becomes zero, and when $\beta > \phi$, negative, rendering the whole expression imaginary from a physical point of view.

These equations give two values of s consistent with the conditions. Since, however, the reaction of the wall is a *passive* force, corresponding to the *active* pressure of the earth, it appears at once from Moseley's principle that the smaller value of s must be taken. Therefore, finally,

$$s = \frac{\cos\beta\{2-\cos^2\phi\} - 2\sqrt{\cos^2\beta - \cos^2\phi}}{4-\cos^2\beta\{4-\cos^2\phi\}} \quad . \quad . \quad (13)$$

and

$$s = \frac{1-\sin\phi}{1+\sin\phi} \text{ when } \beta = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

Given therefore the angle of repose of the earth, and the corresponding friction angle for the earth on the wall, the direction and amount of the mutual action becomes determinate if we assume that failure cannot take place until β has attained its limiting value, which may be denoted by β_2. It may be at once remarked that, however closely β may approach to ϕ, it can never exceed that value ; since, when $\beta = \phi$, the surface of separation, between the wall and the earth, becomes a plane of rupture. When, on the other hand, it is supposed with greater generality that at the time of fracture $\beta_2 > \beta < \phi$, equilibrium must be destroyed without any relative motion between the wall and the earth in the immediate neighbourhood of the inner face having, in the first instance, taken place. In other words, the inclination denoted by β has not reached its

limiting amount when failure occurs along one or both of the two possible planes of rupture which may be shown to exist.

The author ventures to advance the view that the condition of things may be rendered intelligible by a second application of Moseley's principle. For, consider the stability of the wall, and suppose the critical condition to have been attained, and that so before β had reached the value β_2. To the actual value of β looked upon for the moment as a limiting angle, there corresponds a certain roughness of the wall; and suppose that by some external agency (such as a change of temperature or the like) the degree of roughness of the actual wall to be, in effect, reduced to this value. It is impossible to conceive that by a process such as this the critical state can have been disturbed. On the other hand, there can be no difficulty in perceiving that, if it is a fact that friction between the earth and the wall adds to its stability, failure may be brought about by simply allowing the degree of roughness to fall ever so little below the critical value—the value, that is, which corresponds to the actual value of β when the wall is in the critical state. Hence β has a value such that the stability of the wall in the critical state is a maximum; or, in other words, the overturning moment of the earth pressure for any given wall is a minimum with respect to β.

In most cases which occur in practice, β may range through all values up to ϕ; for not only are the inner faces of retaining walls usually left rough, but they are frequently stepped in a manner which makes a cross section somewhat resemble a flight of steps.

The author has calculated some numerical values for s, corresponding to certain values of ϕ and β, which will be found in an annexed table. He has also tabulated the corresponding values of the ratio of breadth to depth of a wall whose density is equal to the density of the earth, and whose moment of stability is just equal to the overturning moment of the earth pressure. A polar curve showing the values of these ratios for $\phi = 40°$ corresponding to the successive values of β is also annexed.

Equation (14) resulting from (13) by putting $\beta = 0$, is identical with the corresponding formula of Rankine. The author has, however, farther verified the formula (13) and by consequence (6), for the case where $\phi = 60°$ and $\beta = 30°$. Inserting these values in (13),

$$s = \cdot 0853 \qquad \frac{1}{s} = 11 \cdot 723$$

To find the principal axes for this stress system, put $\alpha = 0$ in (5), and let θ_a be the corresponding value of θ. This gives, on solution of the resulting quadratic,

$$\tan \theta_a = -\frac{f - \cos \beta}{2 \sin \beta} \pm \sqrt{\frac{(f - \cos \beta)^2}{4 \sin^2 \beta} + 1} \quad . \quad . \quad . \quad (15)$$

where f is written for $1/s$.

Hence, $\tan \theta_a = \cdot 046 \text{ or } -21 \cdot 760$

$$\therefore \quad \theta_a = 2^\circ\, 38' \text{ or } -87^\circ\, 22'$$

Assigning to p_v for simplicity the value 10, and making use of (3) or (4),

$$r' = 10 \cdot 02 \qquad r'' = \cdot 718$$

where r' and r'' denote respectively the greatest and least principal stresses.

Now from Rankine's ellipse of stress it is seen at once that the obliquity has a maximum value on two sections symmetrically situated with respect to the axes of amount

$$\sin^{-1} \frac{r' - r''}{r' + r''},$$

which for the case in point becomes—

$$\sin^{-1} \frac{9 \cdot 302}{10 \cdot 738} = \sin^{-1} \cdot 866 = 60^\circ\,;$$

and this agrees with the original assumption. The relative positions of the two planes for which the obliquity is maximum, the principal axes, and the vertical and horizontal directions are shown in the diagram, upon which, also, the ellipse of stress for the system in question has been represented.

Suppose, for simplicity, that the cross section of the wall is rectangular. The oblique pressure p_o, inclined to the normal to the inner face of the wall at the angle β, is found, for any point, simply by multiplying the ratio s by the pressure due to the column of earth at that point. Let the earth have the uniform density ρ_e. Then the oblique pressure on the face at the depth h from the surface is

$$p_o = sh\rho_e.$$

The force on an element of area 1 foot long horizontally, and of depth dh is therefore

$$dP = sh\rho_e dh.$$

Hence the whole action of the earth on the wall per foot-run is represented by a force

$$P = \tfrac{1}{2}h_1{}^2 s\rho \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

where h_1 is the effective depth of the earth. P will act at the centre of pressure, and be inclined at the angle β to the horizontal. Let b be the breadth, h the height, and ρ_m the density of the wall. Then, equating the overturning moment to the moment of stability, and solving a quadratic

$$\frac{b}{h_1} = \frac{\rho_e}{\rho_m}\left\{ -\frac{s \sin\beta}{2} + \sqrt{\left(\frac{s\sin\beta}{2}\right)^2 + \frac{\rho_m}{\rho_e} s \frac{\cos\beta}{3}} \right\} . \quad . \quad . \quad (17)$$

A good example of the divergence of the results obtained by Rankine's formula from actual facts, even where the earth mass approximates to the hypothetical granular material of the mathematical investigation, is adduced by Mr Baker in his paper, "On the Actual Pressure of Earthwork," already referred to. The example in question is not only interesting in itself, but has the additional advantage of having been selected by Mr Flamant for an application of Professor Boussinesq's formulæ.

Mr Baker says—"When the wood paving was recently laid in Regent Street, the space being limited, the stacked wooden blocks in many cases had to do duty as retaining walls to hold up the broken stone ballast required for the concrete substructure. In one instance (Example I.) the author noted that a wall of pitch pine blocks, 4 feet high and 1 foot thick, sustained the vertical face of a bank of old macadam materials which had been broken up, screened, and tossed against this wall, until the bank had attained a height of 3 feet 9 inches, a width at the top of about 5 feet, and slopes on the farther sides, deviating little from 1·2 to 1" (*Proc. Inst. C.E.*, vol. lxv. p. 145). [1·2 to 1 corresponds to an angle of repose, 39° 48′].

Now, Rankine's hypothesis amounts to putting $\beta = 0$ in the notation of the present paper. Putting therefore in (17)

$\beta = 0.$

$h_1 = 3{\cdot}75.$

$\rho_e = 101$ lbs. per cub. foot. [Mr Baker.]

$\rho_m = 46 \times \dfrac{4}{3{\cdot}75} = 49$ lbs. per cub. foot. [Mr Baker.]

there is found

$$\frac{b}{h_1} = {\cdot}386 \quad \therefore\ b = 1{\cdot}448 \text{ ft.}$$

But actually the wall was only 1 foot thick; whereas, as has just appeared, according to Rankine's formula it would just have been on the point of overturning had it been 1·448 feet thick.

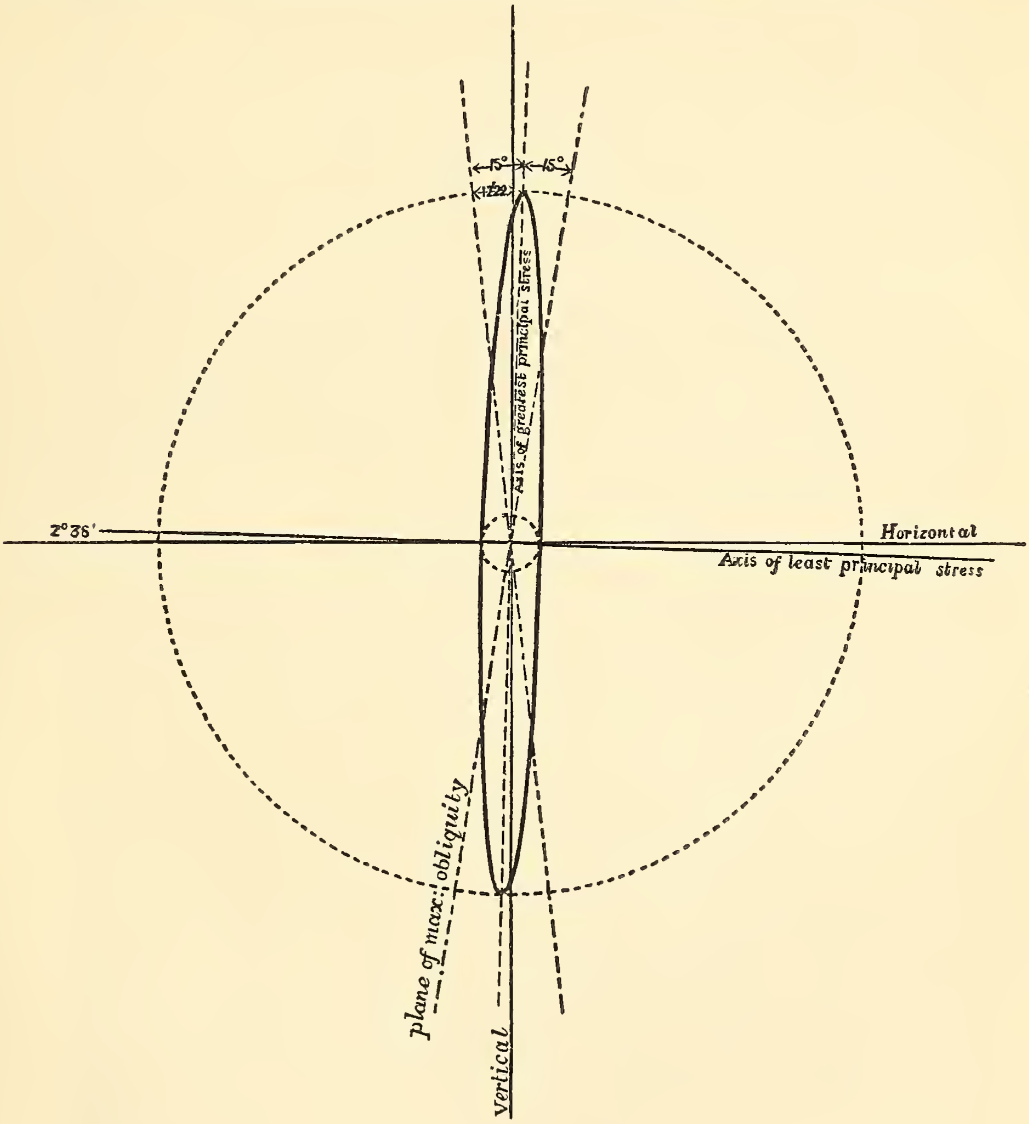


Fig. 2.

According to the view adopted in this paper β will assume a value such that the overturning couple will be a minimum. Taking 39° 48′, as within the errors of observation 40°, an examination of

$\phi \rightarrow$	0°		5°		10°		15°		20°		25°		30°		35°		40°		45°	
$\beta \downarrow$	s	b/h	s	b/h	s	b/h	s	b/h	s	b/h	s	b/h	s	b/h	s	b/h	s	b/h	s	b/h
0°	1	·577	·840	·529	·704	·484	·589	·443	·490	·404	·406	·368	·333	·333	·271	·300	·217	·269	·172	·239
5°			·989	·532	·730	·461	·602	·429	·498	·385	·410	·351	·336	·320	·273	·289	·219	·260	·173	·232
10°					·956	·481	·651	·410	·522	·366	·425	·338	·345	·308	·279	·279	·223	·252	·175	·225
15°							·905	·435	·578	·363	·454	·328	·363	·298	·291	·271	·231	·243	·181	·219
20°									·841	·389	·512	·321	·394	·290	·309	·264	·243	·237	·189	·213
25°											·768	·346	·450	·286	·339	·256	·261	·231	·200	·207
30°													·693	·306	·392	·252	·289	·225	·218	·202
35°															·616	·270	·338	·222	·243	·197
40°																	·542	·236	·287	·196
45°																			·471	·206

the appended table shows that for $\rho_e = \rho_m$ this occurs when β is about 35°. No great error will be introduced by assuming that β has this value for the case in point. Taking the corresponding value of s from the table, and substituting in (17),

$$\frac{b}{h} = \cdot 2799 \text{ or } b = 1\cdot 05 \text{ ft.}$$

Professor Boussinesq's expressions gave b from 10 to $11\frac{1}{2}$ inches.

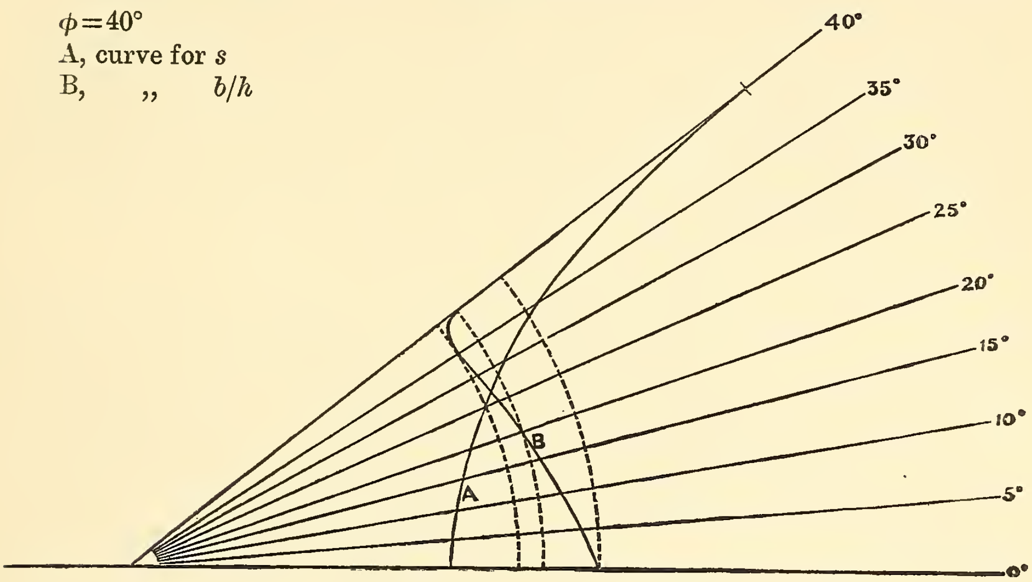


Fig. 3.

2. The Conducting Paths between the Cortex of the Cerebrum and the Lower Centres, in relation to their Function. By Professor D. J. Hamilton.

3. Researches on Micro-Organisms, including ideas of a New Method for their Destruction in Certain Cases of Contagious Disease. By Dr A. B. Griffiths, F.R.S. (Edin.), F.C.S. (Lond. & Paris), Principal, and Lecturer on Chemistry and Biology, School of Science, Lincoln; late Lecturer on Chemistry, Technical School, Manchester, &c.

The opinion of the most profound workers in the so-called "germ diseases" is, that these diseases are really due to the pathological effects of chemical substances (ptomaïnes) elaborated and secreted by certain micro-organisms. That is, a given contagious

disease is rather the result of one or more compounds formed by the life-history of a micro-organism, than the mere presence of that micro-organism itself. By looking at certain cases in the above light, we can well understand why persons suffering with cholera die so rapidly. The alkaloid or ptomaïne (discovered by Pouchet in 1885) which the *Comma bacillus* secretes or forms is rapidly absorbed into the blood, long before the bacillus itself is capable of being absorbed by the mucous membrane of the intestine, and then into the blood. It is a well-known fact that most micro-organisms multiply with great rapidity in the media in which they live, and if particular organisms can be destroyed, the harmful effects of the products produced by their life-histories will *not* increase, and the disease will soon be at an end.

Of course, I am fully aware that it must *not* be supposed that because we find in the blood and tissues of man and animals (suffering from a contagious disease) certain micro-organisms, that these micro-organisms are necessarily the cause, or even indirectly the cause, of that disease. Not until we have obtained by pure cultivations, in an artificial sterilised medium, the organisms in a perfectly pure state, and then, by an injection into the blood of man or an animal of the purified organisms, the disease is reproduced, can we say that a particular disease is the result of the life-history of any particular micro-organism.

It is not my object here to describe the methods adopted by physiological chemists to obtain pure cultivations of any given micro-organism, although no interpretations can be given of any experiments unless the experimenter has worked with purified organisms obtained by artificial cultivation with all its precautions.

I wish to detail what appears to my mind the most reasonable method for the treatment of those contagious diseases whose "seat of war" is in the *blood* itself. I have already had the honour of presenting to the Royal Society of Edinburgh a paper "On the Action of Salicylic Acid on Ferments" (*Proc. Roy. Soc. Edin.*, No. 121, pp. 527–530). It was shown in that paper that an aqueous solution of salicylic acid (0·2 grm. of the acid in 1000 c.c. of water) was capable of destroying such micro-organisms as *Mycoderma aceti*, *Bacterium lactis*, and *Bacillus butyricus* (*B. amylobacter*). It was found, on a close microscopical

examination with high powers, that the salicylic acid solution had acted upon the form of cellulose forming the external wall of these lowly organisms, perforating it, and ultimately destroying the life of the organisms.

During the last nine months I have turned my attention to the action of this salicylic acid solution upon other living micro-organisms.

First of all, I may state that when the above quantity of salicylic acid was added to 1000 c.c. of sterilised wort, and then pure cultivations of *Mycoderma aceti* were introduced into the wort contained in an ordinary two-necked Pasteur's flask (fig. 1), no change took place in it, not even after a month had elapsed, and the vessel kept all the time at the most suitable temperature

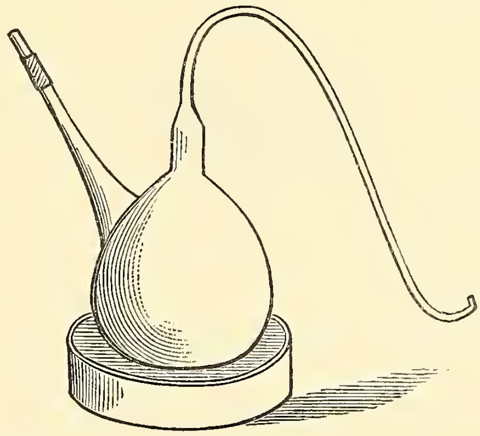


FIG. 1.—Pasteur's Two-Necked Flask.

(32° to 38° C.) for the life-history of the organism. After a month had passed, a small quantity of this wort was transferred (with all physiological caution) into a second Pasteur's flask containing sterilised wort *minus* the salicylic acid. No change was observed in the least, not after the elapse of a month. I experimented in a like manner with *Bacillus butyricus*, *Bacterium lactis*, and obtained similar results. From these experiments, and my previous microscopical studies, I draw the conclusion that the salicylic acid solution is an antiseptic agent destroying these low micro-organisms. In the case of *Bacillus butyricus*, I found that the said salicylic acid solution not only destroyed the organism, but its spores as well. My friend, Mr W. L. Gadd, F.C.S. (Principal Chemical Assistant to Dr W. Thomson, F.R.S.E., of the Royal Institution, Manchester), writes me that recently he has examined "*a sample of*

damaged ginger beer which was quite thick and jelly-like." This injury was caused "*by a minute ferment which can easily be destroyed by salicylic acid*" (Gadd).

1. *Micrococcus prodigiosus.*

The spherical micro-organism (*Micrococcus prodigiosus*) which has been found upon cooked and raw meat kept in the dark, was found on transplanting into 1000 c.c. of sterilised beef-broth (neutral) to grow well in the dark, and formed a thick pellicle on the surface of the liquid. On the addition of 0·2 grm. of salicylic acid to the broth, the growth ceased; and on transferring a portion of this broth into another flask containing sterilised broth, no

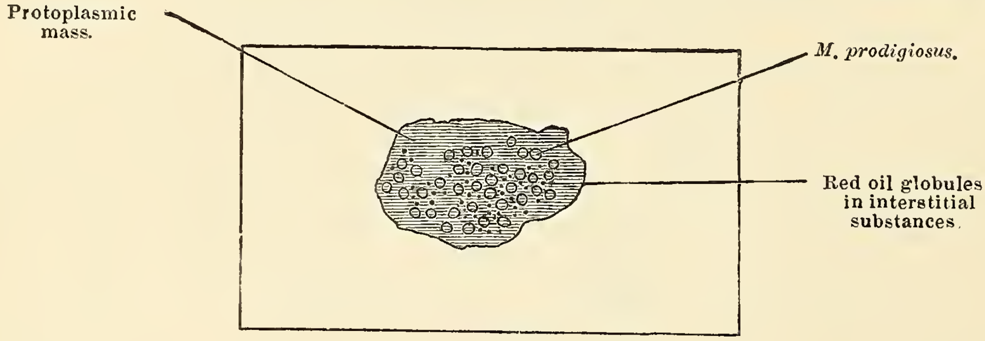


FIG. 2.—*M. prodigiosus* (zooglœa state) (much enlarged).

development took place, not after remaining in this fluid at a temperature of about 34 C. for two weeks. I may say in passing that I have found that the red pigment produced by this "chromogenic micrococcus" contains some compound of iron. On treating these organisms upon a slide under the microscope, with a weak solution of potassium sulphocyanate, a deeper red colour was produced, no doubt due to the formation of ferric sulphocyanate. Again, when a solution of potassium ferrocyanide was run between the slide and cover-slip containing *Micrococcus prodigiosus*, a blue coloration was obtained of "Prussian blue." It appears from the above that the colour produced by this micro-organism is some iron compound.

2. *Micrococcus aurantiacus and Bacterium aeruginosum.*

I have also found that the salicylic acid solution proved fatal to *Micrococcus aurantiacus* and *Bacterium aeruginosum*, the micro-organisms found upon badly-made bread, that has been kept in a

damp place for three weeks. The first micro-organism produces an orange colour, and the second a green colour upon bread. These micro-organisms grow well in flour-paste at a temperature of about 34° C., forming a coloured skin upon the surface of the paste. If the paste is treated, first of all, with salicylic acid, and then *M. aurantiacus* and *B. aeruginosum* transplanted to the paste, they do *not* produce any growth, and ultimately die. The salicylic acid solution destroys the spores of these two micro-organisms, although their spores are said to withstand a temperature of 125° C.

3. *Micrococci found in Diarrhœa.*

The micrococci found largely in secretions from the bowels of persons suffering with diarrhœa were transplanted into sterilised beef-broth contained in a Pasteur flask (No. 1), and kept at 40° C. They multiplied rapidly during two days. Another flask (No. 2), containing sterilised beef-broth and salicylic acid (in the proportion of 0·2 grm. of the acid in 1000 c.c. of the broth), was taken and inoculated with these actively growing micrococci, but their growth was stopped. On inoculating another flask (No. 3) with some of the contents of flask (No. 2), and keeping it about 40° C. for three weeks, no further growth took place. This proves that the salicylic acid had killed the micrococci in question. I am fully aware that it has not been thoroughly ascertained whether this micrococcus is the cause of diarrhœa, yet, for my purpose, the above is to show that the said salicylic acid destroys this micrococcus.

4. *Leptothrix buccalis and Bacillus subtilis.*

These non-pathogenic forms found in the healthy human mouth are also destroyed by salicylic acid. When examined under the microscope, there was the appearance of the solution of salicylic acid perforating the cellulose wall of *Bacillus subtilis*, and so destroying the organism (fig. 3, B). At first we notice the cell-wall becomes thinner on the side that the salicylic acid solution is being run in between the slide and cover-slip, and is then perforated.

5. *Penicilium glaucum.*

Penicilium glaucum (the mould of preserved fruits, old leather, &c.). When mounted in a drop of water on the slide under the

microscope, it was observed that on running in between the slide and cover-slip the solution of salicylic acid, the cellulose walls of hyphæ, conidia, conidiophores were all perforated, and ultimately dissolved by the acid.

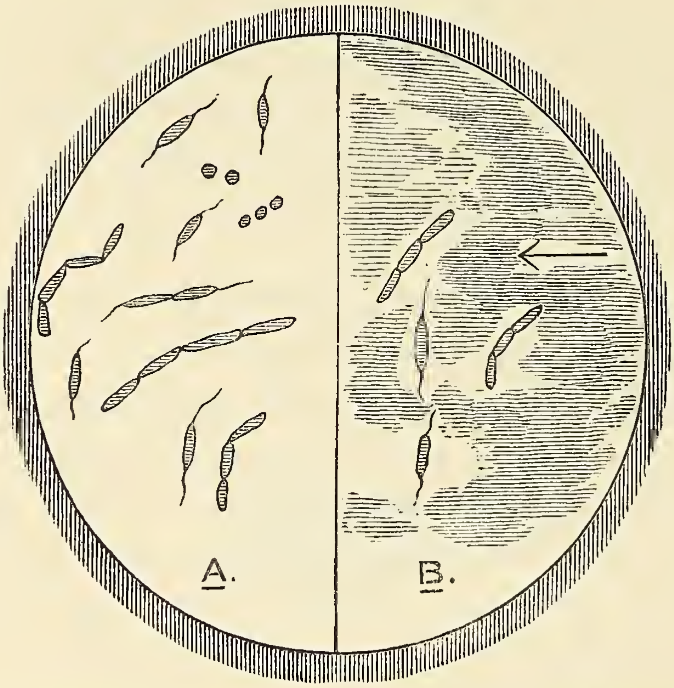


FIG. 3.—A, *Bacillus subtilis* (much enlarged). B, ←= Direction of flow of salicylic acid solution.

N.B.—In fig. 3 B the cell-walls should be thinner (than in woodcut) and perforated, especially on the observer's right.

6. *Vaccine Lymph.*

The micrococcus vacciniæ, which is the active principle of the vaccine virus (as shown by Pasteur, and not the fluid medium in which these micrococci live their life-histories), is also acted upon by salicylic acid, for the lymph so treated loses its power of inoculation.

7. *Micrococcus ureæ* (Von Tieghem).

Micrococcus ureæ, or the zymogenic ferment of urine which splits up urea into ammonium carbonate, is found in dumb-bells, chains, and in the zooglœa state. If urine is allowed to stand until it begins to smell of ammonia, and then a drop of this urine examined microscopically, the *Micrococcus ureæ* will be found to be present. If now fresh urine (sterilised) is treated with salicylic acid, and then inoculated with *Micrococcus ureæ* (from the putrid urine), no change at all occurs in the urine, not after the elapse of

sixteen days. Therefore, I conclude that the said acid has acted upon this organism in a similar manner to those already mentioned.

8. *Protococcus vulgaris and Protococcus pluvialis* (fig. 4).

These two species of Protococcus are *not* acted upon by the solution of salicylic acid, and a much stronger solution of the acid has *no* effect upon them. It appears that there is a difference in the

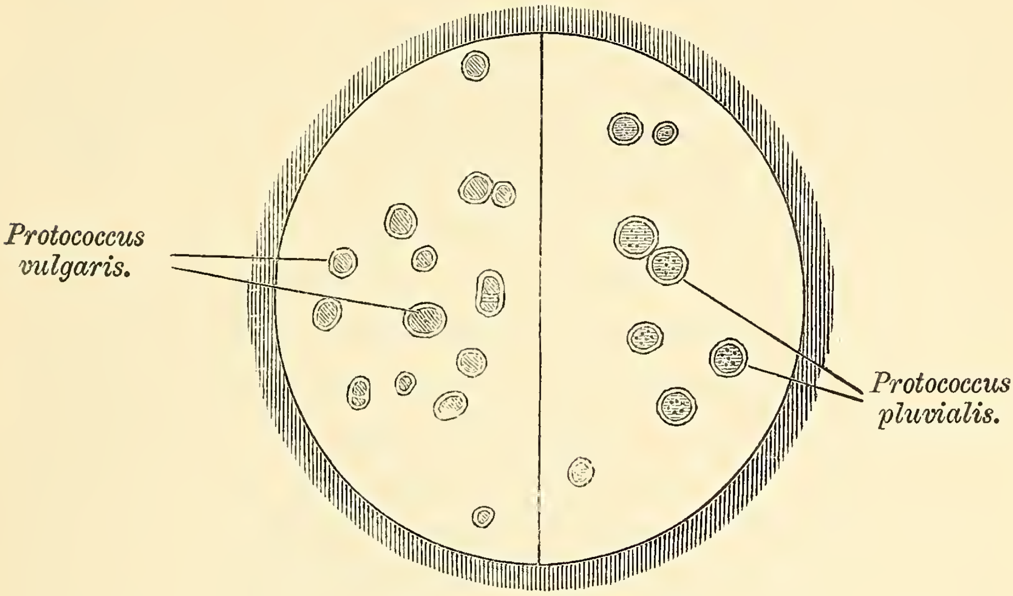


Fig. 4.

atomic structure of the cellulose forming the cell-walls of these two species of Protococcus and the various micro-organisms alluded to in this paper; that is, the cellulose of Protococcus is more like the cellulose of the higher forms of plant life, which is not acted upon by the acid in question.

So far, we have seen that salicylic is an antiseptic agent, capable of attacking or acting upon their cellulose walls.

This acid, and acids generally, appear adverse to the life-histories of certain micro-organisms. It will be remembered that M. Bochefontaine swallowed secretions from choleraic patients containing *Comma bacilli*, made up in the form of pills, without any serious consequences. There is no doubt the *acid* properties of the gastric juice in the stomach had acted upon these micro-organisms in some way or other, and so prevented them living their life-histories in the intestine.

The practical outcome of this piece of research (although far from

completed) may prove a remedy (placed upon a scientific basis) for certain contagious diseases whose organisms *reside in the blood.*

I may allude, in passing, that there is no science on such an unsatisfactory basis as medicine, and the present method of administering medicines (in such diseases as above) is first of all into the stomach by means of the mouth. The medicine has to pass through a good portion of the alimentary canal before it is absorbed into the blood. The medicine may become changed (before it is absorbed) by the various secretions pouring into the alimentary canal. Hence, for those *contagious* diseases whose seat of action is in the blood (the blood being their soil), we ought to apply our remedy directly to the blood itself by *injections.*

With this idea in view (and from analogy of the action of salicylic acid upon a large number of micro-organisms), it is most probable that the above salicylic acid solution will destroy *Micrococcus erysipelatosus*, *Micrococcus scarlatinæ*, *Bacillus malariæ*, *Bacillus tuberculosis*, &c. These micro-organisms have been proved to be the cause of the diseases in which they are found in great numbers in the blood. Swine fever, cattle plague, pleuro-pneumonia have also been proved to be the results of different species of micrococcus. All these organisms reside in the blood of man and animals suffering with these diseases.

In the case of *Bacillus tuberculosis*, according to Dr E. Freund (*Nature*, vol. xxxiv. p. 581, Oct. 14, 1886), of Vienna, this micro-organism appears to form cellulose within the blood of tuberculous persons. This cellulose is a product of its (*B. tuberculosis*) life-history, and we have seen in numerous cases that the salicylic acid attacks micro-parasitic cellulose, if I can so use the expression. Therefore the hint may be given, that salicylic acid may prove a successful remedy in tuberculosis, when injected directly into the blood of diseased patients.

It may be asked—If the salicylic acid solution is injected into the blood, will it not destroy the red and white corpuscles? On April 28, 1886, I opened a vein in my left arm, and injected into the blood the solution of salicylic acid, and with the exception of a headache or so there were no abnormal results.

A microscopical examination of the blood (two hours after injection) revealed that the blood corpuscles were in a perfectly

healthy state. The salicylic acid solution had *no* action upon the corpuscles of blood of a poor quality. Hence, I have reason to conclude that it may be, with a more extended study of the action of this solution of salicylic acid upon disease "germs" and their organisms, we have the most rational mode of treating those contagious diseases whose seat of energy is in the blood.

Note to above Paper.

The oven for sterilising tubes, cotton-wool, &c., I use in my experiments, was devised by my wife, and is seen in fig. 5. A good point of this oven is, that the shelf contains a series of holes

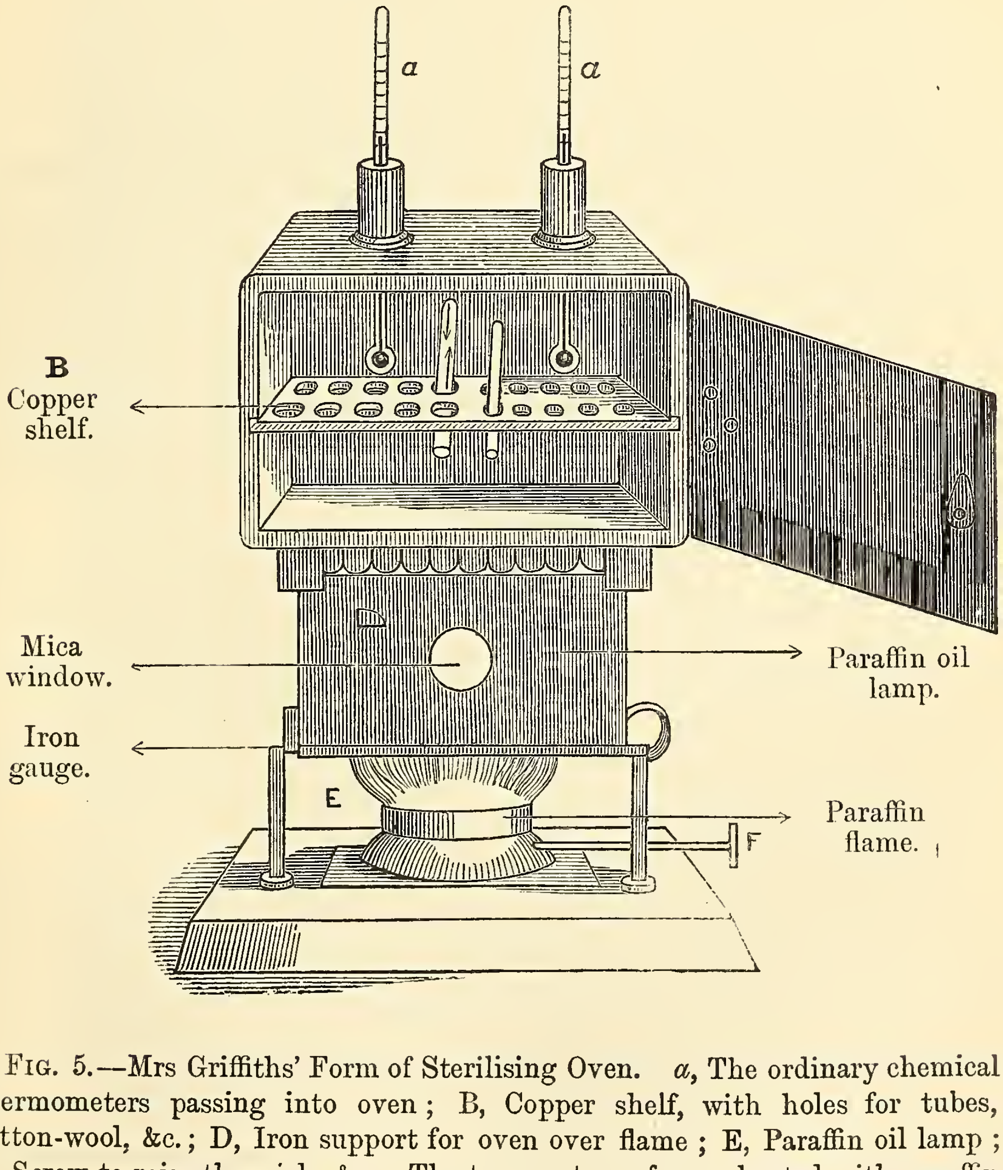


FIG. 5.—Mrs Griffiths' Form of Sterilising Oven. *a*, The ordinary chemical thermometers passing into oven; B, Copper shelf, with holes for tubes, cotton-wool, &c.; D, Iron support for oven over flame; E, Paraffin oil lamp; F, Screw to raise the wick, &c. The temperature of oven heated with paraffin oil can go as high as 155° C., and with gas, above 300° C.

of various sizes for test-tubes. By placing the test-tubes in inverted positions (as in fig.), the heated air rises in the tubes, and a current of air in each tube is formed, and thus destroys all organisms and their spores, for they are detached by these currents (of heated air rising), and exposed on all sides to the full heat of the oven.

Monday, 7th February 1887.

The HON. LORD M'LAREN, Vice-President, in the Chair.

1. On Cases of Instability in Open Structures. By E. Sang, LL.D.

(Abstract.)

In the course of some remarks on the design proposed for the Forth Bridge, the author of this paper had enunciated the remarkable theorem, that any symmetric structure built on a rectangular base, and depending on linear resistance alone, is necessarily unstable. The proof of it, given in the eleventh volume of the *Transactions of the Royal Scottish Society of Arts*, is derived from considerations affecting the special case; but this theorem is only one of an extensive class, and therefore the subject of instability among linear structures in general is here taken up.

In the case of regular or semi-regular arrangements, having the corners of an upper supported from the corners of an under polygon, it is shown that when the figures are of odd numbers the structures are stable, while those with even numbers are unstable; unless indeed the polygons be placed conformably, in which case the stability extends to both classes.

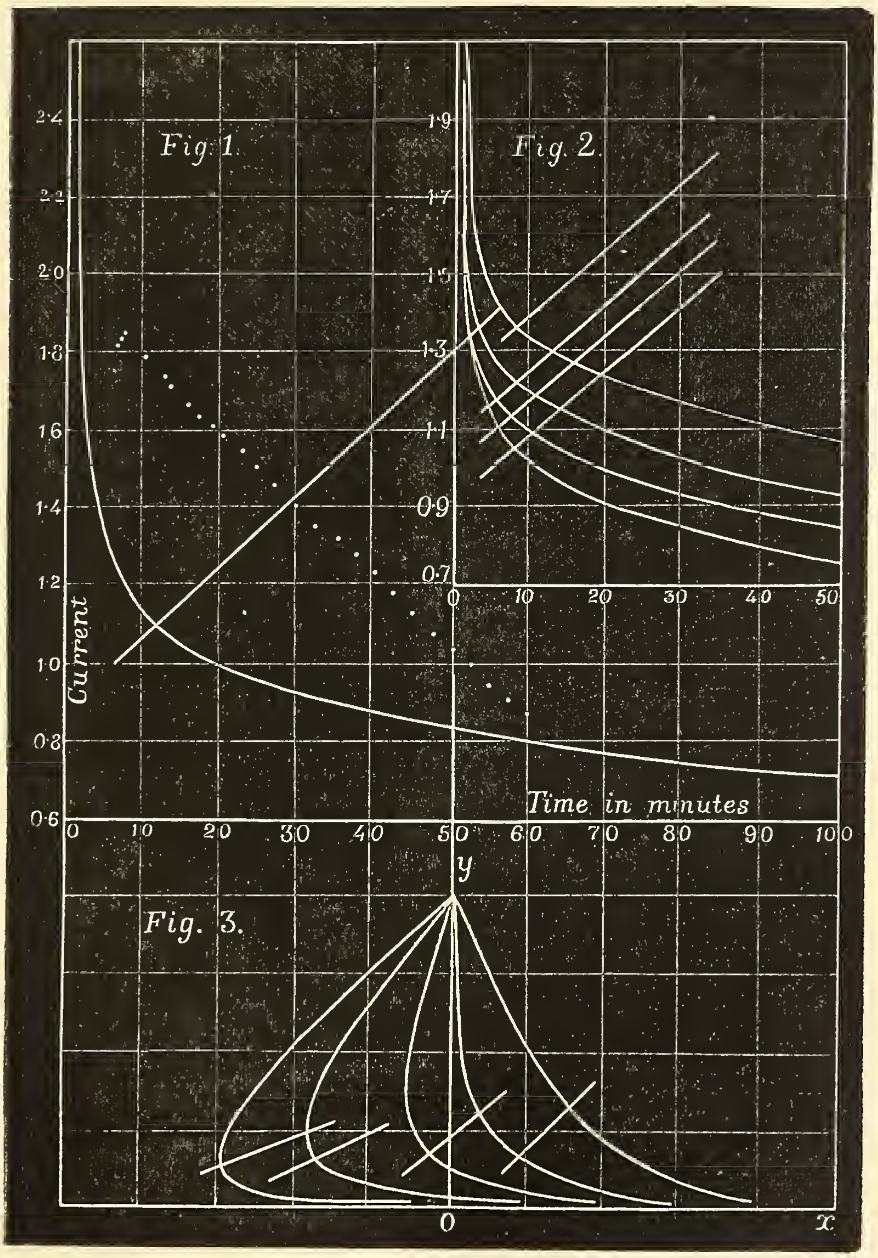
The paper ends with the following admonition:—These cases of instability in open structures have been elicited by means of the simplest considerations in geometry and statics; they lie indeed on the very surface of mechanical inquiry. They do not occur as isolated examples, but are arranged in extensive groups; and, being found in those classes of structures which may be called shapely, they stand out as warning beacons to those engaged in engineering pursuits.

2. On the Increase of Electrolytic Polarization with Time. By W. Peddie, Esq. Communicated by Professor Tait.

In a paper communicated to this Society last Session I described some preliminary experiments on this subject, which led to the approximate empirical formula connecting current-strength and time

$$i = a + b\epsilon^{-ct + d\epsilon^{-et}}$$

where a, b, c, d, and e are constants of positive sign. The electrodes



used were of platinum, and had a surface area of about 5 sq. cm. Subsequently I have used platinum electrodes having an area of

rather more than 60 sq. cm. The curves I obtained evidently possess, to a considerable extent, symmetry about an axis. They roughly resemble hyperbolas, but have rather more curvature near the vertex. A few of them are represented in fig. 2. Time is measured horizontally, each scale-division representing $6\frac{1}{4}$ minutes. The numbers on the vertical scale, when multiplied by 0·267, give the current-strength in ampères. I used in each experiment a battery of 3 tray-Daniell cells, the electromotive force of which remained constant during the experiment. The battery was coupled in circuit with a Helmholtz tangent galvanometer and the electrolytic cell. The reduction factor of the galvanometer is 0·267. The needle of the galvanometer ceased vibrating in about half a minute after completing the circuit, at which time the first reading was taken. The following tables give the details of one experiment:—

Deflection in Degrees at intervals of One Quarter-Minute.

64·8, 64·4, 63·5, 62·7, 61·8, 60·9, 59·7, 58·8, 58·2, 58, 57·8, 57·4, 57, 56·7, 56·2, 55·7, 55·2, 54·6, 54, 53·6, 53·1, 52·7, 52·4, 51·9, 51·7, 51·4, 51·1, 50·9, 50·5, 50·3, 50, 49·8, 49·6, 49·4, 49·2, 48·9, 48·8, 48·6, 48·4, 48·3.

At intervals of One Half-Minute.

47·9, 47·6, 47·3, 47·1, 46·8, 46·6, 46·4, 46·2, 45·9, 45·8, 45·7, 45·6, 45·5, 45·3, 45·2, 45·1, 44·9, 44·8, 44·7, 44·5, 44·4, 44·3.

At intervals of One Minute.

44·1, 43·9, 43·7, 43·6, 43·4, 43·2, 43·1, 42·8, 42·7, 42·6, 42·5, 42·3, 42·2, 42·1.

At intervals of Three Minutes.

41·6, 41·1, 40·8, 40·3, 39·9.

At intervals of Five Minutes.

39·4, 38·7, 38·4, 37·8, 37·4, 36·9, 36·6, 36·4, 36, 35·7, 35·4, 35·1, 34·8, 34·5.

These results are shown graphically in the curve in fig. 1. In each experiment the total resistance in the circuit was slightly different. A special feature of the group of curves in fig. 2 is the close parallelism of the axes.

I have adopted, as a better approximation to the law connecting current-strength and time, the equation

$$i = a + b\epsilon^{ci - kt} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1).$$

The curve represented by this equation does not differ much from that represented by the equation formerly given. It possesses to a certain extent symmetry about an axis. In fig. 3 I have represented a group of curves having this equation, passing through a given point on the axis of i, and having a common asymptote. The curves which pass to the left of the axis of i cannot, of course, represent the physical phenomena, but they show the axial symmetry well. If another group is drawn, the axis of i being a tangent, and each member having a different horizontal asymptote, a close resemblance between it and the group in fig. 2 is evident.

If equation (1) holds, we have

$$\log (i - a) - ci = \log b - kt \quad . \quad . \quad . \quad . \quad (2).$$

Hence the quantity on the left-hand side of this equation plotted against time should give a straight line. The group of points in fig. 1 is obtained in this way from the curve in that figure. The near coincidence of all the points with a straight line shows that the curve is closely represented by an equation of the form (1).

I have also made an experiment with only one tray-Daniell cell in the circuit. There was, consequently, no decomposition of the electrolyte (an aqueous solution of sulphuric acid). In this case the constant a must be zero. The equation

$$i = 0{\cdot}4168\epsilon^{1{\cdot}092i - 0{\cdot}4015t}$$

represents the results of observation almost exactly, the difference being well within the limit of possible errors in drawing the curve free-hand through the observed points. For small values of t there is considerable discrepancy between the results of observation and calculation in both experiments, so that the formula (1) must not be applied when t is below a certain limit, which, for the results tabulated above, is about 5 minutes.

3. Astronomical Notes. By Ralph Copeland, Esq., Ph.D. Communicated by Lord M'Laren.

PRIVATE BUSINESS.

Dr J. B. Buist, Mr A. B. Brown, C.E., Mr Ferdinand Faithful Begg, Mr J. Arthur Thomson, Dr Andrew Thomson, Dundee; Mr William Caldwell Crawford, Mr J. G. Bartholomew, Dr William Hunter, Mr Thomas Goodall Nasmyth, Sir John Fowler, M. Inst. C.E., Mr W. H. Barlow, M. Inst. C.E., and Mr John Muter were balloted for, and declared duly elected Fellows of the Society.

Monday, 21st February 1887.

REV. PROFESSOR FLINT, D.D., Vice-President, in the Chair.

1. Further Determinations of the Effect of Pressure on the Maximum Density Point of Water. By Professor Tait.

2. On the Height of the Land of the Globe above Sea-Level. By Dr J. Murray.

3. Note on the Effects of Explosives. By Professor Tait.

(*Abstract.*)

Many of the victims of the dynamite explosion, a year or two ago, in the London Underground Railway, are said to have lost the drum of one ear only, that nearest to the source. This seems to point to a projectile, not an undulatory, motion of the air and of the gases produced by the explosion. So long, in fact, as the disturbance travels faster than sound, it must necessarily be of this character, and would be capable of producing such effects.

Another curious fact apparently connected with the above is the (considerable) finite diameter of a flash of forked lightning. Such

a flash is always photographed as a line of finite breadth, even when the focal length is short and the focal adjustment perfect. This cannot be ascribed to irradiation. The air seems, in fact, to be driven outwards from the track of the discharge with such speed as to render the immediately surrounding air instantaneously self-luminous by compression.

Such considerations show at once how to explain the difference between the effects of dynamite and those of gunpowder. The latter is prepared expressly for the purpose of developing its energy gradually. Thus while the flash of gunpowder fired in the open is due mainly to combustion of scattered particles,—that produced by dynamite is mainly due to impulsive compression of the surrounding air, energy being conveyed to it much faster than it can escape in the form of sound.

4. Report on Fossil Fishes collected in Eskdale and Liddesdale. Part I. Ganoidei—Supplement. By Dr Traquair.

5. On the Equilibrium of a Gas under its own Gravitation only. By Sir W. Thomson.*

This problem, for the case of uniform temperature, was first, I believe, proposed by Tait in the following highly interesting question, set in the Ferguson Scholarship Examination (Glasgow, October 2, 1885):—"Assuming Boyle's Law for all pressures, form the equation for the equilibrium-density at any distance from

* *Note of February* 22, 1887.—Having yesterday sent a finally revised proof of this paper for press, I have to-day received a letter from Prof. Newcomb, calling my attention to a most important paper by Mr J. Homer Lane, "On the Theoretical Temperature of the Sun," published in the *American Journal of Science* for July 1870, p. 57, in which precisely the same problem as that of my article is very powerfully dealt with, mathematically and practically. It is impossible now, before going to press, for me to do more than refer to Mr Lane's paper; but I hope to profit by it very much in the continuation of my present work which I intended, and still intend, to make.—W. T.

the centre of a spherical attracting mass, placed in an infinite space filled originally with air; Find the special integral which depends on a power of the distance from the centre of the sphere alone."

The answer (in examinational style !) is :—Choose units properly; we have

$$\frac{d\rho}{dr} = -\frac{\rho \int_0^r \rho r^2 dr}{r^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1),$$

where ρ is the density at distance r from the centre. Assume

$$\rho = Ar^{\kappa} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2).$$

We find $A = 2$, $\kappa = -2$; and therefore

$$\rho = \frac{2}{r^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

satisfies the equation in the required form.

Tait informs me that this question occurred to him while writing for *Nature* a review of Stokes' Lecture * on Inferences from the Spectrum Analysis of the Lights of Sun, Stars, Nebulæ, and Comets; and in the *Proceedings of the Edinburgh Mathematical Society* he has given some Transformations of the equation of Equilibrium. The same statical problem has recently been forced on myself by considerations which I could not avoid in connection with a lecture which I recently gave in the Royal Institution of London, on "The Probable Origin, the Total Amount, and the Possible Duration of the Sun's Heat."

Helmholtz's explanation, attributing the Sun's heat to condensation under mutual gravitation of all parts of the Sun's mass, becomes not a hypothesis but a statement of fact, when it is admitted that no considerable part of the heat emitted from the Sun is produced by present in-fall of meteoric matter from without. The present communication is an instalment towards the gaseous dynamics of the Sun, Stars, and Nebulæ.

To facilitate calculation of practical results, let a kilometre be the unit of length; and the terrestrial-surface heaviness of a cubic kilometre of water at unit density taken as the maximum density, under ordinary pressure, be the unit of force (or approximately, a thousand million tons heaviness at the earth's surface). If p be the

* Lecture III. of Second Course of "Burnet Lectures," Aberdeen, Dec. 1884; published, London, 1885 (Macmillan).

pressure, ρ the density, and t the temperature from absolute zero, we have, by Boyle and Charles's laws,

$$p = \mathrm{H}\rho t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4);$$

where t denotes absolute (thermodynamic *) temperature, with 0° C. taken as unit; and H denotes what is commonly, in technical language, called "the height of the homogeneous atmosphere" at 0° C. For dry common air, according to Regnault's determination of density,

$$\mathrm{H} = 7{\cdot}985 \text{ kilometres} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4').$$

Let β be the gravitational coefficient proper to the units chosen; so that $\beta mm'/\mathrm{D}^2$ is the force between m, m' at distance D. The earth's mean density being 5·6, and radius 6370 kilometres, we have

$$\frac{4\pi}{3} . 6370 . 5{\cdot}6\beta = 1; \text{ and therefore } 4\pi\beta = 1/11890 \quad . \quad . \quad . \quad . \quad (5).$$

Let now the p, ρ, t of (4) be the pressure, density, and temperature at distance r from the centre of a spherical shell containing gas in gross-dynamic † equilibrium. We have, by elementary hydrostatics

$$\frac{dp}{dr} = -\rho\left(\mathrm{M} + \int_a^r dr 4\pi r^2 \rho\right)\beta/r^2 \quad . \quad . \quad . \quad . \quad (6),$$

whence

$$\frac{d}{dr}\left(\frac{r^2}{\rho}\frac{dp}{dr}\right) = -4\pi\beta r^2 \rho \quad . \quad . \quad . \quad . \quad . \quad . \quad (7),$$

where M denotes the whole quantity of matter within raidius a from the centre; which may be a nucleus and gas, or may be all gas.

If the gas is enclosed in a rigid spherical shell, impermeable to heat, and left to itself for a sufficiently long time, it settles into the condition of gross-thermal equilibrium, by "conduction of heat," till the temperature becomes uniform throughout. But if it were stirred

* The notation of the text is related to temperature Centigrade on the thermodynamic principle (which is approximately temperature Centigrade by the air-thermometer), as follows :—

$$= \frac{1}{273}(\text{temperature Centigrade} + 273);$$

see my Collected Mathematical and Physical Papers, vol. i. arts. xxxix. and xlviii. part vi. §§ 99, 100; and article "Heat," §§ 35–38 and 47–67, *Encyc. Brit.*, and vol. iii. (soon to be published) of Collected Papers.

† Not in molecular equilibrium of course; and not in gross-thermal equilibrium, except in the case of t uniform throughout the gas.

artificially all through its volume, currents not considerably disturbing the static distribution of pressure and density will bring it approximately to what I have called convective equilibrium * of temperature, that is to say, the condition in which the temperature in any part P is the same as that which any other part of the gas would acquire if enclosed in an impermeable cylinder with piston, and dilated or expanded to the same density as P. The *natural stirring* produced in a great free fluid mass like the Sun's, by the cooling at the surface, must, I believe, maintain a somewhat close approximation to convective equilibrium throughout the whole mass. The known relations between temperature, pressure, and density for the ideal "perfect gas," when condensed or allowed to expand in a cylinder and piston of material impermeable to heat, are †

$$p = \mathrm{HT}\rho^k \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8),$$

$$t = \mathrm{T}\rho^{k-1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9);$$

where k denotes the ratio of the thermal capacity of the gas, pressure constant, to its thermal capacity, volume constant, which is approximately equal to 1·41 or 1·40 (we shall take it 1·4) for all gases, and all temperatures, densities, and pressures; and T denotes the temperature corresponding to unit density in the particular gaseous mass under consideration.

Using (8) to eliminate p from (7) we find

$$\frac{d}{dr}\left[r^2 \frac{d(\rho^{k-1})}{dr}\right] = -\frac{4\pi\beta(k-1)}{\mathrm{HT}k} r^2 \rho \quad . \quad . \quad . \quad . \quad . \quad . \quad (10),$$

which, if we put

$$\rho^{k-1} = u \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11),$$

$$1/(k-1) = \kappa \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12),$$

and

$$r^{-1}\sqrt{\frac{\mathrm{HT}k}{4\pi\beta(k-1)}} = x \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13),$$

takes the remarkably simple form

$$\frac{d^2u}{dx^2} = -\frac{u^\kappa}{x^4} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14).$$

* See "On the Convective Equilibrium of Temperature in the Atmosphere," *Manchester Phil. Soc.*, vol. ii., 3rd series, 1861; and vol. iii. of Collected Papers.

† See my Collected Mathematical and Physical Papers, vol. i. art. xlviii. note 3.

Let $f(x)$ be a particular solution of this equation; so that

and therefore
$$\left.\begin{aligned} f''(x) &= -[f(x)]^{\kappa}x^{-4} \\ f'(mx) &= -[f(mx)]^{\kappa}m^{-4}x^{-4} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (15).$$

From this we derive a general solution with one disposable constant, by assuming

$$u = Cf(mx) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16);$$

which, substituted in (14), yields in virtue of (15),

$$m^2 = C^{-\kappa+1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17);$$

so that we have, as a general solution,

$$u = Cf[xC^{-\frac{1}{2}(\kappa-\,)}] \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18).$$

Now the class of solutions of (14) which will interest us most is that for which the density and temperature are finite and continuous from the centre outwards to a certain distance, finite as w shall see presently, at which both vanish. In this class of cases u increases from 0 to some infinite value, as x increases from some finite value to ∞. Hence if $u = f(x)$ belongs to this class, $u = Cf(mx)$ also belongs to it; and (18) is the general solution for the class. We have therefore, immediately, the following conclusions :—

(1) The diameters of different globular* gaseous stars of the same kind of gas are inversely as the $\frac{1}{2}(\kappa - 1)$th powers (or $\frac{3}{4}$ powers) of their central temperatures, at the times when, in the process of gradual cooling, their temperatures at places of the same densities are equal (or "T" the same for the different masses). Thus, for example, one sixteenth central temperature corresponds to eight-fold diameter; one eighty-first central temperature corresponds to twenty-seven fold diameter.

(2) Under the same conditions as (1) (that is, H and T the same for the different masses), the central densities are as the κth powers

* This adjective excludes stars or nebulæ *rotating steadily* with so great angular velocities as to be much flattened, or to be annular; also nebulæ revolving circularly with different angular velocities at different distances from the centre, as may be approximately the case with spiral nebulæ. It would approximately enough include the sun, with his small angular velocity of once round in 25 days, were the fluid not too dense through a large part of the interior to approximately obey gaseous law. It no doubt applies very accurately to earlier times of the sun's history, when he was much less dense than he is now.

(or $\frac{5}{2}$ powers) of the central temperatures; and therefore inversely as the $\frac{2\kappa}{\kappa-1}$, or $\frac{2}{2-k}$, or $\frac{10}{3}$, powers of the diameters.

(3) Under still the same conditions as (1) and (2), the quantities of matter in the two masses are inversely as the $\left(\frac{2}{2-k}-3\right)$th powers (inversely as the cube roots) of their diameters.

(4) The diameters of different globular gaseous stars, of the same kind of gas, and of the same central densities, are as the square roots of their central temperatures.

(5) The diameters of different globular gaseous stars of different kinds of gas, but of the same central densities and temperatures, are inversely as the square roots of the specific densities of the gases.

(6) A single curve $[y=f(r^{-1})]$ with scale of ordinate (r) and scale of abscissa (y) properly assigned according to (18), (17), and (11) shows for a globe of any kind of gas in molecular equilibrium, of given mass and given diameter, the absolute temperature at any distance from the centre. Another curve $\{[y=f(r^{-1})]^{\kappa}\}$, with scales correspondingly assigned, shows the distribution of density from surface to centre.

It is easy to find, with any desired degree of accuracy, the particular solution of (13), for which

$$u=\text{A}, \text{ and } \frac{du}{dx}=\text{A}', \text{ where } x=a \quad . \quad . \quad . \quad . \quad (19),$$

a denoting any chosen value of x, and A and A′ any two arbitrary numerics, by successive applications of the formula

$$u_{i+1}=\text{A}-\int_a^x dx\left(\text{A}'-\int_a^x dx\frac{u_i^{\kappa}}{x^4}\right) \quad . \quad . \quad . \quad (20);$$

the quadratures being performed with labour moderately proportional to the accuracy required, by tracing curves on "section"-paper (paper ruled with small squares) and counting the squares and parts of squares in their areas. To begin, u_0 may be taken arbitrarily; but it may conveniently be taken from a hasty graphic construction by drawing, step by step, successive arcs * of a curve with radii of

* This method of graphically integrating a differential equation of the second order, which first occurred to me many years ago as suitable for finding the shapes of particular cases of the capillary surface of revolution, was successfully carried out for me by Prof. John Perry, when a student in my

curvature calculated from (13) with the value of du/dx found from the step-by-step process. If this preliminary construction is done with care, by aid of good drawing-instruments, u_1 calculated from u_0 by quadratures will be found to agree so closely with u_0, that u_0 itself will be seen to be a good solution. If any difference is found between the two, u_1 is the better: u_2 is a closer approximation than u_1; and so on, with no limit to the accuracy attainable.

Mr Magnus Maclean, my official assistant in the University of Glasgow, has made a successful beginning of working out this process for the case $u=16$ where $x=\infty$; and has already obtained a somewhat approximate solution, of which the produce useful for our problem is expressed in the following table :—

Numerical Solution of $\frac{d^2u}{dx^2}+x\ {}^{4}u^{2\cdot5}=0$.

Distance from centre $=r=1/x$.	Reciprocal of distance from centre $=x=1/r$.	Temperature $-u$.	Density $=u^{2\cdot5}$.	Mass within distance r from the centre $=du/dx$ $=\int_x^\infty dxu^{2\cdot5}x_{-\cdot4}$
0	8	16·00	1024	·00
·100	10	14·46	795·2	·28
·111	9	14·14	751·6	·38
·125	8	13·71	695·8	·52
·143	7	13·10	621·2	·731
·167	6	12·20	520·0	1·056
·200	5	10·92	394·1	1·566
·250	4	9·00	243·0	2·336
·333	3	6·15	93·81	3·436
·500	2	2·25	7·595	4·366
·667	1·5	0	0	4·49

The deduction from these numbers, of results expressing in terms of convenient units the temperature and density at any point of a given mass of a known kind of gas, occupying a sphere of given radius, must be reserved for a subsequent communication.

laboratory in 1874, in a series of skilfully executed drawings representing a large variety of cases of the capillary surface of revolution, which have been regularly shown in my Lectures to the Natural Philosophy Class of the University of Glasgow. These curves were recently published in the *Proc. Roy. Instit.* (Lecture of Jan. 29, 1886), and *Nature*, July 22 and 29, and Aug. 19, 1886; also to appear in a volume of Lectures now in the press, to be published in the *Nature* series.

One interesting result which I can give at present, derived from the first and last numbers of the several columns of the preceding table, is, that the central density of a globular gaseous star is $22\frac{1}{2}$ times its average density.

Monday, 7th March 1887.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. On the Equilibrium of a Gas under its own Gravitation only. Part III. By Sir W. Thomson.

2. History of the Theory of Determinants. Part I. Determinants in General: Hindenburg (1784) to Reiss (1829). By Dr Muir.

[This Paper is printed at the end of the *Proceedings* of the Session.]

3. Note on Solar Radiation. By Mr John Aitken.

In the many theories that have been advanced to explain the comparative constancy of solar radiation in long past ages as evidenced by geological history, it has been generally assumed that the temperature of the sun has not varied much, and to account for its not falling in temperature a number of theories have been advanced, all suggesting different sources from which it may have received the energy which it radiates as heat. Since the chemical theory was shown to be insufficient to account for the vast amount of heat radiated, other theories, such as the meteoric theory and the conservation of energy theory, have been advanced.

In all these theories it is generally assumed that in order that the radiation from the sun may be constant its temperature must also be constant, that, in fact, radiation from the sun is in proportion to its temperature; and that if the temperature of the sun falls, its radiation effect at the earth's surface will fall in direct proportion to

its loss of heat. Now there are certain physical facts which have induced me to think that this is not necessarily so. Nay, it seems even possible that the amount of heat radiated by the sun might go on increasing while its temperature was decreasing.

The facts which seem to point to this conclusion are—

First, We know that different forms of matter vary greatly in their power of radiating heat. For instance, a non-luminous gas flame radiates far less heat than a luminous one, though actually at a higher temperature, and a red-hot surface of platinum radiates far less heat than an oxidised surface of iron of the same area and temperature.

Second, We know that elementary bodies generally radiate far less heat than compound ones. It has been shown that the radiating powers of substances go on increasing with the increased complexity of their constitution.

Third, We know that at high temperatures compound bodies are decomposed or broken up into simpler forms. Or, to put it in another way, and as a solar inhabitant would put it, bodies which have an affinity for each other do not combine unless their temperature is below a certain fixed point for the substances.

Combining these statements we see that in the sun, owing to its high temperature, matter must be in much simpler forms than it is on the earth, and all recent investigation points markedly in this direction. It seems therefore in the highest degree probable that the average radiating power of the matter in the sun is much less than that of the matter of the earth. Again, the hotter the sun the simpler its constitution will be, and the weaker its radiating power. From this we see that there is no necessary proportion between the temperature of the sun and the amount of heat it radiates, as change of temperature is accompanied by change of constitution, and increase of radiating power.

These considerations lead us on, and suggest that the store of energy from which the sun has drawn in long past ages may possibly be its own internal supply; that the sun in its earlier ages was at a much higher temperature than it is at present, but, owing to its simpler constitution, it radiated only about as much heat as it does now, and that as its temperature fell its matter became more compound, and its radiating power increased, which enables it now

to keep up about the same radiation effect, though its temperature may have fallen greatly.

It is evident that much of this is purely theoretical and formed on rather a weak basis; we have so few measurements to go by. Though it is a law that compound bodies radiate more than simple ones, yet we do not know enough of the constitution of the sun to say how much the increased radiating power, due to increased complexity, would compensate for the fall of temperature. I have said it might actually more than counteract the fall of temperature and cause an increased radiation effect. It may, however, only balance it, or it may even only reduce the rate of decrease of radiation, and make it only a little less than proportional to the fall in temperature.

The whole of these remarks are almost pure speculation. The principal cause for their being written is to point out that the radiating power of the sun may have varied in quantity and quality from age to age; that its amount may not be directly proportional to its absolute temperature; and further, that it is extremely doubtful whether we can apply to solar matter the radiation measurements which have been obtained of earth matter, so that any estimates we may make of the temperature of the sun from measurements of solar radiation must be received with considerable hesitation.

Added 9*th July* 1887.

Sir William Thomson has shown that the sun has within itself an enormous store of energy due to its high temperature. This energy is altogether apart from what we on the earth are accustomed to consider as the energy stored up in a hot body. He has shown that the shrinkage or falling in of the great mass of the sun due to cooling is capable of developing a very great amount of energy. His calculations show, that if the shrinkage was so great that the surface of the sun was to descend at the rate of 35 metres per year, or 70 kilometres per 2000 years, there would be developed about the same amount of energy that is radiated by the sun according to the measurements taken by Pouillet.

Forbes has shown Pouillet's measurements to be much too small, and that the amount radiated by the sun is 1·6 greater than

Pouillet supposed. Professor Langley, by more perfect methods, has shown that Forbes's figure is also too small, and there are evident reasons for supposing that even Langley's measurements are too low. The sun will therefore require to shrink a good deal more than 35 metres per year to develop from gravitational sources alone the energy radiated by it.

Apart, however, from the energy developed by shrinkage, there will evidently be energy developed within the sun in another way while it is cooling. The falling temperature will be accompanied by combustion, though not in the manner supposed in the old combustion theory of solar energy. There will, however, evidently be a development of energy due to the combination or falling together of the molecules which will ensue on the decrease of temperature. Here we have a field for the chemist to come in and do for the chemical part of the subject what Sir W. Thomson has done for the gravitational. It must, however, be confessed that his task is a far more difficult one, at least at present, as we know very little about the condition of matter in the sun, and almost nothing about the amount of energy developed when the simpler forms of matter combine.

Now, though the sun may receive an enormous amount of energy from those two sources, yet it is evident they can never keep the temperature of the sun constant, because, before energy can be developed in either of those ways, the temperature of the sun must fall, and the energy developed will be in proportion to the amount of the fall.

4. On Laplace's Nebular Theory, considered in relation to Thermodynamics. By Sir W. Thomson.

5. On a Class of Alternating Functions. By Dr Muir.

6. Note on Hoar-Frost. By Mr John Aitken.

Hoar-frost is generally described as frozen dew, and is supposed to be deposited in the same manner and under the same conditions as dew; the only difference being in the temperature at which it is

deposited. Though in a general way this may be so, yet there are certain differences in the conditions, and the manner in which the vapour is condensed at the different temperatures, which seem worth referring to.

If we examine a surface, such as a sheet of glass, exposed horizontally near the ground on a dewy night, we shall generally find that the windward edges are dry. This indicates that the air itself is not cooled to the dew-point, though the surfaces of bodies exposed to radiation are, and the air has to travel some distance over the cold surface before its temperature is reduced to the dew-point. If, however, we examine this same surface when the temperature is low enough to cause the deposited moisture to form hoar-frost, we shall frequently find a marked difference. The sheet of glass is generally not only covered with the deposited vapour up to the windward edges, but the deposit is heaviest along these edges, the ice crystals growing furthest out in that direction. This peculiarity in the deposition of the hoar-frost may also be observed on almost all objects—such as branches of trees, iron fences, &c. The heaviest deposit will often be found on the windward side and not on the top, where we might expect to find it, owing to the stronger radiation from that surface.

The question then is, What is the cause of this difference? Why should no vapour be deposited along the windward edges of surfaces when the temperature is above 32°, while the heaviest deposit is formed on these edges when the temperature is below the freezing-point? The dryness of that part of the dewed surface where the air first touches it is caused by the air not being saturated, and requiring to travel some distance over the cold surface before it is cooled below its dew-point. When, however, hoar-frost is forming, the air seems generally to act as if it were supersaturated: the crystals growing most towards the wind seems to indicate that the air does not require to be cooled before it deposits its moisture. But is it possible for the air to be supersaturated? Under ordinary conditions we know this is impossible. Owing to the vast amount of dust in the air there is always plenty of *free-surface* present to prevent this happening so long as the temperature is above the freezing-point. When, however, the temperature falls below this point, we have a much more complicated condition of matters.

A considerable time ago it was suggested by Professor James Thomson and by Kirchhoff that the vapour pressure of ice might be less than that of water at the same temperature; Professor Ramsay and Dr Young have shown that this is the case,* and they have experimentally measured the comparative temperatures of ice and water under the same vapour pressure. This they have done to a temperature of 9 degrees below the freezing-point; lower they could not go, as the water always froze when its temperature was reduced to that point. They found that the ice and the water had the same temperature at 32°, and under a pressure of 4·6 mm. But when the pressure was still further reduced, the water became colder than the ice; and when the pressure was about 3·20 mm. the water was at a temperature of about 23°, while the ice was about 24°. The water was thus about a degree colder than the ice.

It is evident, therefore, that if by any means the ice had been cooled to the same temperature as the water, its vapour-pressure would have been less than that of the water. So that if we have a water-surface and an ice one at the same temperature and near each other, vapour will tend to pass from the water to the ice, because, the vapour pressure of the water being higher than that of ice, the air which is saturated to a water surface is supersaturated to an ice one.

Something like this seems to take place when hoar-frost is forming. When the air is cooled, condensation takes place on the dust nuclei, resulting in a foggy condensation. This moisture condensed in the air seems always to keep the liquid form; at least we do not see during frosty weather any indications of the particles being frozen. In the fogs formed low down in our atmosphere there are no optical or other phenomena such as we might expect to find if they were frozen. That the temperature of the air is far below the freezing-point is no evidence that the fog particles will be solid, as it is well known that water, even when in contact with solid surfaces, and with what seem favourable nuclei for forming freezing centres, may yet remain liquid at a temperature far below the freezing-point. Thin films and small drops seem difficult to freeze; I have frequently seen my night-radiation thermometer cooled many degrees below the freezing-point, and yet the film condensed on its surface

* *Phil. Trans. Roy. Soc.*, part ii., 1884.

was in a liquid state. It seems, therefore, quite in keeping with our knowledge that these fog particles in frosty weather may be liquid.

Such being the case, we have water particles floating in the atmosphere during frosty and foggy weather, and the pressure of the vapour in the air will correspond to that of a liquid surface; it will therefore be greater than that of an ice one at the same temperature. Under these conditions the air will rapidly unburden itself of part of its vapour when it comes into contact with an ice surface. This seems to be the reason why hoar-frost grows in the direction from which the air is moving, because the air, being supersaturated, unburdens itself on the first ice surface with which it comes into contact, and does not, as when dew is forming, require to be brought into a condition to cause it to give up its vapour.

In the foregoing I have taken extreme conditions under which dew and hoar-frost are formed, as they are better suited to illustrate the point. There are, however, many intermediate conditions in which both dew and hoar-frost appear to be deposited in nearly the same way. On some nights the sheet of glass is dewed all over and up to all the edges, and there are some nights on which no hoar-frost is deposited on the windward edges of the plate, the air having to pass some distance over the cold surface before its temperature is low enough for it to deposit its moisture. The conditions under which the plate is dewed to its edges are when there is no wind and the air nearly saturated; and the conditions under which no hoar-frost is deposited along the windward edges are when there is some wind, a clear sky, and the air not saturated.

So far as my memory and recorded observations go, we never have a heavy deposit of hoar-frost when the sky is clear, or in those conditions in which we have our heaviest deposits of dew. On all those occasions on which trees and every exposed surface become clothed in crystal garments, and all nature in a single night becomes changed to a wondrously pure and fairy-like scene, the transformation seems always to be accomplished in a thick and foggy atmosphere, which requires the morning's sun to dissolve the veil and disclose its beauties. The thick and foggy state seems to be the general condition of our atmosphere during the growth of these heavy deposits of hoar-frost, and it is a necessary one, if the explanation we have given is correct.

It will be observed that these thick and foggy nights, when heavy deposits of hoar-frost are formed, are the very nights on which little or no dew would be deposited, because the radiation would be checked by the fog. These, however, are the very nights most favourable for the deposit of hoar-frost, because the air has a large amount of vapour in it,—is in fact saturated. And further, while dew requires that the surface on which it is deposited be cooled by radiation, this is not so necessary, and indeed may be absent, in the formation of hoar-frost; because the fog particles radiate and cool the air to the saturated temperature of vapour at a water surface, and the passing air discharges part of its vapour on all ice crystals or other nuclei with which it comes in contact; the passing air at the same time absorbs the heat of crystallisation, while the heat of condensation is balanced by the heat absorbed by the evaporation from the water particles.

But, further, it will be observed that not only are those nights on which hoar-frost is most abundant not similar to the nights on which heavy dews are formed, but they are generally nights on which there would be no dew at all if the temperature was above the freezing-point; these hoar-frosty nights do not therefore correspond to dewy nights, when the temperature is higher, but rather to those nights when every object is wet and dripping, not with dew, but wet with deposited fog particles.

7. On the Quotient of a Simple Alternant by the Difference-Product of the Variables. By Dr Muir.

8. Investigations on the Influence of certain Rays of the Solar Spectrum on Root-Absorption and on the Growth of Plants. By Dr A. B. Griffiths, F.R.S.E., F.C.S. (Lond. & Paris), and Mrs A. B. Griffiths.

This paper details an investigation undertaken to see the influence of certain rays of white light on root-absorption and assimilation in the vegetable kingdom. One of us has been for some years investigating a problem as to the use of ferrous sulphate as a plant food (see Dr Griffiths' memoirs in Journal of Chemical Society

[*Trans.*], 1883, 1884, 1885, 1886, 1887). In the experiments to be detailed here, we have used ferrous sulphate as an indicator of root-absorption.

In seven small flower-pots, each filled with the same kind of soil, and treated with a known weight of ferrous sulphate, were sown some mustard seeds. When the little plants had made their appearance above the soil, they were exposed for some hours each day to the coloured lights of the spectrum.

No. 1 was exposed to the red part of the spectrum; No. 2 to the orange, and so on; after so many hours' exposure they were removed to a dark place. This operation was performed for several weeks, each pot being exposed to its own part of the spectrum daily until the plants had grown to a considerable size. The plants in each pot were then reduced to ashes, and submitted to analysis, the following percentage of ferric oxide being found:—

Mustard Plants.

	Percentage of Fe_2O_3 in Ash.
Pot No. 1 exposed to red part of spectrum, gave . .	0·92
,, 2 ,, orange ,, . .	1·43
,, 3 ,, yellow ,, . .	2·51
,, 4 ,, green ,, . .	1·90
,, 5 ,, blue ,, . .	0·71
,, 6 ,, indigo ,, . .	0·20
,, 7 ,, violet ,, . .	0·15

The same number of seeds were placed in each pot, and each pot received the same quantity of iron sulphate during the investigation. The plants, as they grew, were watered from time to time with a weak solution of ferrous sulphate, always of the same strength (see figure).

We have also tried the same experiments upon bean seeds (working exactly under similar conditions as those detailed above), with the following results:—

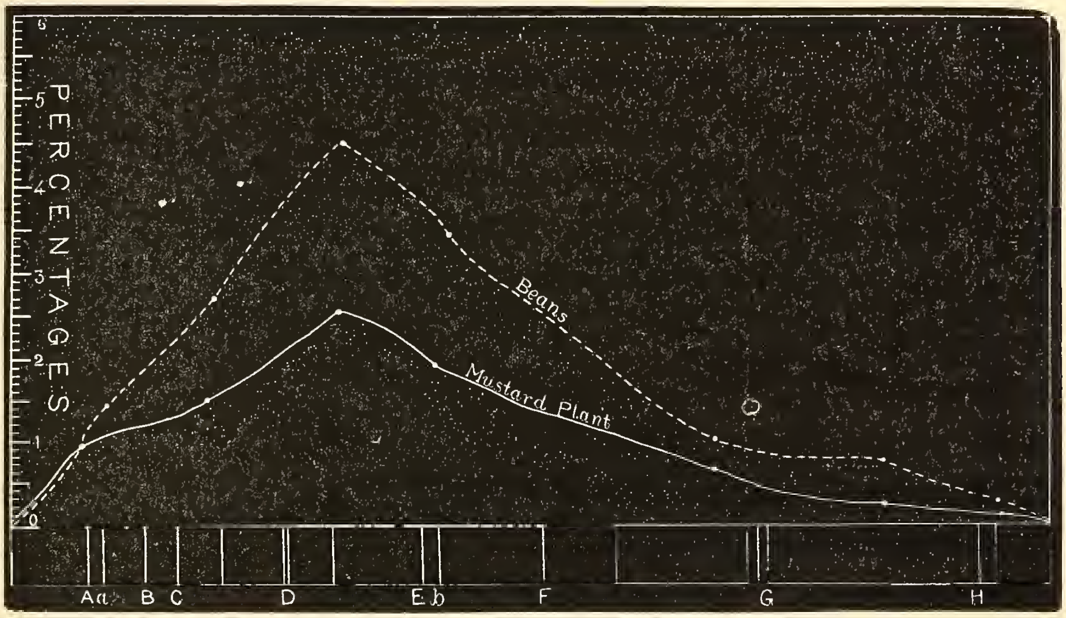
Bean Plants.

	Percentage of Fe_2O_3 in Ash.
Pot No. 1 exposed to red part of spectrum, gave . .	1·40
,, 2 ,, orange ,, . .	2·75
,, 3 ,, yellow ,, . .	4·52
,, 4 ,, green ,, . .	3·34
,, 5 ,, blue ,, . .	1·12
,, 6 ,, indigo ,, . .	0·84
,, 7 ,, violet ,, . .	0·53

The soil used in the fourteen experiments was a calcareous soil of the following composition:—

Lime,	53·00
Organic matter,	1·50
Oxides of iron and alumina,	1·62
Magnesia,	0·32
Potash and soda,	0·01
Phosphoric acid,	0·04
Silica,	0·26
Chlorine,	0·01
Sulphuric acid,	None.
Carbonic acid,	43·24
	100·00

Graphic Representation of the Amount of Ferric Oxide in the Ashes of Plants after growing in an Iron Manure, and exposed to various Rays of the Spectrum, illustrating Root-Absorption.



These experiments therefore show that the most active rays for root-absorption coincide with those of assimilation. From the researches of Drs Draper and Pfeffer, and those of Cloez and Gratiolet, it is evident that the most favourable rays of white light are those lying between the yellow and green. In their experiments the greatest amount of oxygen was evolved in this part of the spectrum, therefore the largest amount of carbon is retained by the process of assimilation. From our experiments we obtain the largest percentage of ferric oxide in the ashes of those plants exposed to the yellow or yellow-green part of the spectrum, or between Fraunhofer's

lines D and E; therefore conclude that the most energetic rays of white light for root-absorption coincide with those for assimilation.

In the presence of light, or the different rays of white light, the two processes go on side by side.

Albuminoid Formation.

The mode of formation of albuminoids in the vegetable kingdom is said to be unknown. We know, according to Lieberkühn, that the albumens have an empirical formula of $C_{72}H_{112}N_{18}SO_{22}$. As this molecule contains *sulphur*, the increase of protoplasmic (albuminoids) matter in the living plants, as far as the sulphur is concerned, would only have come from the sulphur of the ferrous sulphate, as there were no other sulphates present in the soil. Moreover, we find the largest percentage of albuminoids in the plants when they have been grown in the yellow part of white light, as the following table shows:—

Percentage of Albuminoids.

	Mustard Plants.	Bean Plants.
Pot No. 1 exposed to red part of spectrum, gave . .	23·3 %	2·4 %
,, 2 ,, orange ,, . .	25·5	4·2
,, 3 ,, yellow ,, . .	27·6	6·5
,, 4 ,, green ,, . .	26·2	5·3
,, 5 ,, blue ,, . .	21·1	3·6
,, 6 ,, indigo ,, . .	18·4	2·2
,, 7 ,, violet ,, . .	13·0	1·8

Also, the percentage of sulphur (estimated directly) was the largest in each case when grown in the yellow part of the spectrum.

Dr W. J. Russell, F.R.S. (of St Bartholomew's Hospital, London), found that in those plants grown by the aid of iron sulphate the relative amount of chlorophyll (in equal areas, and also in equal weights of the leaves) is increased over those not grown with the iron manure. The following table gives—

Dr Russell's Estimation of Relative Amount of Chlorophyll in the Leaves of Dr Griffiths' Crops of 1884.

	Chlorophyll in equal areas of the leaves.	Chlorophyll in equal weights of the leaves.
(1) Beans grown *with* iron sulphate,	100	100
(2) ,, *without* ,,	79	76
(1) Turnips grown *with* ,,	59	61
(2) ,, *without* ,,	40	39

(For further details, see Journal Chemical Society, *Trans.*, 1885, page 54). This proves that a soluble iron salt nourishes the chlorophyll granules; and recently Prof. J. v. Sachs, in describing the symptoms of vegetable chlorosis, recommends the salts of iron as a remedy (Biedermann's *Centralblatt für Agrikultur Chemie*, vol xv. part 9). It has been shown by one of us that crystals of iron sulphate have been found near to the chlorophyll granules when sections of plants are examined under the highest powers of the microscope (Journal Chemical Society, *Trans.*, 1883, page 195).

Hence it appears that albuminoids, or, in other words, the protoplasm of the living cell, is formed by the combined action of assimilation and root-absorption in the vicinity of the chlorophyll granules. The sulphur required to complete the albumen molecule comes from the decomposition of sulphates, and the nitrogen from the nitrates or ammonia salts (added to the soil or derived from organic matter) taken into the plant by root-absorption.

PRIVATE BUSINESS.

Mr Arthur Silva White, Mr W. Peddie, Mr H. M. Cadell, Mr G. B. Wieland, and Mr A. H. Sexton were balloted for, and declared duly elected Fellows of the Society.

Monday, 21*st March* 1887.

The Hon. Lord M'LAREN, Vice-President, in the Chair.

The following Communications were read:—

1. Variations in the Value of the Monetary Standard. By Professor Nicholson.

2. On Ice and Brines. By J. Y. Buchanan.

(*Abstract.*)

The composition of the ice produced in saline solutions, and more particularly in sea-water, has frequently been the object of investigation and of dispute. It might be thought that to a question of

whether ice so formed does or does not contain salt, experiment would at once give a decisive answer. Yet, relying on experiment alone, competent authorities have given contradictory answers. All agree that ice, whether formed artificially in the laboratory by freezing sea-water, or found in nature as one of the varieties of sea-water ice, retains, in one form or another, and with great tenacity, some of the salt existing in solution in the water. The question at issue is whether this salt is to be attributed to the solid matter of the ice or to the liquor mechanically adhering to it, from which it is impossible to free it. Most bodies, and especially those which take a crystalline form, are easily purified and freed from all suspected foreign matter, with a view to analysis, by the simple operation of washing and drying. It is impossible to wash the crystals, formed by freezing a saline solution, with distilled water, because they melt at a temperature below that at which distilled water freezes. The effect of the addition of a small quantity of distilled water to a quantity of saline ice is at first the anomalous one, that what was a wet sludge is transformed into a dry crystalline powder. It is, of course, impossible to dry the ice by heat, and to do so by more intense freezing would be begging the question. The experimental difficulties therefore account for some of the divergence of opinion on the subject. The mixed character of the substances examined has also much to do with it. As a rule, it may be said that those investigators who have confined their observations to the laboratory have concluded that the ice forming when saline solutions of moderate concentration, including sea-water, are frozen, is pure ice, and the salt from which it is impossible to free it entirely belongs to the mother-liquor, while those who have collected and examined sea-water ice in high latitudes have come to the opposite conclusion.

During the Antarctic cruise of the "Challenger" I made a number of observations on the sea-water ice found in those regions, and, relying principally on the fact that the melting temperature of the ice was markedly lower than that of fresh-water ice, and that it was impossible by any of the ordinary means familiar to chemists for freeing crystals from adhering mother-liquor to materially reduce its salinity, I came to the conclusion that the ice forming in freezing sea-water is not a mixture of pure ice and brine, but that it contains

the salt found in it in the solid state either as a crystalline hydrate or as the anhydrous salt, but most probably as a hydrate. In dealing with the subject, Dr Otto Pettersson (*Water and Ice*, p. 302) quotes my observations, and also rejects the view that "sea-ice is in itself wholly destitute of salts, and only mechanically incloses a certain quantity of unfrozen and concentrated sea-water." He founds his belief on the fact that numerous analyses of specimens of sea-water ice have shown that the constitution of the saline contents of different specimens of ice differs for each specimen, and is always different from that of the saline contents of sea-water. Were the salinity due to inclosed unfrozen and concentrated sea-water, we "ought to find by chemical analysis exactly the same proportion between Cl, MgO, CaO, SO_3, &c., in the ice and in the brine as in the sea-water itself." He quotes numerous analyses of specimens of sea-water ice from the Baltic and from the Arctic Seas to show that this is not the case. Calling the percentage of chlorine in each case 100, he found in various sea-waters the percentage of SO_3 to vary from 11·49 to 11·89. In specimens of sea-water ice it varied from 12·8 to 76·6, and in brines separating from the ice and remaining liquid at – 30° C. it varied from 1·14 to 1·16.

This argument appears conclusive. In order to explain all the phenomena observed in connection with sea-water ice he cites Guthrie's investigations, which went to show that, in freezing saline solutions, under a certain concentration, pure ice is formed at a temperature which falls from 0° C., when the amount of salt dissolved is infinitely small, to a certain definite temperature when the solution contains a certain definite percentage of salt. Further abstraction of heat then produces solidification of the solution as a whole, in the form of a crystalline hydrate, of constant freezing- and melting-point. To such hydrates, Guthrie gave the name of cryohydrates. Pettersson quotes the following as being particularly applicable to the case of sea-water :—

The cryohydrate of	Contains per cent. of water.	Solidifies at °C.
NaCl	76·39	– 22
KCl	80·00	– 11·4
$CaCl_2$	72·00	– 37·0
$MgSO_4$	78·14	– 5·0
Na_2SO_4	95·45	– 0·7

And he refers more particularly to the cryohydrate of Na_2SO_4 forming and melting at $-0°·7$.

Now the bearing of Guthrie's experiments is to show that, while at sufficiently low temperatures, and with suitable concentration, the water will solidify along with one or other of the salts in solution, until this low temperature and high concentration are attained, pure ice must be the result of freezing.

The abnormal phenomena attending the formation and the melting of ice in saline solutions and sea-water find a natural explanation in an observation which I have frequently quoted, and which Dr Pettersson mentions in a footnote at p. 318, namely, that "a thermometer immersed in a mixture of snow and sea-water which is constantly stirred indicates $-1°·8$ C." If this is true, it is clear that my melting-point observations proved nothing. On repeating the experiment I found it confirmed, and took the opportunity this winter of investigating the matter more closely. The paper now communicated to the Royal Society of Edinburgh contains the first portion of the results. It deals with the subject under two heads, namely, (*a*) the temperature at which sea-water and some other saline solutions freeze, and the chemical constitution of the solid and the liquid into which they are split by freezing; and (*b*) the temperature at which pure ice melts in sea-water and in a number of saline solutions of different strengths.

(*a*) The freezing experiments were limited to sea-water and solutions of NaCl comparable with sea-water.

Chloride of Sodium.—Four solutions were used, and they were intended to contain 3, 2·5, 2, and 1·5 per cent. NaCl respectively. Forty grammes of this solution, in a suitable beaker, were immersed in a freezing mixture of such composition as to give a temperature from 2° to 2°·5 C. below the freezing temperature expected. The temperature at which ice began to form (if necessary after adding a minute splinter of ice) was noted, and the freezing was allowed to continue with constant stirring till the temperature had fallen 0°·2 C. A specimen of the mother-liquor was removed, and the chlorine in it determined; the chlorine in the original solution had been determined before. The beaker was then removed from the freezing bath and allowed to melt. The temperature in all cases rose during melting exactly as it had fallen during freezing. In the following

table are given the means of the temperature at which ice began to form in the original solution, and that of the liquid when the sample of brine was taken, and the means of the chlorine found in the original solution and in the brine sample :—

Mean freezing temperature, .	−1°·875 C.	−1°·63	−1°·30	−0°·975] ;
Mean per cent. Cl., . .	1·87	1·60	1·30	0·98

It will be seen that, in the dilute solutions experimented with, the percentage of chlorine expresses, in terms of the Centigrade scale, the lowering of the freezing-point of the solution.

Sea-Water.—Similar experiments were made with sea-water of different degrees of concentration. In sea-water from the Firth of Clyde containing 1·84 per cent. of chlorine, ice forms at −1°·9 C. The following results are from means of close-agreeing results :—

Freezing temperature, .	−2°·0	−1°·5	−1°·0	−0°·5
Per cent. chlorine, .	1·94	1·445	0·963	0·475
Difference, . . .	0·06	0·055	0·037	0·025

Sea-water resembles a chloride of sodium solution containing the same percentage of chlorine, and the resemblance is closer the greater the dilution. When the beaker was removed from the freezing-bath, the temperature rose during melting as it had fallen during freezing. In these experiments, which had for their object the determination of the temperature at which the crystals melted, as well as that at which they began to form in the water, it was impossible to remove a sample for analysis large enough to enable the sulphuric acid to be determined in it.

For this purpose a series of observations were made, using quantities of 300 grammes of sea-water. Freezing was continued usually until the temperature had fallen 0°·3 C. below that at which crystals began to form. The mother-liquor was then separated from the crystals by means of a large pipette with fine orifice, before removing the beaker from the freezing bath. The magma of crystals was then brought rapidly on a filter and drained by means of the jet pump. The ice, thus drained, was then melted, and the three fractions were analysed. In the following table (I.) the results of four experiments are given. In the one column (W) will be found the weight of the original water taken and of the fractions into which it was split on freezing ; in the other (R) will be found

the ratio of SO_3 to Cl found by analysis, the chlorine being set down as 100; thus, in I. the percentage of chlorine found in the crystals, melting at the lowest temperature, was 1·497, and that of the SO_3, 0·174; the ratio (R) is therefore 11·62.

TABLE I.—*Freezing Sea-Water—Analyses of Fractions.*

<table>
<tr><td>No. of Experiment,</td><td colspan="2">I.</td><td colspan="2">II.</td><td colspan="2">III.</td><td colspan="2">IV.</td></tr>
<tr><td>Nature of Water,</td><td colspan="2">Forth 100 °/ₒ.</td><td colspan="2">Mother-liquor.</td><td colspan="2">Clyde 100 °/ₒ.</td><td colspan="2">Clyde 50 °/ₒ.</td></tr>
<tr><td></td><td>W.</td><td>R.</td><td>W.</td><td>R.</td><td>W.</td><td>R.</td><td>W.</td><td>R.</td></tr>
<tr><td>Original water,</td><td>300</td><td>11·83</td><td>90</td><td>11·67</td><td>300</td><td>11·58</td><td>300</td><td>11·21</td></tr>
<tr><td>Mother-liquor, .</td><td rowspan="2">170·6</td><td rowspan="2">11·67</td><td>...</td><td>11·83</td><td>102</td><td>11·57</td><td>78</td><td>11·67</td></tr>
<tr><td>Drainings, .</td><td>...</td><td>...</td><td>94</td><td>11·56</td><td>109</td><td>...</td></tr>
<tr><td>Crystals, . .</td><td>106</td><td>11·62</td><td>23</td><td>11·22</td><td>97</td><td>11·67</td><td>106</td><td>11·4</td></tr>
<tr><td>" . .</td><td>22·5</td><td>11·11</td><td>...</td><td>...</td><td>...</td><td>...</td><td>...</td><td>...</td></tr>
</table>

It will be seen that the ratios (R) found for mother-liquor, drainings, and ice agree with one another quite as closely as those found in samples of pure sea-water from different localities. It is to be remembered that in these experiments the water was frozen *gently*—that is, the rate of abstraction of heat was low, the temperature of the freezing bath being regulated so as to be about 2° C. below the freezing temperature of the solution. Much of the error and uncertainty about the freezing of saline solutions arises from the violence of the methods employed. Judging then by the constancy of the relation of the percentage of Cl to SO_3, we see that in sea-water, frozen at moderate temperatures, the composition of the saline contents of the original water, the mother-liquor, and the ice is identical; and we are justified in concluding that it is probable that the saltness of the ice is due to unfrozen and concentrated sea-water adhering to it. Ice forming in even very weak saline solutions closely resembles snow (which is ice forming in air), and has the same remarkable power of retaining mechanically several times its weight of water or brine.

If we assume that the ice formed in freezing sea-water is pure ice, and that the saline ingredients are retained by the portion remaining liquid, we can calculate the amount of ice which has been formed if we know the salinity of the original water and that of the residual brine. In the case of sea-water the salinity varies directly with the

percentage of chlorine. The weight of the brine remaining after any freezing operation is found by multiplying the weight of the original water used by the ratio of the chlorine percentage found in the original water to that found in the brine. The difference between the weight so found and that of the original water is the weight of the ice formed. In Table II. the results of this calculation are given for Experiment III. on pure sea-water from the Clyde, and for Experiment IV. on the same water diluted with an equal weight of distilled water.

TABLE II.—*Calculation of Ice formed, on the basis of the Salinity of the Original Water and of the Residual Brine.*

No. of Experiment, . . .		III.	IV.
Weight of original water (grammes), . .	W	300	300
Per cent. Cl in ditto, ...	c	1·836	0·923
Per cent. Cl in mother-liquor, . . .	K	2·212	1·153
Weight of mother-liquor, . . $W\frac{c}{K}=$	L	249·0	293·3
Weight of ice, ... $W-L=$	I	51·0	60·7

Sea-water, like other saline solutions, is easily cooled several degrees below its freezing-point before crystals begin to form. While cooling down to and below what was known to be its freezing-point, simultaneous observations of the temperature of the sea-water and the freezing-bath were made from half minute to half minute. From these observations, the rate of abstraction of heat for different differences of temperature of sea-water and bath was found. At a given moment a minute splinter of ice (weighing much less than a drop of water) was introduced. Crystals immediately began to form, and the temperature rose in from ten to fifteen seconds to the freezing-point. During the freezing the temperatures of bath and sea-water were observed at regular intervals. The heat removed is thus made up of that eliminated during the few seconds when freezing began and the temperature rose to the freezing-point, which is found by multiplying the rise of temperature by the weight of liquid, and that removed during the subsequent cooling, which is found from the duration of the operation and the rate of loss of heat, deduced from observations made during the cooling. The specific heat of the solution is taken as that of the water which it contains,

namely 0·965 for III., and 0·9825 for IV. The mean freezing temperature was $-2°·05$ for III. and $-1°·05$ for IV. The latent heat of water freezing at 0° is 79·25. The specific heat of ice being 0·5, the latent heat of water freezing at $-2°·05$ is 78·22, and that of water freezing at $-1°·05$ is 78·73. In Experiment III. the total heat extracted during the freezing was $4230 \times 0·965 = 4082$ heat units (gramme-degrees), and dividing this by 78·22 we find 52·2 grammes as the weight of pure ice formed at $-2°·05$ C., equivalent to this abstraction of heat. In Experiment IV. the heat abstracted was found to be $5193 \times 0·9825 = 5102$. Dividing this by 78·73 we find 64·8 grammes as the equivalent weight of ice formed.

We have calculated the weight of ice which would be found, first on the basis of the salinity of the solution; and second, on the basis of the observed thermal exchange, assuming in both cases that, in the act of freezing pure ice is formed. Thus—

TABLE III.

No. of Experiment, . . .	III.	IV.
	grammes.	grammes.
Calculated from thermal exchange, . .	52·2	64·8
,, ,, salinity, . .	51·0	60·7
Difference, ...	1·2	4·1

The agreement between the two quantities of ice formed as calculated by the different methods is as close as could be expected, and renders probable the truth of the common assumption that the solid body formed is pure ice.

It has, moreover, been proved by Guthrie, Rüdorff, and others that, in solutions of the salts occurring in sea-water, ice does separate out at first, and continues to separate out until the concentration has become many times greater than that of sea-water. Assuming that in sea-water all the chlorine is united to sodium, 85 per cent. of the water would have to be removed as ice before a cryohydrate would form, and if it contained nothing but sulphate of soda in the proportion corresponding to the sulphuric acid found in it, over 90 per cent. of the water would have to go as ice, before the cryohydrate would be formed.

In my experiments about 15 per cent. of the weight of the water was frozen out as ice, causing a lowering of freezing-point by 0°·3 C.

In nature it is probable that the ice forming at the actual freezing surface does so at an almost uniform temperature, the local concentration produced by the formation of a crystal of ice being immediately eliminated by the mass of water below. In the interstices of the crystals there will be retained a weight of slightly concentrated sea-water at least as great as that of the ice crystals. These retain the brine in a meshwork of cells, and, as the thickness of the ice covering increases, and the freezing surface becomes more remote, the ice and the brine become more and more exposed to the atmospheric rigours of the Arctic winter. The brine will continue to deposit ice until its concentration is such that, for example, the cryohydrate of NaCl is ready to separate out. It probably will separate out until it comes in conflict with, for instance, the chloride of calcium or the chloride of magnesium, which will retain some of the water, without solidifying, even at the lowest temperatures. At the winter quarters of the "Vega" brine was observed oozing out of sea-water ice and liquid at a temperature of $-30°$ C. It was very rich in calcium and especially magnesium chlorides. In fact, *it is probably quite impossible by any cold occurring in nature to solidify sea-water.*

b. Melting of pure ice in sea-water and other saline solutions.—A large number of experiments were made with solutions of concentration comparable with that of sea-water, and in one or two cases the experiments were extended to low temperatures and strong solutions. As a rule, from 50 to 100 grammes of solution, cooled to 0° C., were mixed with an equal weight of pounded ice, also at 0° C. The thermometer used for all these determinations was one of Geissler's normal ones, divided into tenths of a degree Centigrade; and its zero-point was verified almost daily. Along with the thermometer, a pipette of suitable capacity was immersed in the beaker, and used with the thermometer for keeping the mass well mixed. Its upper aperture was closed with a small cork, which was removed from time to time to permit of some of the brine being sucked up and allowed to run back again. The inside of the pipette was thus kept constantly moistened with the slowly altering solution in the beaker. The temperature was read after very thorough mixing, and the sample thereupon immediately removed and preserved for analysis.

As a rule, samples were taken for analysis at intervals of 0°·4 C. The results for three classes of salt in dilute solutions are arranged in Tables IV., V., and VI.

TABLE IV.—*Giving the percentage of Chlorine in Solutions of various Chlorides in which Ice melts at given Temperatures.*

Temperature of melting Ice.	Chloride in Solution.						
	HCl	NaCl	KCl	(Sea-Water.)	$MgCl_2$	$CaCl_2$	$BaCl_2$
	Per cent. Chlorine in Solution.						
° C.							
−3.5	3·06	3·30	...	...	4·12	...	...
−3·0	2·68	3·02	3·00	...	3·62	3·70	...
−2·5	2·28	2·53	2·50	...	3·12	3·20	...
−2·0	1·85	2·02	2·00	...	2·62	2·70	2·72
−1·5	...	1·50	1·50	1·500	1·19	2·15	2·10
−1·0	...	1·02	1·02	1·034	1·51	1·50	1·47
−0·5	...	0·50	0·52	0·588	0·87	...	...

TABLE V.—*Giving percentage of Potassium in Solutions of various Potassium Salts in which Ice melts at given Temperatures.*

Temperature of melting Ice.	Salt in Solution.			
	KCl	KI	$K\frac{Cl+I}{2}$	KOH.
	Per cent. K in Solution.			
° C.				
−3·0	3·29	3·02	3·15	...
−2·5	2·79	2·59	2·68	2·60
−2·0	2·28	2·13	2·17	2·08
−1·5	1·74	1·63	1·66	1·57
−1·0	1·18	1·13	1·12	...
−0·5	0·59	0·60	0·57	...

TABLE VI.—*Giving percentage of Hydrogen in Solutions of various Hydrogen Salts in which Ice melts at given Temperatures.*

Temperature of melting Ice.	Salt in Solution.			
	H_2SO_4	HCl	HNO_3	HKO
	Per cent. H in Solution.			
° C.				
−3·0	0·144	0·076	0·077	...
−2·5	0·119	0·065	0·065	0·066
−2·0	0·097	0·052	0·052	0·053
−1·5	0·073	...	0·042	0·041
−1·0	0·048	...	0·032	...

On considering them, it was at once evident that the lowering of the melting-point of ice followed the concentration of the solution, but the law deviated in all cases from that of strict proportionality to the amount of salt dissolved, in some cases to a greater extent than in others. In comparing the effects of different salts in solution on the melting-point of ice, no simple connection could be traced between their absolute weights and the effects produced; but on comparing chemically equivalent weights, a very close connection was discovered. This will be evident from the inspection of the tables. In each the first column contains the temperatures at which pure ice melts; and in the parallel columns the percentages of chlorine, potassium, or hydrogen in the solutions of the salts indicated at the head of each column, when ice melts in them at the temperature indicated. The figures thus give numbers proportional in each table to the chemically equivalent weights of the different salts. They show at first that, whereas the presence of equal absolute weights in solution produces very different effects, the presence of chemically equivalent weights produces very similar effects. On closer inspection, it is seen that the effects are almost identical where the elements to which the common constituent is united belongs to the same group of the periodic series, and differ sharply where these elements belong to different groups. In the case of the chlorides of sodium and potassium the number expressing the percentage * of chlorine in the solution expresses equally the depression of the melting-point of ice in terms of the Centigrade scale. The same depression of melting temperature is produced by 10 per cent. less of chlorine united to hydrogen, and by 30 to 35 per cent. more of chlorine when united to magnesium, calcium, or barium.

The results obtained with sea-water are also given, for comparison. It will be seen that it behaves very approximately as a solution of chloride of sodium containing the same amount of chlorine.

It is perhaps not very astonishing that unit weight of potassium in saline solution should produce the same effect in lowering the melting-point of ice, whether it is united to Cl or I; but it shows clearly how independent this action is of the general character of the body in solution when we find the effect produced by unit

* All percentages are *by weight.*

weight of hydrogen identical, whether it is united to such opposite radicles as Cl or OK. Table VI. shows further the effect of valence. While a given weight of hydrogen produces the same effect in solution, whether it be united to the very different but both univalent radicles Cl and OK, its effect is reduced by one-half when united to the bivalent SO_4. That valence is not the only factor is shown by comparing the effects of hydrogen and potassium when united to the common element, chlorine. Hydrochloric acid in solution produces a markedly more powerful lowering effect on the melting-point of ice than the equivalent amount of chloride of potassium. Of all the substances that I have experimented on, hydrochloric acid is the most energetic in reducing the melting-point of ice, and with ordinary strong acid and pounded ice there is no difficulty in producing temperatures as low as the freezing-point of mercury. In the case of hydrochloric acid, sulphuric acid, chloride of sodium, and chloride of calcium, I have carried my experiments to low temperatures and great concentration. But before passing to them it is well to consider the more dilute solutions with regard to their density.

That the mere density of the solution in which the ice is melting has no direct connection with the lowering of its melting-point is shown by the following table, in which the specific gravities (at 15° C.) are given of the solutions of different salts which gave the same depressions of melting-point :—

Temperature of melting.	Specific Gravity of Solutions of				
	NaCl	KCl	$MgCl_2$	$CaCl_2$	$BaCl_2$
° C.					
−2·86	1·03370	1·03850	1·03893	1·04756	...
−1·8	1·02174	1·02535	1·02715	1·03262	1·06633

There are many similarities in the effects produced by greatly increasing the pressure upon pure water and by dissolving salts in it. First, there is an absolute diminution in the volume of the solution as compared with the sum of the volumes of its components; second, in virtue of this compression by molecular forces it has become less compressible by mechanical means; third, the temperature of maximum density and the freezing temperature are lowered; and fourth, the former of these two temperatures is lowered more rapidly than the latter. All these effects are produced *in kind*

by increasing the pressure on pure water. Whether, or in how far, they agree in degree must be decided by future experiments.

TABLE VII.

Temperature of melting Ice.	Salt dissolved.		
	HCl	NaCl	$CaCl_2$
	Per cent. Cl in Solution.		
° C.			
−35	15·26		
−30	13·98	...	15·97
−25	12·60	...	14·47
−20	11·00	...	12·65
−15	9·17	11·10	11·29
−10	7·02	8·40	8·93
− 5	4·15	4·72	5·65

Experiments with Concentrated Solutions.—Several series of experiments have been made with hydrochloric acid, chloride of sodium, and chloride of calcium, and also with sulphuric acid. Table VII. gives the results, in the same form as preceding tables, for the chlorides.

It will be seen that, in proportion as the solution becomes more concentrated, further additions of salt produce a greater effect in lowering the melting-point of ice, and at a temperature of − 15° C. equivalent weights of NaCl and $CaCl_2$ produce identical results. In Table VIII. the results for hydrochloric acid and sulphuric acid are given in terms of the percentage of hydrogen in the solution.

TABLE VIII.

Temperature of melting Ice.	Acid dissolved.	
	HCl	H_2SO_4
	Per cent. H in Solution.	
−25° C.	0·355	0·538
−20	0·310	0·487
−15	0·258	0·418
−10	0·198	0·332
− 5	0·117	0·205

The temperatures given in these tables are all in terms of the same thermometer, which has not been verified for this part of its scale by comparison with a standard or with the air thermometer.

We have thus seen that, owing to its peculiar physical properties, it is impossible to prepare the crystalline solid which separates from

sea-water and analogous saline solutions in a condition to enable the question, whether the salt does or does not form part of the solid matter of the crystals, to be solved directly by chemical analysis.

So far as chemical analysis is applicable, it is in favour of the salt belonging exclusively to the adhering brine. When sea-water is carefully frozen artificially, the ratio between the chlorine and the sulphuric acid is the same for the solid contents of the original water, the crystals, and the mother-liquor. It is exceedingly unlikely, if part of the salt went into the crystals, leaving the remainder in the brine, that there would be no selective separation of its constituents.

It has been shown that snow or pure lake ice, which, when melting by itself or immersed in pure water at atmospheric pressure, melts at the constant temperature called 0° C. or 32° Fahr., changes its melting temperature when immersed in a saline solution. The altered melting temperature, however, is the same for solutions of the same composition (no doubt with some allowance for pressure) and different for solutions of different composition.

The temperature at which pure ice melts in a solution is identical with that at which ice separates from the same solution on being sufficiently cooled.

When sea-water is frozen to the extent of 15 per cent. of its mass, and the crystals so formed are allowed to melt in the liquid in which they have been produced, they melt exactly as they have been formed. If snow or pure ice be immersed in the brine formed by partially freezing sea-water, it melts at the same temperature as the ice which had been formed by freezing the sea-water, so long as the chemical composition remains the same in each case.

The heat removed in freezing sea-water to the extent of 15 per cent. of its mass accounted for the production of the same amount of ice as was given by calculation on the basis of the chlorine found in the mother-liquor.

When some saline solutions are cooled for a sufficient length of time at a sufficiently low temperature, there arrives a certain concentration at a certain temperature, when further removal of heat causes solidification of the brine as a whole (cryohydrate).

The concentration necessary for the solidification of even the cryohydrate of highest melting temperature is such that in the

primary freezing of the water of the sea no such body can be formed. It would follow from this consideration alone that the first ice formed on the sea in Arctic regions consists of pure ice, and it is also certain that it would retain a large quantity of the residual sea-water in its interstices. During the winter this inclosed liquor would solidify in the interstices of the crystals to ice and cryohydrates, in so far as the temperature and the nature of the salts in solution would permit. From my experiments with chloride of calcium, and the existence of brines observed to remain liquid at $-30°$ C. at the winter quarters of the "Vega," it is unlikely that sea-water, as a whole, can ever be completely solidified in nature. The presence of unfreezable or difficultly freezable brine in freshly-formed sea-water ice, explains its eminently plastic character even at very low temperatures.

The fact that cryohydrates of different salts solidify and melt at different temperatures, sufficiently explains the various composition of different specimens of *old* sea ice.

The apparent expansion, near the melting-point, of ice formed by freezing water which contains any salt at all is perfectly explained on the hypothesis that in the act of freezing the water rigidly excludes all saline matter from participation in its solidification.

The residual and unfreezable brine which remains in considerable quantity liquid when sea-water is frozen, must also remain in greater or less quantity when fresh water is frozen. All natural waters, including rain-water, contain some foreign and usually saline ingredients. If we take chloride of sodium as the type of such ingredients, and suppose a water to contain a quantity of this salt equivalent to one part by weight of chlorine in a million parts of water, then we should have a solution containing 0·0001 per cent. of chlorine, and it would begin to freeze and to deposit pure ice at a temperature of $-0°{\cdot}0001$ C.; and it would continue to do so until, say, 999,000 parts of water had been deposited as ice. There would then remain 1000 parts of residual water, which would retain the salt, and would contain, therefore, 0·1 per cent. of chlorine, and would not freeze until the temperature had fallen to $-0°{\cdot}1$ C. This water would then deposit ice at temperatures becoming progressively lower, until, when 900 more parts of ice had been deposited, we should have 100 parts residual water, or

brine as it might now be called, containing 1 per cent. of chlorine, and remaining liquid at temperatures above $-1^\circ{\cdot}0$ C. When 90 more parts of ice had been deposited, we should have 10 parts of concentrated brine containing 10 per cent. chlorine and remaining liquid as low as -13° C. In the case imagined, we assume the saline contents to consist of NaCl only, and with further concentration the cryohydrate would no doubt separate out and the mass become really solid. On reversing the operations, that is, warming the ice just formed, we should, when the temperature has risen to about -13° C., have 999,990 parts ice and 10 brine containing 10 per cent. chlorine. Now, owing to the remarkable fact that pure ice, in contact with a saline solution, melts at a temperature which depends on the nature and the amount of the salt in the solution, and is identical with the temperature at which ice separates from a solution of the same composition on cooling, the brine liquefies more and more ice at progressively rising temperatures, until, as before, when the temperature of the mass has risen to $-0^\circ{\cdot}1$ C., it consists of 999,000 parts of ice and 1000 parts of liquid water, containing 1 part of chlorine. The remainder of the ice will melt at a temperature gradually rising from $-0^\circ{\cdot}1$ to 0° C.

The consideration of this example furnishes an easy explanation of the anomalous behaviour of ice formed from anything but the very purest distilled water, in the neighbourhood of its melting-point. This subject has been studied with great care and thoroughness by Pettersson. The apparent expansion of all but the very purest ice, when cooled below 0° C., is ascribed by him in part to solid saline contents of the ice which exercise a disturbing and unexplained influence on its physical properties. Viewed in the light of the fact that the presence of even the smallest quantity of saline matter in solution prevents the formation of ice at 0° C., and promotes its liquefaction at temperatures below 0° C., we see that this apparent expansion of the ice on cooling is probably due to the fact that we are dealing, not with homogeneous solid ice, but with a mixture of ice and saline solution. As the temperature falls this solution deposits more and more ice, and its volume increases. But the increase of volume is due to the formation of ice out of water, and not to the expansion of a crystalline solid already formed.

In Table IX. are given the volumes occupied by the ice (with inclosed brine) formed by freezing 100,000 c.c. (at 0° C.) of a water containing chloride of sodium equivalent to 7 grammes chlorine in 1,000,000 cubic centimetres (at 0° C.).

Table IX.—*Water containing* 7 *parts Cl in* 1,000,000.

Temp. ° C.	Water frozen. c.c.	Ice formed. c.c.	Brine remaining. c.c.	Ice and Brine. c.c.	Pettersson III. Vol. of Ice at T°. c.c.	Diff.
T	V_1	v_1	V_2	v_2	P	$P - v_2$
−0·07	99000	107979	1000	108979	108980	1
−0·10	99300	108306	700	109006	109007	1
−0·15	99533	108561	467	109028	109038	10
−0·20	99650	108687	350	109037	109048	11
−0·40	99825	108879	175	109054	109057	3

The volume (v_2) of the ice and brine formed on freezing this water is compared with that (P) observed by Pettersson in freezing a sample of the distilled water in ordinary use in the laboratory.

It will be seen that the volumes observed by Pettersson agree very closely with those calculated for a water containing 7 parts of chlorine in a million, on the assumption that the saline matter is contained entirely in adhering liquid brine.

The irregularities in the melting-points of bodies like acetic acid, to which Pettersson refers, are without doubt due to a perfectly similar cause.

Also the very low latent heat observed by Pettersson for sea-water is to be explained by the fact that the salt retains a considerable proportion of the water in the liquid state even at temperatures many degrees below the freezing-point of distilled water.

Thus, he made two determinations of the latent heat of sea-water containing 1·927 per cent. Cl and 3·53 per cent. salt. The freezing took place in the one case between the temperatures −9°·0 and −7°·47 C., and in the other between −8°·35 and −6°·94 C., and the results he found were 52·7 and 51·5. The mean initial temperature in these two experiments is −8°·7 C., and the mean final temperature −7°·2 C. At −7°·2 C. ice would form on cooling, and would melt on warming a solution of chloride of sodium containing 6·48 per cent. Cl, which represents 11·87 per cent. of the sea salt. In order to concentrate a brine containing 3·53 per cent. salt to one

containing 11·87 per cent., 70 per cent. of the water in it must be removed. Hence in sea-water freezing at a final temperature of $-7°\cdot2$ C. there is formed 70 per cent. of ice, and there remains liquid 30 per cent. of brine. Freezing began at the mean temperature $-8°\cdot7$ C., and the latent heat of pure ice at this temperature is 75. Calculating the latent heat of this mixture from the heat liberated in the calorimeter during freezing, and assuming that the whole mass had solidified, Pettersson's results give the mean latent heat of this sea-water as 52·1. Calculating the apparent latent heat on the assumption that 70 per cent. of the mass solidifies into pure ice and that 30 per cent. remains liquid, we get the number 51·5. On all grounds therefore we must conclude that pure ice is the primary product in freezing sea-water and saline solutions of moderate concentration.

The *plasticity* of ice and the motion of glaciers receive a simple and natural explanation when we see, as in Table IX., that, if the water from which this ice is produced contains no more than 7 parts of chlorine per millon, it will, in the process of thawing, when the temperature has risen to $-0°\cdot07$ C., consist to the extent of 1 per cent. of its mass of liquid brine or water. The water considered in Table IX. is certainly not less free from foreign ingredients than rain or snow. It follows, therefore, that a glacier, in a climate where the temperature is for the greater part of the year above 0° C., must have a tendency to *flow*, owing to the power of saline solutions to deposit ice and to dissolve it at temperatures below 0° C.

The *verification of thermometers* by comparison with the air thermometer is always troublesome. It results from the above investigations that, if the temperature at which ice melts in solutions of a salt, such as chloride of calcium of different degrees of concentration, were once and for all carefully determined by means of a standard air thermometer, a thermometer could be indirectly but satisfactorily compared with the air thermometer at temperatures below 0° C. by immersing it in a mixture of ice and chloride of calcium solution, and taking a series of readings of the thermometer and samples of the brine simultaneously. By determining the chlorine in the samples the concentration of the brine is ascertained, and the comparison with the standard effected.

Freezing Mixtures.—The results obtained in examining the melting-point of ice in saline solutions affords data for mixing freezing baths of any degree of cooling power. With chloride of sodium, for instance, a rough rule is to have such an amount of salt dissolved in the brine that the percentage of chlorine shall give the desired temperature in Centigrade degrees below the freezing-point. In my experiments in freezing sea-water in quantities of 300 grammes, I usually made up the bath of 500 grammes pounded ice, 400 grammes water, and 45 grammes common salt. When mixed, the liquid contained about 4 per cent. Cl, and gave a temperature a little below $-4°$ C. In the course of an hour the liquid would contain 3 per cent. to 3·25 per cent. Cl, and the temperature have risen to $-3°$ C. By using such baths freezing operations can always be kept completely in hand.

3. On the Distribution of Temperature in the Antarctic Ocean. By J. Y. Buchanan.

(*Abstract.*)

In the regions of the Antarctic Ocean where icebergs are numerous, and where in winter the sea-water freezes, the distribution of temperature in the deeper layers of water is peculiar. The facts are detailed in the *Challenger Narrative* (vol. i.). The general result of her observations went to show that, from the edge of the ice-pack, a wedge of cold water stretches northwards for more than 12° of latitude, underlying and overlying strata at a higher temperature than itself (p. 418).

Although the conditions and facts likely to throw light upon the cause of this phenomenon are discussed, no satisfactory explanation of it is given. One important fact is noticed at p. 421—"The fact that the cold wedge above referred to extended north just as far as the icebergs did in March 1874 points to there being some connection between the temperature and the presence of melting icebergs." It is well known that icebergs consist of land-ice, which is nearly pure frozen water, and melts in the air at 32° F. It was thought that the effect of immersion of such a substance in a medium having a temperature 3° F. lower than its melting-point would be to indefinitely preserve it—that, in fact, only the lower

surfaces of the icebergs large enough to reach to the depth of 300 fathoms would suffer any melting at all. The existence of the cold stratum was ascribed wholly to the cold brine, separated from the ice on the freezing of the sea-water, sinking downwards with an initial temperature of from 28°·5 to 29° F. This cause, though existing and in operation, is quite inadequate to produce the effect observed. The facts related in the preceding paper furnish a complete explanation of the cold wedge of water in the Antarctic Ocean and the dependence of its thickness and temperature on the range of icebergs. These enormous islands of ice, a very large proportion of which rise in tabular form to a height of 200 to 300 feet above the sea, float in many cases with their lower surfaces at a depth of from 250 to 300 fathoms. The warmer and denser water coming from lower latitudes (see *Challenger Narr.*, vol. i. p. 428) bathes these lower surfaces, the temperature of the mixture at the surface of contact falls, the heat abstracted from the sea-water melts a corresponding amount of the ice of the iceberg, and a saline solution is produced, less salt, and therefore lighter than the water away from contact with the iceberg, and having a temperature which depends immediately on the strength of the resulting solution. Being lighter than the surrounding water, this resulting solution necessarily flows up along the sides of the berg to the surface, and its place is taken by fresh undiluted sea-water, which in its turn is cooled, diluted, and transferred to the surface. The result is the production of a means of circulating and of cooling and equalising the temperature of the water within the reach of icebergs. As there is continual renewal of the ocean water brought into contact with the ice, and as its composition is constant, the temperature produced is practically constant, namely, 28°·8 to 29°·0 F., or – 1°·7 to – 1°·8 C. The layer of lighter water from 50 to 80 fathoms thick at the surface is due principally to this melting of land-ice, though it is also due in small proportion to the melting of sea-ice.

Table giving the Temperature at which Ice melts in Sea-Water containing different percentages of Chlorine.

Temp. C., . .	1°·0	1°·1	1°·2	1°·3	1°·4
Per cent. Cl, . .	1·040	1·131	1·222	1·313	1·404
Temp. C., . .	1°·5	1°·6	1°·7	1°·8	1°·9
Per cent. Cl, . .	1·495	1·586	1·678	1·769	1·880

The density (at 15°·56 C.) of the sea-water which comes in contact with the lower surfaces of the icebergs is 1·0255, which represents a chlorine percentage of 1·90. Ice actually *melting* in this water would produce a temperature of – 1°·92 C. When ice is *immersed* in this water it lowers its temperature, and a portion of the ice is melted, producing dilution. The concentration, therefore, or chlorine percentage, which will determine the melting temperature of the ice, will be a little lower than that of the original sea-water. From the "Challenger" observations we see that, on the confines of the pack ice, the cold stratum of water has a uniform temperature of 29° F. (–1°·67 C.). Ice melts at this temperature in sea-water containing 1·65 per cent. of chlorine. In this process ice is melted, so that 100 grammes pure warm sea-water become 119 grammes of diluted cold sea-water. It will be observed that the ice which has been formed in the atmosphere at a temperature of 32° F. comes in this way to be melted at a temperature of 29° F.; and the pressure exerted by the 300 fathoms of sea-water, though it may assist in the lowering of the melting temperature, is insufficient to account for the amount.

Monday, 4th April 1887.

Dr J. MURRAY, Vice-President, in the Chair.

The following Communications were read:—

1. Note on a Formula for $\Delta^n 0^i/n^i$ when n, i are very large Numbers. By Professor A. Cayley.

The following formula

$$\frac{\Delta^n 0^i}{n^i} = e^{-nq}\left[1 + \left(\frac{i+1-2n}{2n}\right)q - \left(\frac{n+i+2}{2}\right)q^2\right], \quad q = e^{-(i+1)/n}$$

is given by Laplace (*Theorie Analytique des Probabilités*, 2 ed. Paris, 1814, p. 195) as an approximate value of $\Delta^n 0^i/n^i$, when n and i are very large numbers, and is applied immediately afterwards to the case where i is of the order $n \log n$. As remarked by Professor

Tait, it is certainly not applicable to the case where i is of the order n, for taking $i = An$, A a given number however large, then q is indefinitely near to the very small value e^{-A}, but nevertheless the last term $-\frac{1}{2}(n+i+2)q^2$, by taking n sufficiently large may be made as large as we please, and the value would thus come out negative. It is thus necessary that i should be at least of the order $n \log n$; but it may be of any higher order.

Writing for greater convenience $r = ne^{-i/n}$ (where r is not very large) then $nq = re^{-1/n} = r(1-X)$ if $X = 1 - e^{-1/n}$, and the formula becomes

$$\frac{\Delta^n 0^i}{n^i} = e^{-r(1-X)}\left[1 + \frac{i+1-2n}{2n}e^{-1/n}\frac{r}{n} - \frac{n+i+2}{2}e^{-2/n}\frac{r^2}{n^2}\right].$$

Here $X = \frac{1}{n} - \frac{1}{1.2}\frac{1}{n^2} + \frac{1}{1.2.3}\frac{1}{n^3} + \&c.$, and the exponential e^{rx}

$= 1 + rX + \frac{r^2X^2}{1.2} + \ldots$ is thus also expansible in negative powers of n, and the formula becomes

$$\frac{\Delta^n 0^i}{n^i} = e^{-r}\left(1 + rX + \frac{r^2X^2}{1.2} + \ldots\right)\left[1 + \frac{i+1-2n}{2n}\,.\,e^{-1/n}\frac{r}{n} - \frac{n+i+2}{2}e^{-2/n}\frac{r^2}{n^2}\right]$$

viz., putting for X its value,

$$= e^{-r}\{1$$
$$+ r\,.\left(\frac{i+1-2n}{2n^2}e^{-1/n} + 1 - e^{-1/n}\right)$$
$$+ r^2\left(\frac{-n-i-2}{2n^2}e^{-2/n} + \frac{i+1-2n}{2n^2}\left(1-e^{-1/n}\right)e^{-1/n} + \frac{1}{2}\left(1-e^{-1/n}\right)^2\right)$$
$$+ \&c.\}$$

or finally expanding the $e^{-1/n}$, and taking the whole result as far as $\frac{1}{n^2}$: coefficient of r is

$$\left(-\frac{1}{n} + \frac{i+1}{2n^2}\right)\left(1 - \frac{1}{n}\right) + \frac{1}{n} - \frac{1}{2n^2}, \quad = \frac{1+\frac{1}{2}i}{n^2};$$

coefficient of r^2 is

$$\left(-\frac{1}{n} + \frac{-2-i}{n^2}\right)\left(1 - \frac{2}{n}\right) + \left(-\frac{1}{n} + \frac{4i}{2n^2}\right)\frac{1}{n} + \frac{1}{2n^2}, \quad = -\frac{\frac{1}{2}}{n} + \frac{-\frac{1}{2}-\frac{1}{2}i}{n^2};$$

whence the formula becomes

$$\frac{\Delta^n 0^i}{n^2} = e^{-r}\left\{1 + r\frac{1+\frac{1}{2}i}{n^2} + \frac{r^2}{1.2}\left(-\frac{1}{n} + \frac{-1-i}{n^2}\right) + . . \right\}.$$

It seems to me that the correct result up to this order of approximation is

$$\frac{\Delta^n 0^i}{n^2} = e^{-r}\left\{1 + r\frac{\frac{1}{2}i}{n^2} + \frac{r^2}{1.2}\left(-\frac{1}{n} + \frac{-i}{n^2}\right)\right\}.$$

My investigation is as follows: we have

$$\frac{\Delta^n 0^i}{n^i} = 1 - \frac{n}{1}\left(1-\frac{1}{n}\right)^i + \frac{n \,.\, n-1}{1\,.\,2}\left(1-\frac{2}{n}\right)^i + . . .$$

the series being a finite one, but the number of terms is very large. But observe that, however large n is, we can take i so large that the second term $n\left(1-\frac{1}{n}\right)^i$ may be as small as we please; taking this term to be of moderate amount, say $= r_1$, the subsequent terms will be not very different from $\frac{r_1^2}{1\,.\,2}$, $\frac{r_1^3}{1\,.\,2\,.\,3}$, . . . and the approximate value is $1 - r_1 + \frac{r_1^2}{1\,.\,2} -$ &c., which is a convergent series having its sum $= e^{-r_1}$. To work this properly out, I represent the successive terms by r_1, $\frac{r_2}{1\,.\,2}$, $\frac{r_3}{1\,.\,2\,.\,3}$, . . . so that the series is

$$= 1 - r_1 + \frac{r_2}{1\,.\,2} - \frac{r_3}{1\,.\,2\,.\,3} + . . .$$

Taking r a value at pleasure not very different from r_1, and multiplying by

$$(1 =)e^{-r}.\, e^r = e^{-r}.\left(1 + r + \frac{r^2}{1\,.\,2} + \ . . .\right),$$

the sum is

$$= e^{-r}.\left\{1 + (r - r_1) + \frac{1}{1\,.\,2}(r^2 - 2rr_1 + r_2)\right.$$

$$\left. + \frac{1}{1\,.\,2\,.\,3}(r^3 - 3r^2 r_1 + 3rr_2 - r_3) + . . .\right.$$

Assume now $r = ne^{-i/n}$, we have

$$r_1 = n\left(1-\frac{1}{n}\right)^i = ne^{i\log\left(1-\frac{1}{n}\right)} = r(1+X_1);\ X_1 = e^{-\frac{1}{2}\frac{i}{n^2} - \frac{1}{3}\frac{i}{n^3} - ..}$$

and similarly

$$r_2 = n\,.\,n-1\,.\left(1-\frac{2}{n}\right)^i = n^2\left(1-\frac{1}{n}\right).\,e^{i\log\left(1-\frac{2}{n}\right)}$$

$$=\left(1-\frac{1}{n}\right)r^2(1+\mathrm{X}_2); \qquad \mathrm{X}_2 = e^{-\frac{1}{2}\frac{4i}{n^2}-\frac{1}{3}\frac{8i}{n^3}-\cdots}$$

$$r_3 = n\,.\,n-1\,.\left(1-\frac{2}{n}\right)^i = n^2\left(1-\frac{1}{n}\right).\,e^{i\log\left(1-\frac{2}{n}\right)}$$

$$=\left(1-\frac{1}{n}\right)\left(1-\frac{2}{n}\right)r^3(1+\mathrm{X}_3); \qquad \mathrm{X}_3 = e^{-\frac{1}{2}\frac{9i}{n^2}-\frac{1}{3}\frac{27i}{n^3}-\cdots}$$

$$\vdots \qquad \vdots \qquad \vdots$$

It is now easy to calculate the successive terms $r-r_1$, $r^2-2rr_1+r_2$, &c., and it is to be observed that, in the parts independent of the X's, we have only terms divided by n, n^2, or higher powers of n: thus in $r^4-4r^3r_1+6r^2r_2-4r^3r_3+r_4$ we have r^4 into

$$1-4+6\left(1-\frac{1}{n}\right)-4\left(1-\frac{1}{n}\right)\left(1-\frac{2}{n}\right)$$
$$+\left(1-\frac{1}{n}\right)\left(1-\frac{2}{n}\right)\left(1-\frac{3}{n}\right), \quad =\frac{3}{n^2}-\frac{6}{n^3}.$$

We thus obtain the formula

$$\begin{aligned}\frac{\Delta^n 0^i}{n^i} = e^{-r}\Big\{1 \\
&+ r\left(\qquad -1\mathrm{X}_1\right)\\
&+\frac{r^2}{1.2}\left(-\frac{1}{n}\qquad -2\mathrm{X}_1+\left(1-\frac{1}{n}\right)\mathrm{X}_2\right)\\
&+\frac{r^3}{1.2.3}\left(-\frac{2}{n^2}\qquad -3\mathrm{X}_1+3\left(1-\frac{1}{n}\right)\mathrm{X}_2-\left(1-\frac{1}{n}\right)\left(1-\frac{2}{n}\right)\mathrm{X}_3\right)\\
&+\frac{r^4}{1.2.3.4}\left(-\frac{3}{n^2}-\frac{6}{n^3}-4\mathrm{X}_1+6\left(1-\frac{1}{n}\right)\mathrm{X}_2-4\left(1-\frac{1}{n}\right)\left(1-\frac{2}{n}\right)\mathrm{X}_3\right.\\
&\qquad\left.+\left(1-\frac{1}{n}\right)\left(1-\frac{2}{n}\right)\left(1-\frac{3}{n}\right)\mathrm{X}_4\right)\\
&\qquad\vdots \qquad\qquad \vdots \qquad\qquad \vdots\Big\}.\end{aligned}$$

where $r=ne^{-i/n}$ as above, and X_1, X_2, ... have the above-mentioned values, the exponentials being expanded in negative powers of n.

Writing $X_1 = \frac{-\frac{1}{2}i}{n^2}$, $X_2 = \frac{-2i}{n^2}$ we have

$$\frac{\Delta^n 0^i}{n^2} = e^{-r}\left\{1 + r\frac{\frac{1}{2}i}{n^2} + \frac{r^2}{2}\left(-\frac{1}{n} + \frac{-i}{n^2}\right)\right\},$$

which is the foregoing approximate value.

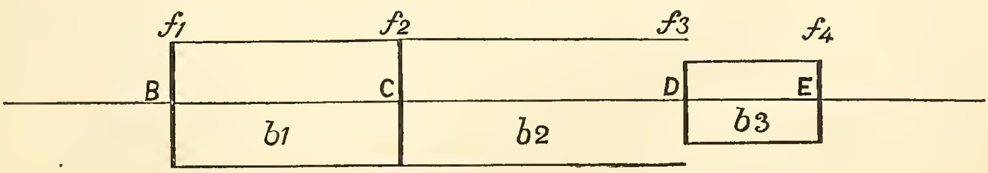
2. On the Fossil Flora of the Radstock Series of the Somerset and Bristol Coal Fields. (Upper Coal Measures.) Part I. By R. Kidston, Esq., F.G.S.

3. On the Achromatism of the Four-Lens Eye-Piece: New Arrangement of the Lenses. By Edward Sang, LL.D.

In designing a telescope for a particular class of observations, it was found desirable to have a field-bar in the focus of the object-glass, and, at the same time, to have an image of that object-glass exterior to the eye-lens. These desiderata cannot both be got by the achromatic arrangement of two lenses made of one material; they are combined in the ordinary four-lens eye-piece.

While investigating the action of the four lenses with a view to a third condition, found, however, to be unattainable, I was led to notice a porism altogether new to me, and which guides us to a new arrangement of the lenses. Believing that this porism has hitherto escaped notice, I venture to submit it to the Society.

Let B, C, D, E represent four thin lenses, all of one material, arranged along the axis of a telescope, whose object-glass A is at a distance beyond the limits of the page, and let us denote their focal lengths by f_1, f_2, f_3, f_4,



while their distances are b_1, b_2, b_3; then the condition of achromatism is contained in the equation

$$\begin{aligned}0 &= f_1f_2f_3 + f_1f_2f_4 + f_1f_3f_4 + f_2f_3f_4 - 2b_1(f_2f_3 + f_2f_4 + f_3f_4) \\ &\quad - 2b_2(f_1 + f_2)(f_3 + f_4) - 2b_3(f_1f_2 + f_1f_3 + f_2f_3) \\ &\quad + 3b_1b_2(f_3 + f_4) + 3b_1b_3(f_2 + f_3) + 3b_2b_3(f_1 + f_2) \\ &\quad - 4b_1b_2b_3 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(A)}.\end{aligned}$$

With this one condition of achromatism among the seven quantities, we are at liberty to conjoin other conditions, subject of course to their possibility. The most obvious collateral condition is that the combination DE be achromatic in itself, so that it may be used separately as an inverting eye-piece. This achromatism is expressed by the equation

$$0 = f_3 + f_4 - 2b_3 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(B)},$$

and, on eliminating b_3 thereby, we get

$$\begin{aligned}0 &= 2(f_1 + f_2)f_3^2 + b_1(f_2f_3 + f_2f_4 + f_3f_4 - 3f_3^2) \\ &\quad + b_2(f_3 + f_4)(f_1 + f_2 - 2b_1) \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(C)}.\end{aligned}$$

This equation (C) shows that the pair B, C cannot be made self-achromatic at the same time with the pair D, E, because this would imply the condition $f_1 + f_2 - 2b_1 = 0$, and would necessitate an infinite value for b_2. The equation (C) may be written in the form

$$\left\{\frac{2f_3}{f_2 + f_4}(2b_1 - f_1 - f_2) - b_1\right\}f_3 = b_1f_2 + b_2(f_1 + f_2 - 2b_1) \quad . \quad \text{(C)},$$

in which the second member is altogether independent of f_3 and f_4.

Now, in arranging a Huygenian eye-piece, we may assume any ratio between f_3 and f_4. If we wish to use a field-bar, as in surveying instruments, we make f_4 the greater, otherwise we prefer to make f_4 the lesser, because then the image of the object-glass is outside of and close to the lens E, and so a large field of view is had. In this case it is important that the Rheita's lens D be well out of the focus of E, in order that any minute imperfections or dust-particles on its surface may be out of view. One maker may prefer the ratio two to one; another maker may adopt that of three to one.

If then we have fixed upon some ratio n, and resolved to make in all our Huygenian eye-pieces

$$f_3 = nf_4 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(D)},$$

the above equation takes the new form

$$f_3 . \frac{(3n-1)b_1 - 2n(f_1+f_2)}{n+1} = b_1 f_2 + b_2(f_1+f_2-b_1),$$

and f_3 becomes indeterminate when both the conditions

$$(3n-1)b_1 - 2n(f_1+f_2) = 0,$$

and

$$b_1 f_2 + b_2(f_1+f_2-b_1) = 0$$

are satisfied at the same time.

If, therefore, we make

$$b_1 = \frac{2n}{3n-1}(f_1+f_2) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(E)}$$

and

$$b_2 = \frac{2n}{n+1} f_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(F)}$$

we may use, along with these, any eye-piece C, D, provided that it have

$$f_2 : f_4 :: n : 1.$$

An eye-piece constructed in this way has the several advantages belonging to all four-lens ones. It shows the objects in their natural position; it allows of a field-bar across which cobwebs and micrometric scales may be placed; a stop between the lenses B and C, having its aperture equal to the image of the object-glass, cuts off all extraneous light; we may introduce there a sun-screen, which shall not be heated so intensely as that usually placed outside of the eye-lens E. But this particular arrangement superadds another. Our inverting Huygenian eye-pieces having been all constructed to the same ratio n, we screw on the portion B, C, D, or uprighter as it may be called, and are then at liberty to use any one of our battery, the magnifying powers being at the same time considerably augmented.

4. An Effective Arrangement for Observing the Passage of the Sun's Image across the Wires of a Telescope. By Edward Sang, LL.D.

At night the cobwebs of the telescope are invisible for want of light; we have to illuminate either the field or the wires, so as to make them visible by contrast. At noon the astronomer meets

with the same kind of difficulty from an opposite cause ; the intense sun's light has to be obscured by a dark glass, which, at the same time, completely obliterates the spider-lines ; these are only seen on the sun's face. In consequence, the advent of the sun's edge to a wire cannot be observed ; the line must be fairly on the sun's face before we can see it, and thus the noted instant is necessarily too late,—too late by a quantity depending on the power of the telescope and on the skill of the observer. Hence the estimate of the sun's apparent diameter from observations of the meridian passage may be expected to err slightly in defect, while the thence-deduced right ascension must be too great.

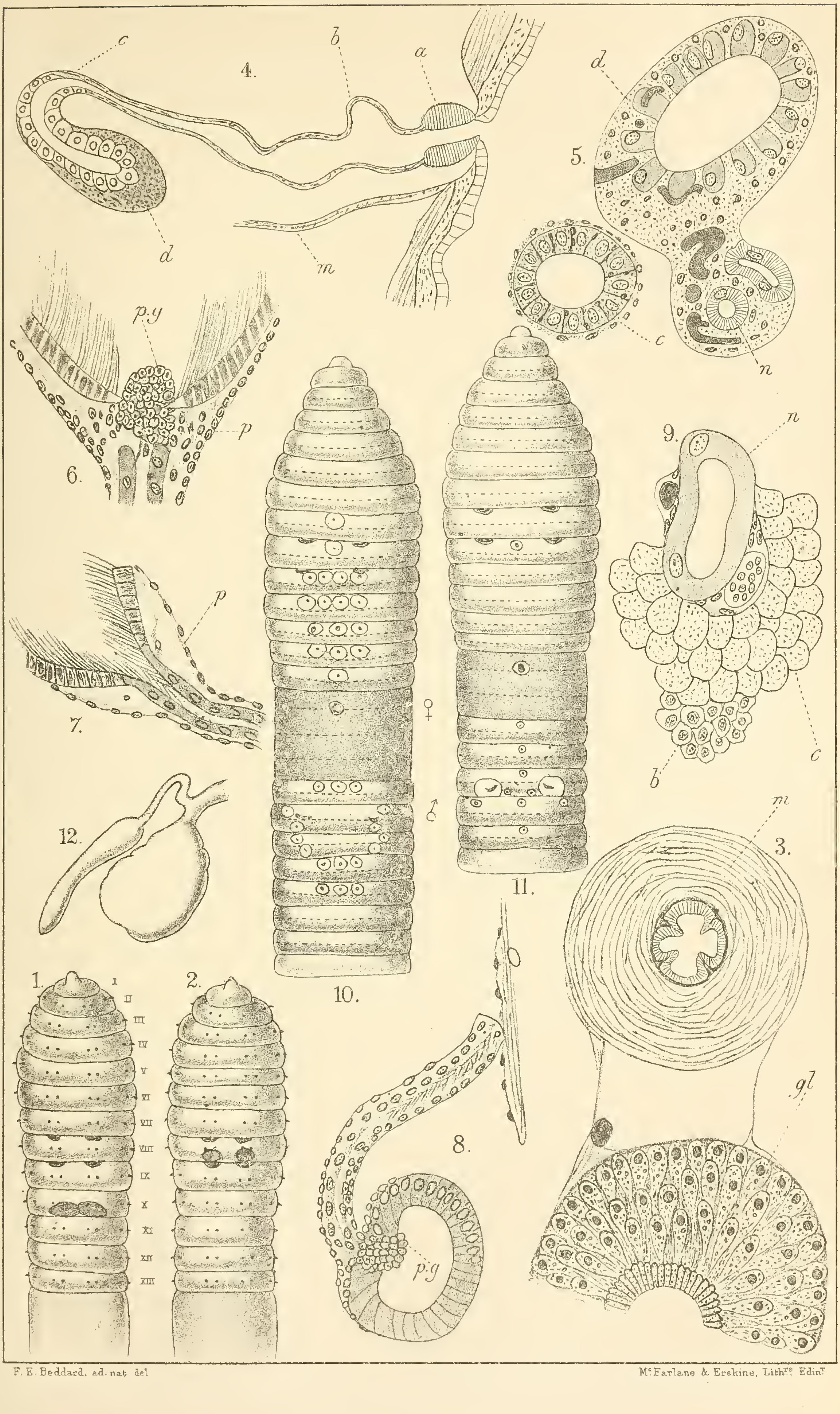
But, if a thin cloud pass before us, we use a paler screen and see the wires over the whole field while the sun's edge remains distinctly defined ; the observations are then satisfactorily made. It occurred to me that, at all times, we may make an artificial cloud, and, to-day just four weeks ago, I laid a thin muslin over the object-glass of my alt-azimuth, and got all that is needed.

5. Observations on the Structural Characters of certain new or little-known Earthworms. By Frank E. Beddard, M.A., Prosector to the Zoological Society of London, and Lecturer on Biology at Guy's Hospital. (Plate V.)

The present paper contains a description of five apparently new species of Lumbricidæ from Australia and New Zealand, one of these species being perhaps the type of a new genus, which I have named *Neodrilus;* the remaining species are *Acanthodrilus neglectus*, from New Zealand, *Perichæta newcombei*, *Urochæta*, sp. ?, from Australia, and *P. upoluensis*, from one of the Pacific islands. I have endeavoured to make these descriptions as full as the material, in many cases in an excellent state of preservation, has enabled me to do. I have also incorporated into this paper some few notes on *Perichæta antarctica*, Baird, a species which has not yet been sufficiently discriminated.

Acanthodrilus neglectus, n. sp.

In my paper on New Zealand Lumbricidæ, recently published in the "Proceedings of the Zoological Society" (*P. Z. S.*, 1885, pt. iv.),



F. E. Beddard, ad. nat. del

McFarlane & Erskine, Lithʳˢ Edinʳ

I described two species of *Acanthodrilus*—*A. novæ-zelandiæ* and *A. dissimilis*—very closely allied in structure, and agreeing in a number of points to differ from the third species, *A. multiporus*. *A. dissimilis* is distinguished from *A. novæ-zelandiæ* mainly by the character of the spermathecæ; these organs are present in both species to the number of two pairs. In *A. novæ-zelandiæ* each spermatheca, which is somewhat pear-shaped, is provided with a number of small diverticula arranged round its external orifice; in *A. dissimilis*, the spermatheca has but a single pair of diverticula, which are of very considerable size. The former species is also frequently provided with a double dorsal blood-vessel; this character is, however, not absolutely distinctive of *A. novæ-zelandiæ*; some individuals agree with *A. dissimilis* in possessing a single dorsal vessel. I may state that the condition of the dorsal vessel is no criterion of the age of the individual. In the largest specimens of *A. novæ-zelandiæ* dissected by me the dorsal vessel was double, while those specimens in which it was represented by a single tube happened to be very small.

On again looking through the collection of New Zealand earthworms which Prof. T. J. Parker kindly sent me, I find that I have confounded two apparently distinct species under the name of *Acanthodrilus dissimilis*.

As there are a large number of individuals of *A. dissimilis* which fall into two series, I think that I am justified in making a specific, or at least a subspecific distinction, although the point wherein the two series of individuals differ is after all rather a small one; but it seems to me that a differential character, if it be constant for a large number of specimens, is of importance, however small.

The accompanying drawings illustrate the difference to which I refer.

In fig. 1, which represents the anterior segments of the body seen ventrally, there are a pair of genital papillæ situated on segment 10. For this variety I shall retain the name *A. dissimilis*. In fig. 2 the genital papillæ occupy a different position; they are situated on the 8th segment. For this variety I propose the name of *A. neglectus*.

Neodrilus monocystis, nov. gen. et sp.

On looking over a collection of earthworms which I have received

from New Zealand by the kindness of Prof. T. J. Parker, I found a single individual which differs from the rest in a number of characters. The remaining specimens belong to three distinct new species which I have lately described,* referring them to the genus *Acanthodrilus.* The specimen which forms the subject of the present communication appeared at first sight to belong to the species *Acanthodrilus dissimilis*, F. E. B., though considerably more slender than any of the individuals of that species which the collection contained.

The setæ are disposed in four series of pairs, and the nephridial pores alternate in position precisely as in *Acanthodrilus dissimilis.* The clitellum occupies a similar position, and extends over an equal number of segments, viz., 5 (13–17). Instead of there being two pairs of spermathecal apertures, there is only a single one situated between the 7th and 8th segments, on a line with the inferior pair of setæ. The male generative pore is placed upon the 17th segment, and each pore is continuous with a groove upon the integument, which extends over the following segment, and ends upon the middle of the 19th segment. I could not, however, detect a second pair of male generative pores upon this segment. In *Acanthodrilus dissimilis*—at least in many individuals—there is a similar groove connecting the genital pores of the 17th with those of the 19th segment entirely similar to that of *Neodrilus.* It is possible that this supposed new genus *Neodrilus* is really an *Acanthodrilus*, in which the posterior pair of male generative pores, together with their glands, have not yet been developed. I am not aware, however, of any similar instance in the genus *Acanthodrilus*, and the present species is fully mature. In favour of this supposition, however, is the condition of certain other peculiar accessory generative structures which I have lately described † in a species of *Acanthodrilus* from New Caledonia ; these are sometimes present and sometimes absent in mature individuals. Another possibility is that the present individual is abnormal, and it is principally for these reasons that I have hesitated in making a new genus ; though there can be no doubt of the *specific* distinctness of the worm.

The male generative pore is continuous with a long, coiled, tubular prostatic gland, the proximal region of which is a slender

* *Proc. Zool. Soc.*, 1885, pt. iv. † *Proc. Zool. Soc.*, 1886.

muscular (fig. 3 *m*) tube, while the distal region is thick and glandular (fig. 3 *gl*); with the aperture is also connected a thin-walled sac containing a bundle of long penial setæ.

Spermathecæ.—In the 8th segment are a pair of oval spermathecæ, which open on to the exterior in the groove which separates this segment from the one in front. As is so generally the case, they are provided with a diverticulum. The diverticulum of each spermatheca lies in the segment in front of that which contains the spermatheca itself, and is remarkable in being actually larger than the spermatheca. In most, if not in all, the genera of earthworms which are included in Perrier's two groups, Intraclitellians and Postclitellians, the spermathecæ open close to the anterior boundary of the segment which contains them. In certain species of *Lumbricus* and other Anteclitellian genera, the position is sometimes different, the spermathecal apertures being situated near to the posterior boundary of their segment. In one instance, at any rate, the spermathecæ actually perforate the mesentery bounding the segment which contains them on their way to the exterior. In a species of earthworm lately described by myself in a note communicated to the Royal Society of Edinburgh,* this is the case with one or more of the seven pairs of spermathecæ which are present in that species. It might be imagined, therefore, that in *Neodrilus* the anterior larger portion of the spermatheca really corresponds to the spermatheca, while the posterior smaller portion is the homologue of the diverticulum so constantly found in *Perichæta*, *Acanthodrilus*, and in other genera. Without an examination of the minute structure of the two regions of the spermatheca, it would be difficult to say which was spermatheca and which diverticulum. In three species of *Acanthodrilus* I have described, I believe for the first time, a very marked difference in minute structure between the spermatheca and the diverticulum, which is correlated with the fact that the spermatozoa always appear to be stored up in the diverticula. In the present species I find an identical difference in the structure of the spermatheca and its appendage, which leads to the inference that the anterior sac is the diverticulum. Seeing that in many cases, especially in *Perichæta*, the diverticula of the spermathecæ extend into the segment anterior to that which con-

* *Proc. Roy. Soc. Edin.*, 1885–6, p. 451.

tains the spermatheca itself, the disposition of these structures in *Neodrilus* is perfectly normal.

The *Nephridia* have exactly the same structure as in *Acanthodrilus dissimilis,* and, as already mentioned, alternate in position from segment to segment in the same fashion. This fact cannot, however, be regarded as a proof that the two worms belong to the same genus. I shall have occasion to point out in a future paper that an Australian earthworm, *Cryptodrilus fletcheri,* n. sp., possesses nephridia which are in every respect similar to those of *Acanthodrilus* and *Neodrilus* in structure and in position, and other instances are there mentioned.

Urochæta, sp.

The present description is the outcome of an investigation into the structure of an Australian species of the genus *Urochæta.* The specimens were kindly given to me by Mr S. Prout Newcombe, and come from Queensland. I have been able to examine a large number, all of which were in a very fair state of preservation for microscopical examination.

The genus is at present known to inhabit Brazil, the West Indies, Java, Sumatra, and Australia, and comprises only three species at most. The first of these was originally described by Fritz Müller,* who met with it in Brazil, under the name of *Lumbricus corethrurus.* The specific name "brush-tail" was given to the worm on account of the irregular disposition of the setæ at the posterior end of the body; the segments are in this region of the body very close together, and the setæ being usually (at least in the contracted state of the body) much protracted and directed backwards, the aptness of the name will be very evident to any one acquainted with these worms. Fritz Müller did not thoroughly investigate the structure of the worm, and was therefore unable to see any reason for removing it from the genus *Lumbricus.*

A somewhat fuller account of the same species was given by Perrier in his "Recherches pour servir à l'histoire des lombriciens serrestres." † Perrier rightly created a new genus for the reception

* *Arch. f. Naturg.*, xxiii.; *Ann. and Mag. Nat. Hist.*, ser. 2, vol. xx.; *Abh. d. naturf. Gesellsch. in Halle,* v., vi., 1857; in *Landplanarien,* von Max Schultze.

† *Nouv. Arch. d. Muséum,* t. viii. (1872).

of this species, which he termed *Urochæta*, but he altered the specific name of Müller into "*hystrix.*" Two years later M. Perrier published * a much more detailed and beautifully illustrated memoir upon the same species, which he referred to more correctly under the name of *Urochæta corethrura.* The specimens investigated by Perrier were obtained, not only from Brazil, but also from the West Indies (Martinique), and, which is more remarkable, from the island of Java. Perrier is inclined to think that the occurrence of the same species in the New World and in Java is rather to be explained by its accidental importation into the latter country, than to be regarded as of importance as a fact in geographical disposition. The occurrence, however, of a very closely allied species in the neighbouring island of Sumatra is somewhat against the supposition, and I am not at all certain that the species to be described in the present paper—a native of Australia—is really different from *Urochæta dubia.*

A second species of the genus has been quite lately described by Dr Horst, † under the name of *Urochæta dubia*, from Sumatra. Dr Horst's description is necessarily—owing to the poor condition of his material—brief, and only refers to the more important points.

The differences between *Urochæta dubia* and *U. corethrurus* are chiefly in the position of the spermathecæ (situated in segments 6, 7, 8, instead of 8, 9, 10) and in the fact that there are four pairs of modified clitellar setæ, a pair upon each of the segments 18, 19, 20, 21, instead of the single pair (on segment 20) of *U. corethrurus.* It appears also that in Horst's species all the segments anterior to the clitellum are furnished with setæ, while in *U. corethrura* the first three segments are devoid of these structures; furthermore, the irregularity in the arrangement of the setæ begins to be evident in segment 10 in *U. dubia*, and not until segment 14 in *U. corethrurus.*

The *clitellum* is very readily to be made out with specimens of the Australian *Urochæta*, and occupies about eight segments, commencing with the 14th and ending with the 22nd. Very often the first and last of these segments were only partially

* *Arch. de Zool. Exp.*, t. iii. (1874).
† *Midden Sumatra*, Vermes door Dr R. Horst, p. 7.

invaded by glandular substance. In fully mature individuals the clitellum was perfectly developed on the ventral as well as on the dorsal side of the segments pertaining to it. A remarkable fact about the clitellum of this species is that the glandular substance is entirely undeveloped between the segments, so that this region of the body is just as plainly segmented as any other region; indeed, the contrast between the thick glandular appearance of the segments themselves, and the deep furrows which separate them, renders the segmentation if anything rather more conspicuous than elsewhere.

It is to be noted that the number of segments occupied by the clitellum and their position is the same as in the other two species of *Urochæta.*

The disposition of the *setæ* is remarkable; in the anterior segments of the body, comprising the first eight segments, the setæ are arranged, as in *Lumbricus*, in four series of pairs; the two setæ of each pair are closely approximated to each other, and the intervals between the pairs are not widely different.

In the 9th segment there is already some little difference in the setæ; the two setæ of each of the ventral pairs are at a little greater distance from each other than in the preceding segments; the dorsal pair of setæ of the right side is completely similar to the same pair of setæ in the foregoing segments; on the left side, however, the two setæ have become widely separate, the distance between them being much greater than that which separates the individual setæ of the ventral pairs.

In the next segment the two setæ of each of the ventral pairs are somewhat more widely separated from each other, but the two setæ of each of the dorsal pairs are again quite close together, as in the earlier segments.

In the next few segments the two ventral pairs of setæ remain exactly as in the segments just described; the innermost setæ of the dorsal pairs correspond exactly in position to the innermost of the same pair of setæ in the earlier segments. The outermost setæ, however, vary very much in position, being sometimes nearer to, and sometimes further away from the innermost setæ; moreover, the two halves of the body are not symmetrical in this respect.

Throughout the greater part of the body, commencing shortly

after the clitellar segments, if not earlier, the setæ have a partly regular, partly irregular arrangement. The ventral setæ of each pair have a fixed position, and correspond for a large number of consecutive segments; the dorsal setæ of each pair are, on the contrary, quite irregular in their disposition. There appears to be no regular alternation in their arrangement; it sometimes happens that the seta of two consecutive segments will correspond in position, sometimes the setæ of one segment, and the next but one or next but two, &c., that it is impossible to lay down any general statements. The two halves of the body are not symmetrical in respect of their setæ. In the hinder part of the body there is a perfectly regular alternation of the setæ from segment to segment; each seta of one segment is exactly between two setæ of the preceding and consecutive segments; and this statement applies to all the setæ in that region of the body, hence there are exactly sixteen rows in this region of the body, while there are a great many more anteriorly.

In *U. corethrurus* the setæ of the anterior segments are disposed regularly and in pairs; but the two setæ of each pair do not appear from Perrier's description to be so closely applied as in my species. They agree in the fact that in the posterior part of the body the setæ regularly alternate, each seta being placed between two setæ of the preceding and succeeding segments. Perrier, however, says nothing about the disposition of the setæ in the middle portion of the body. I must assume, therefore, for the present that the remarkable arrangement of the setæ of my *Urochæta* in this, by far the greater portion of the body, is peculiar to that species, and distinguishes it from *Urochæta corethrurus.* Dr Horst's description of *Urochæta dubia* seems to show that this species differs but little in this particular respect from *U. corethrurus.*

With regard to the shape of the setæ, I have to record an important difference from *U. corethrurus.* Perrier describes and figures the setæ in the latter species as being bifid at their free extremity, and dwells upon the similarity in this respect to the *Naidea.* Horst says nothing about the structure of the setæ in *U. dubia.* In my species I did not succeed in observing any bifurcation of the distal extremity of the setæ; these structures are, in fact, precisely similar to those of other earthworms. This differ-

ence might be regarded as of generic value, were it not for the correspondence in all other essentials of structure.

Another point of difference from *U. corethrurus* concerns the genital setæ; not, however, in their general shape, for I find no difference in this respect between the genital setæ of my *Urochæta* and those figured by Perrier. But while in *U. corethrurus* the genital setæ are confined to segment 20, where they replace the ventralmost setæ on either side, my species has four pairs of these peculiarly modified setæ; they have precisely the distribution mentioned by Horst in *U. dubia*, being found upon segments 18–21, and occupying the position of the ventralmost setæ.

Intersegmental Septa.—As in so many other species and genera of earthworms, the present species exhibits a thickening of certain of the anterior mesenteries. There are four of these specially thickened mesenteries, the first of which immediately follows the gizzard; the last forms the posterior boundary of segment 10. It is in the segments bounded by these thick mesenteries that the spermathecæ lie.

The hindermost of these thickened mesenteries, as already stated, marks off the 10th from the succeeding segment; the arrangement of the mesenteries in front of this does not correspond exactly with the external segmentation. The posterior spermatheca lies in a segment which is bounded anteriorly by the last but one of the thickened mesenteries, and posteriorly by the last of these; externally, however, this segment is distinctly separated by a cross furrow into two segments; and, moreover, the difference between the external and internal segmentation is not only marked by a cross furrow, but also by what is more important, namely, a distinctly double row of setæ.

In the median ventral region of the body there are traces left of the mesentery which should divide the 9th from the 10th segment on either side of the nerve cord; and symmetrically disposed in relation to the nerve cord and to each other is a muscular band, which is attached above to the posterior stout mesentery, and below to the furrow which marks the division between the 9th and 10th segments. The stout mesenteries are everywhere at their insertion on to the body wall divided into separate muscular bands, two of them only being left between segments 8 and 9.

A comparison of the above description with that of Perrier (*loc. cit.*, p. 390) will show that there is some little difference in these points from *U. corethrurus*. Perrier, in fact, states that in his species the specially thickened mesenteries are inserted on to the posterior margin of segments 5, 7, 8, 9, and 11; two segments, viz., 6 and 10, appear therefore to have lost the posterior mesentery, instead of only one segment, as in my species.

There is some difficulty in making an exact comparison between the two species, because Perrier's figure (pl. xv. fig. 28) does not agree with his description. In the figure referred to there are but *four* thickened mesenteries, which seem to correspond exactly in their arrangement to the mesenteries of the Australian species. There seems, however, to be a slight difference in position; the last thick mesentery in my species forms the posterior margin of segment 10, if the commencement of the clitellum has been rightly referred by me to the 12th segment. It is, however, not an easy matter to differentiate the two or three anterior segments of the body; and, as Perrier had living specimens at his disposal, it is probable that his enumeration of the segments is more correct than mine. In this case the clitellum in my species begins a segment later than in his.

Integument.—Perrier's memoir contains a detailed account of the structure of the integument (pp. 382–400), illustrated by numerous figures. I cannot, however, altogether reconcile his description and figures, in so far as they refer to the structure of the epidermis, with the appearances presented by my own sections.

In fig. 1 Perrier gives a general view of the epidermis or surface view, in which it is seen to be marked out into polygonal areas, separated by a certain amount of interstitial matter; some of these contain granular bodies (lettered *a* in his figure), while others are without them. Between the setæ are certain very peculiar structures (*g*), which appear in section to be contained in sac-like diverticula (fig. 3) of the chitinous cuticle. The bodies themselves are highly refractive; these evidently correspond to similar structures described by Vejdovsky in *Anachæta*.*

In transverse sections through the integument of my specimens of

* *Monograph. d. Enchytræiden*, p. 21; see also a paper by myself in *Proc. Roy. Soc. Lond.*, 1885, p. 464.

Urochæta, I have met with these peculiar structures in abundance. They stain very deeply in borax carmine, but have the appearance of being formed of some resistant substance, being frequently indented; they lie at the base of the epidermic cells, just in the position in which Perrier has figured them (*loc. cit.*, pl. xii. fig. 2 *g*). There is, however, this difference, that whereas in *U. corethrurus* they almost invariably form a regular line between the several setæ of a segment, being but rarely disposed irregularly, in my species the contrary is the case; they are very frequently irregular in size as well as position, though they form always a continuous row between the setæ, and are not, as far as my experience goes, found elsewhere. Perrier is quite right in stating that the polygonal areas in his figure correspond to cells, but has overlooked the fact (which was not known at the time when he wrote) that the "interstitial" substance is also cellular, and consists of elongated narrow cells, the polygonal spaces being occupied by large glandular cells with granular contents which do not stain. In fig. 2, pl. xii. of Perrier's memoir, a transverse section through the epidermis is figured, which does not at all represent the appearances presented by my sections. In Perrier's figure are represented a series of columnar granular cells, among which are a few peculiar rod-like bodies; these latter I am unable to identify in my preparations, unless, indeed, they correspond to the columnar hypodermic cells. The columnar granular cells appear to be a very inaccurate representation of the large glandular cells, which appear to be much more numerous in *Urochæta* than in *Lumbricus*. Judging by other earthworms, it does not appear to be at all likely that M. Perrier's fig. 2 illustrates a real difference in the structure of the epidermis from my species.

I have frequently noticed, on a superficial view of the epidermic, irregularly shaped refractive bodies, like those figured by Perrier and lettered *a* in his figure (pl. xii. fig. 1), within the glandular cells.

Excretory Organs.—My species of *Urochæta* possesses, like *U. corethrurus*, a pair of large glands in the anterior segments of the body, which have been termed by Perrier "glandes à mucosité." These glands open on to the exterior of the body through a long duct with muscular walls. With regard to

the external orifice, Perrier remarks (*loc. cit.*, p. 460):—"En faisant des coupes dans la région antérieure du corps, nous avous constamment rencontré dans l'épaisseur même des téguments un canal circulaire entouré d'une sorte de sphincter et présentant des cils vibratiles très-reconaissables même sur des individus desséchés. Nous avions d'abord pensé que nous avions sous les yeux la coupe de la portion du canal excréteur des glandes à mucosité qui est logée dans les téguments; mais nous n'avons pu nous convaincre de l'inexactitude [exactitude?] de cette appréciation. Dans nos coupes ce canal s'est toujours montré unique, et les glandes à mucosité ont des orificies excréteurs distincts; de plus, le canal en question nous a paru occuper la partie la plus antérieure du corps; et ces faits sont contraires à la supposition qui nous était d'abord venue à l'esprit." If M. Perrier means to state in the above-quoted words that the excretory duct of the "glande à mucosité" is furnished at its termination with a "sphincter" like that which surrounds the aperture of the nephridia, I am in a position to confirm the correctness of his statement. By a series of transverse sections, I have been able to trace on both sides of the body the duct of this gland to its external opening, and I find that the latter is surrounded by one of these peculiar bodies which Perrier was the first to record in the case of the nephridia. On the other hand, the duct never traversed the body walls except, of course, at the point where it perforates it on its way to the exterior, and the *two* ducts were both perfectly distinct. M. Perrier does not mention whether the single duct which he found in the transverse section was situated laterally or in the median line. I cannot detect any trace of cilia in these canals, which, indeed seem to be hardly needed, as they are physiologically replaced by the muscular walls. The presence of the "sphincter" is evidently an important additional resemblance between the glandes à mucosité and the nephridia.

With regard to the *nephridia*, I am unable to find in my species what Perrier states to be the relations of the internal funnel in *U. corethrurus.* He says (*loc. cit.*, p. 438)—"Les pavillons vibratiles . . . (sont) très-rapprochés de la ligne médiane et appliqués contre la cloison. Il y a là quelque chose de différent de ce qu'on observe chez les naïdiens, où les pavillons vibratiles traversent en général la cloison antérieure de chaque anneau, fait que l'on retrouve

aussi chez les *Pontodrilus.* Les Lombrics au contraire semblent d'après les auteurs, se comporter comme les *Urochæta.*"

M. Perrier figures (*loc. cit.*, pl. xvi. figs. 38, 39) the isolated nephridia, which obviously could not be detached entire if the funnel were not situated in the same segment as that which bears the external pore. Nevertheless, in my species I observed in numerous cases that the internal funnel of the nephridium is situated in the segment anterior to that which bears the external pore. I was able to prove this point conclusively by a series of longitudinal sections. It may be that Perrier's specimens and mine differ in this respect, which is certainly rather remarkable. Perrier's assertion about *Lumbricus* is evidently a slip. The funnel (figs. 6, 7, 8) of the nephridium recalls that of *Dendrobæna rubida* (Vejdovsky, *System u. Morph. d. Oligochæten*, pl. xiv. figs. 15, 16) in the extraordinary development of cells, doubtfully regarded by Vejdovsky as peritoneal cells, at the apex of the funnel.

A series of remarkable structures, termed by Perrier "glandes posterieures," and described by him as a portion of the excretory system, now remains for consideration.

These bodies are found as in *U. corethrurus* in the hinder region of the body, but appear to be more numerous than in that species, which has about forty pairs occupying as many segments.

M. Perrier gives a figure of one of these glands (*loc. cit.*, p. xvii. fig. 47), which only partially indicates their structure, as seen in my own preparations. They are somewhat pear-shaped, and terminate in a long slender peduncle, which disappears among the coils of the nephridial tubules. Perrier supposes that they open in common with the latter on to the exterior, but was unable to detect the orifice. Mr Benham* has detected these peculiar glands—"pyriform bodies"—in his genus *Urobenus*, and his description of their minute structure agrees pretty closely with my own observations; these glands open in *Urobenus* ventrally of the lower pair of setæ, while the nephridia open by the dorsal setæ.

Fig. 4 represents one of those glands in *Urochæta* in longitudinal section, reconstructed from a series of sections. It will be seen that its structure is closely similar to that of the same glands in *Urobenus.* The lumen of the gland is lined by a single row of

* *Quart. Jour. Mic. Sci.*, 1886, p. 87, pl. viii. figs. 10, 21.

peculiar cells, rounded and of large size, and each furnished with a distinct nucleus. These cells are evidently larger in proportion, and not so columnar as the corresponding cells in the pyriform vesicle of *Urobenus;* the rest of the gland lying to the outside of these cells is occupied (fig. 5) by a granular substance, with minute darkly staining bodies scattered throughout it (nuclei ?). The lumen ceases some little way in front of the apex of the gland, which is here entirely made up of the granular nucleated substance. It is permeated by blood capillaries derived from the vessels which supply the nephridia. The pear-shaped glandular region of the pyriform vesicle has the structure just described; distally it communicates with a slender muscular duct, which passes gradually into the substance of the gland. The latter is bent upon itself, as indicated in Perrier's figure, so that the duct runs parallel with the gland. But while in *U. corethrurus* and in *Urobenus* the duct is directed towards the nerve cord, the flexure in my *Urochæta* is exactly in the opposite direction. The rounded cells lining the lumen gradually decrease in importance, and the granular substance, with its interspersed nuclei lying to the outside of these cells, eventually disappears; coincidently with these charges the duct of the gland acquires a delicate muscular coat, and the lining epithelium finally becomes a flattened layer of cells. I have traced this muscular sac to its opening on to the exterior in common with the nephridium. Fig. 4 shows the termination of the duct in the rosette-like organ which here as elsewhere guards the orifice of the nephridium. The pyriform vesicle, therefore, is *anatomically* a diverticulum of the nephridial duct in this species.

Spermathecæ.—These organs are present to the number of three pairs; they are situated in segments 7, 8, and 9, and the aperture is in each case placed quite close to the anterior margin of the segment. The spermathecæ of this species are excessively delicate organs, and are often for this reason difficult to distinguish; they are also of very small size, as compared with the spermatheca of many other worms. The smallness of size is manifest rather in their breadth than in their length; when stretched out they reach rather further than across the segment which contains them. These organs are somewhat club-shaped; the distal region is extremely narrow, but widens

out gradually passing backwards, and finally becomes dilated into an oval sac. The spermathecæ sometimes lie straight, and are sometimes coiled into a circle. The walls of the spermathecæ are very thin, owing to the slight development of muscles and the character of the lining epithelium, consisting as it does of flattened cells; these structural features, together with the superficial covering of rounded, vesicular, peritoneal cells, and the general shape of the organs, gives the spermathecæ a very strong resemblance to the diverticula of the nephridia figured by myself in *Acanthodrilus novæ-zelandiæ.** In view of a possible homology between the spermathecæ and such diverticula, it is worth while to record the points of similarity between the two series of organs. Furthermore, I may remark that in a large number of individuals, all fully mature, there was no increased development visible in the spermathecæ, which undoubtedly have a certain appearance of immaturity.

The general shape of the spermathecæ is very like that of the spermathecæ of *Diachæta,*† but they appear to be considerably smaller in the present species, and also differ in that their apertures on to the exterior are at the anterior, instead of at the posterior, boundary of their respective segments.

In *Urochæta corethrurus* ‡ there are also three pairs of spermathecæ not unlike those at present under discussion in shape, and opening like them at the anterior margin of their segment; they are situated, however, rather further back (in segments 8, 9, 10); further, in both *Urochæta* and *Diachæta* the spermathecal segments contain nephridia.

Perichæta newcombei,§ n. sp.

This species is represented by eight individuals, of which four are sexually mature, with a fully developed clitellum.

The colour of the species is a dark purple upon the dorsal surface, gradually passing into a yellowish-brown upon the ventral surface; the intersegmental furrows dorsally, as well as ventrally, are of the

* *Proc. Zool. Soc.*, 1885, pl. lii. fig. 5.

† Benham, *loc. cit.*, pl. ix. fig. 29.

‡ Perrier, *Arch. d. Zool. Exp.*, t. iii. (1874), p. 518, pl. xiii. fig. 12 *pc;* pl. xvii. fig. 49.

§ Named after Mr S. Prout Newcombe.

same colour as the ventral surface; the clitellum also is distinguishable on the dorsal surface by its yellowish tinge.

It is interesting to note that the colour of this species is exactly that of a species of *Perionyx*, from the Philippine Islands, the characters of which I have recently described in a paper communicated to the Zoological Society of London.*

The *preoral* lobe does not divide the circumoral segment. *Dorsal pores* are present between all the posterior segments of the body; in the four mature individuals the first pore is situated between the 5th and 6th segments; the clitellum is marked anteriorly and posteriorly by a conspicuous pore.

The *setæ*, as in other species of the genus, are disposed in a continuous series, occupying the middle line of each segment; they are present on the clitellar segments.

The *clitellum* occupies three segments, Nos. 14, 15, 16; as in all species of *Perichæta* it is developed round the whole circumference of the body.

The *male* and *female generative pores* are placed in exactly the same situation as in other species. The female pore is placed upon the 14th segment, within the row of setæ in the middle line; the male pores are upon the 18th segment, at some little distance from each other, also within the row of setæ.

The *apertures of the spermathecæ* are between 7–8 and 8–9.

A very striking external character of this worm is caused by the great development of *genital papillæ*.

These are developed on the preclitellar segments (fig. 10), as well as on the segments which immediately precede, and on those which follow the 18th segment.

The arrangement of the preclitellar papillæ presents some individual variation, which is probably due to the fact that some of the specimens are more fully mature than others.

In one example the papillæ were more numerous than in any of the others. The 13th segment is furnished with a single papilla in the ventral median line; the 11th and 12th segment have each three papillæ close together, one being median, and the other two disposed symmetrically, one on either side; the 10th segment has four papillæ, of which the middle ones correspond in position to

* *Proc. Zool. Soc.*, 1886, p. 298.

the median papillæ of the two succeeding segments; the 9th segment has a single papilla, corresponding in position to the outermost right-hand one of the 10th segment, the others being indistinct; the 7th and 8th segments have each a single median papilla. In another example the 12th and 13th segments have a single median papilla; the 10th and 11th segments have each three papillæ; the 7th, 8th, and 9th a single median papilla.

Two other examples present an arrangement of the genital papillæ nearly identical with that last described, the only difference being that the papillæ on segments 7 and 8 are wanting.

In every case the papillæ present the appearance of a circular disc similar in colour to the clitellum, and surrounded by a whitish line; the greater part of the disc is placed in front of the row of setæ.

The postclitellar papillæ are not so distinct as the preclitellar. The whole of the ventral integument on the 17th, 18th, and 19th segments lying between the male aperture is whiter in colour than the rest, which renders it very difficult to map out the position of the papillæ. The 17th segment appeared to have a row of these papillæ; in the 18th and 19th segments I could only distinguish two pairs of papillæ, one placed outside of the male pore on the 18th, and in a corresponding position on the 19th segment, and the other placed below, and both inside of the male pore. The 20th segment has a median row of papillæ (3 or 4), the 21st segment has three median papillæ. The postclitellar papillæ are considerably smaller than the preclitellar. I am inclined to think that the whitish appearance of the integument between the male generative pores is due to the crowding together of a row of papillæ, which become distinct and separate on the 20th, and especially on the 21st segment.

The large *pharynx* extends back to about segment 3; the *gizzard* occupies segments 4, 5, and 6; it is important to notice that in every case the segments in which the gizzard lies are separated from each other by distinct, though rather delicate, mesenteries; this fact is worth recording, because in many species of *Perichæta* (and other genera) the gizzard occupies two segments, and the median mesentery has disappeared; there seems to be, however, some connection between this condition and the position of the gizzard.

In the present species the gizzard lies anteriorly to the spermathecæ; in those species where a mesentery has disappeared the gizzard lies further back, and in the same segments with some or all of the spermathecæ.

Calciferous glands are present in segments 10, 11, 12; they are, however, rather dilatations of the lumen of the œsophagus than distinct and separate glands.

The *testes* are situated in the 10th and 11th segments, close to the nerve cord and on either side of it. Dr Bergh is perfectly right in his statement * that the testes and vesiculæ seminales of *Perichæta* are in all essentials similar to those of *Lumbricus.* The testes in the present species are small digitate glands, and are enclosed by the vesiculæ, as is also the nerve cord. The vas deferens passes along the body just below the testis; the funnels of the vasa deferentia open into the vesiculæ seminales, which organs extend from the 9th to the 12th segment.

The *ovaries* are very large, and are situated in the 13th segment.

The *prostates* occupy the usual position.

The *spermathecæ* are present to the number of two pairs, situated in segments 8 and 9; the large somewhat pear-shaped pouch is provided with a small diverticulum on the dorsal side.

The only species of *Perichæta* recorded from Australia are two species, *P. australis* and *P. coxii*, described recently by Mr Fletcher.† It is evident that my species agrees with these two in a great many points; in the first place, there appear to be no intestinal cæca; secondly, the shape and location of the spermathecæ appears to be identical in all three species. The first point of agreement is, however, of more importance than the latter. In a good number of species of *Perichæta* there are two pairs of spermathecæ situated in segments 8 and 9, and each furnished with a slender cylindrical diverticulum; it will be interesting to know if the absence of intestinal cæca is characteristic of other Australian species of the genus.

The present species, however, differs from both its Australian congeners in the presence of vesiculæ seminales in all of the segments from 9–12 inclusive. Fletcher states that these structures are

* *Zeitschr. f. wiss. Zool.*, 1886.

† *Proc. Linn. Soc. N.S. W.*, June 1886, p. 561.

absent in segments 10 and 11 in his species; if this difference is not really due to difference of age, it is clearly of great importance as a distinctive character.

The arrangement of the nephridia is apparently very like what has been described in *P. australis* and *P. coxii*, particularly in the latter species. Fletcher's description is as follows:—"The segmental organs consist of tufted glandular masses, which are large, stalked, and dendriform in some of the most anterior segments, but smaller and inconspicuous elsewhere." I found these structures very conspicuous indeed, and in the 14th and a few succeeding segments they have a very strong superficial resemblance to the ovaries, with which organs their position almost exactly corresponds.

The most characteristic point of difference between my species and the other two is the number and position of the genital papillæ; a comparison of my description with that given by Mr Fletcher of *P. australis* and *P. coxii* will show that the species differ greatly in this respect. Mr Fletcher like myself appears to have examined a considerable number of specimens.

Perichæta upoluensis, n. sp.

This species of *Perichæta*, like the last, is mainly characterised by the number and arrangement of the genital papillæ. It is a native of the island of Upolu, in the South Pacific; I am indebted to Mr R. Damon, of Weymouth, for the opportunity of examining three specimens.

It is an average-sized species, measuring 5 or 6 inches in length.

The apertures of the spermathecæ are between 7–8 and 8–9.

The single aperture of the oviduct is upon segment 14.

The pores of the vasa deferentia are upon segment 18. Each pore is surrounded by a circular area of integument which is marked off from the rest.

The *clitellum* consists of only two segments, Nos. 14 and 15.

The genital papillæ are very small, compared, for example, with those of the last species; they occur in the neighbourhood of the spermathecæ as well as of the male generative apertures.

There is a single papillæ on segment 9, situated in the median ventral line and anteriorly to the row of setæ. The rest of the genital papillæ (so far as my specimens enable me to speak positively)

are situated after the clitellum, *i.e.*, in the neighbourhood of the male generative openings. Each of the segments 16–20 (inclusive) is furnished with a single median papilla, which occupies a precisely similar position to that occupied by the median papilla of segment 9, that is to say, it lies near to the anterior border of the segment (see fig. 11). The 18th segment possesses, in addition, a pair of papillæ, situated just within and close to the male generative orifices; these papillæ are almost on the border line between this and the following segment. The next segment (No. 19) has also an additional pair of genital papillæ; these are placed below and a little to the outside of the generative pores; hence they are placed very close to the anterior border of their segment.

In its internal structure this species does not present any remarkable features.

The *gizzard* is in segments 8 and 9, and as usual these segments are not separated by a mesentery.

In the same two segments are situated the *spermathecæ* (fig. 12), which (see p. 173) are not very different in shape to those of the last species.

The *vesiculæ seminales* are in segments 11 and 12.

The *ovaries* are in segment 13.

The termination of the vas deferens is furnished with a prostate gland, which has the usual lobulated structure.

The hearts are in segments 12 and 13, as is generally the case in *Perichæta.*

Perichæta antarctica, Baird.

Megascolex (*Perichæta*) *antarctica*, Baird, *Proc. Linn. Soc.*, vol. xi. (1873) p. 96.

This species has been described by Baird from a specimen in the British Museum in the following terms:—"Body consisting of about 180 rings. Setæ, surrounding the body, short, black, rather distant. Rings not keeled; larger and more distinct at the anterior extremity, closer at the posterior end, and all smooth. Length 7 inches." Capt. F. W. Hutton, in his "Catalogue of the hitherto described Worms of New Zealand,"* mentions this species, which is a native of New Zealand, and simply quotes Baird's description.

* *Trans. New Zealand Instit.*, vol. xi. (1878) p. 317.

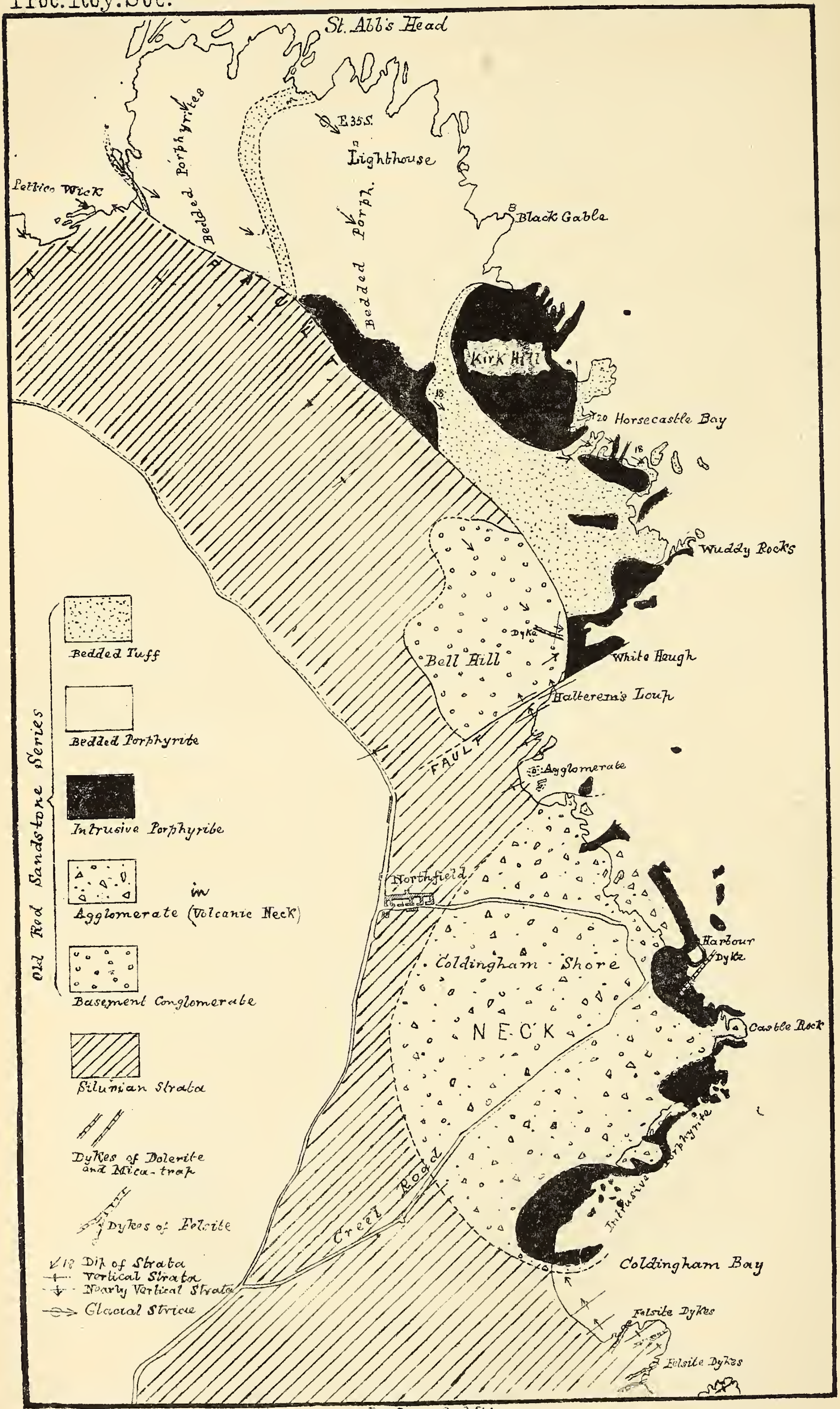
It is perfectly clear that the above-quoted specific diagnosis is entirely insufficient to discriminate the species from many other *Perichætæ;* but an examination of the specimen itself leads me to believe that it is a distinct species. I am unable to give any anatomical description, but the worm exhibits an external character, overlooked by Baird, which is of some value as an aid to discriminating the species. The male genital pores are as usual situated upon the 18th segment of the body, and at some distance from each other; the 17th and 19th segments are each furnished with a single median genital papilla placed exactly in the centre of the segment, and therefore interrupting the line of setæ. The number and arrangement of the genital papillæ seem to be, so far as our knowledge goes, good characters for discriminating the different species of *Perichæta;* although the number is apt to vary somewhat (see p. 171) at different stages of maturity; the number of papillæ in the present species would have to be very largely increased to come up to the number which are characteristic of *Perichæta newcombei* (p. 171), the only other species of *Perichæta* which has genital papillæ in the *median ventral line* on the 17th and 19th segments.

Explanation of Plate V.

Fig. 1. *Acanthodrilus dissimilis.*
Fig. 2. *Acanthodrilus neglectus.*
Fig. 3. *Neodrilus monocystis*, section through prostate; *m*, muscular duct; *gl*, glandular region.
Figs. 4–9. *Urochæta*, sp.
Fig. 4. Median longitudinal section through glandular appendix of nephridium; *d*, glandular *cul-de-sac*; *c*, epithelial lining; *b*, muscular region; *a*, "sphincter" surrounding aperture; *m*, mesentery.
Fig. 5. Transverse section through glandular appendix and a portion of nephridium; *n*, nephridial tubule; *c*, *d*, regions similarly lettered in fig. 4.
Figs. 6, 7, 8. Sections in various planes through nephridial funnel; *p*, peritoneal cells; *p g*, peculiar agglomeration of peritoneal cells in the funnel.
Fig. 9. Transverse section of a nephridial tubule from hinder end of body; *c*, peritoneal cells; *b*, blood corpuscles; *n*, nephridial tubule.
Fig. 10. *Perichæta newcombei*, ventral aspect.
Fig. 11. *Perichæta upoluensis*, ventral aspect.
Fig. 12. Spermatheca of last species.

Geological Map of St. Abbs Head, etc,
Based on that of the Geological Survey.

Proc. Roy. Soc. Vol XIV. Pl. VI.



Scale of Miles.

0 1 2 3 4 5 6 7 8

6. Geology and Petrology of St Abb's Head. By Professor J. Geikie. (Plate VI.)

I. Introduction.

The observations recorded in this paper have reference chiefly to the coast-sections at St Abb's Head and Coldingham Shore. The district was geologically surveyed some twenty-five years ago by my brother, Dr A. Geikie, and subsequently described by him in the Memoirs of the Geological Survey.* Since the publication of that memoir, no further examination of the ground in question appears to have been made. During the past summer I visited the neighbourhood, principally for the purpose of studying the igneous rocks which are so well exposed in sea-coast sections. At the date of the Government Survey of Eastern Berwickshire the aid of the microscope had not yet been invoked by field-geologists for the purpose of determining rock-species, and I was therefore curious to compare the igneous rocks of that region with those of similar age which I had studied elsewhere in Scotland, and more especially with the bedded and intrusive porphyrites and tuffs of the Cheviot Hills and the Sidlaws.

The rocks of the district under review belong, as my brother has shown, to two great systems—the Silurian and the Old Red Sandstone. From Pettico Wick Harbour in the north, to Coldingham Bay in the south, the coast cliffs are composed almost exclusively of rocks of Old Red Sandstone age—Silurian strata appearing only for a short interval, a little to the north of Coldingham Shore. The latter reappear on the south side of Coldingham Bay, and continue along the shore to Callercove Point, in the neighbourhood of Eyemouth. Inland from St Abb's Head and Coldingham Shore they extend for many miles. (See Map, Plate VI.)

II. The Silurian Rocks.

To the description of the Silurian strata given in the Geological Survey's Memoir, I have very little to add. They consist chiefly of greywackés and shales, generally inclined at high angles, and arranged in a series of more or less sharp anticlinal and synclinal folds, which have an average N.E. and S.W. trend. In their least

* Geological Survey Memoirs: *The Geology of East Berwickshire.*

altered condition, they are well seen in the magnificent cliffs that extend westward from Pettico Wick Harbour. Between Coldingham Bay and Callercove Point they are often much crushed, crumpled, twisted, shattered, shifted, and confused—the irregular puckerings and convolutions forming an interesting study. They are minutely cracked and fissured in all directions, the fine cracks and fissures being most frequently filled with white quartz, or with hæmatite and limonite. Where the strata are most highly contorted, they frequently become seamed with a close, irregular network of small veins and mere threads of quartz, the meshes between which are often less than the 16th part of an inch in diameter.

Intrusive Felsite in the Silurian.—Through these excessively contorted rocks ramify here and there tortuous dykes and veins of felsite. The junction between those dykes and the rocks traversed by them is generally well marked. But here and there it is much confused—the fine-grained greywackés being baked and altered, and having the same pale-grey colour as the felsite, so that the line of parting between the two can hardly be distinguished by the unassisted eye. Under the microscope, however, the crystalline and fragmental rocks are readily discriminated. These felsites are confined to the Silurian areas. Nowhere, so far as I saw (and I traversed a considerable area round Coldingham and Eyemouth), do any of the felsitic intrusions penetrate rocks of Old Red Sandstone age.

The rock of these dykes is grey or pale pinkish-grey in colour, compact, sparingly porphyritic, with microscopic crystals of quartz and felspar,—having, in short, the appearance of typical felsite. Here and there veins of white quartz seam the dykes. Viewed under the microscope the rock exhibits a microfelsitic base, scattered through which are abundant small crystals of orthoclase, and a few larger ones of the same mineral. Oligoclase also appears in well-developed crystals, and to the same species some of the smaller crystals of felspar seem to be referable. Both felspars show alteration into saussurite, but this is most frequently the case with the orthoclase. Crystals of quartz, often much corroded, but not unfrequently showing well-defined pyramidal forms, are common; and they generally contain fluid cavities, sometimes in very great abundance. One or two thin spicules of a dichroic mineral, probably mica, were seen, but only in one section. The most

remarkable feature presented by these felsites is the appearance of minute veins which traverse the felsite irregularly, not infrequently crossing and anastomosing with each other at all angles. They vary in width from a mere line up to $\frac{1}{10}$ or $\frac{1}{5}$ of an inch in diameter, and consist chiefly of quartz and felspar, apparently orthoclase. The quartz undoubtedly predominates, but here and there felspar, or saussurite which replaces the felspar, forms the chief ingredient. The quartz occurs in irregular crystalline aggregates and granules, often crowded with fluid cavities, and frequently containing enclosures of saussurite, and occasionally epidote. The felspar also forms irregular crystalline aggregates, but is most usually replaced by saussurite. Not infrequently it forms intergrowths with the quartz, so as to give the veins a micropegmatitic structure. Sometimes the walls of the veins are smooth and even; at other times the quartz and felspar (saussurite) seem to indent the walls, as in the so-called "segregation veins" of granite. The veins now described appear to be confined, as a rule, to the felsite, but occasionally they pass outwards from the latter into the adjacent greywacké.

III. The Old Red Sandstone Series.

The rocks assigned to the Old Red Sandstone are principally of igneous or aqueo-igneous origin. There is one small patch of conglomerate, however, which forms the upper portion of Bell Hill, near the village of Coldingham Shore. This is indicated on the Geological Survey's map as of Upper Old Red Sandstone age. I think it really belongs to the lower division of the series; at all events it is older than the bedded igneous rocks to be described presently. It rests directly upon the Lower Silurian, and is in fact composed exclusively of rounded fragments of greywacké, &c., derived from that formation. It nowhere overlies the igneous rocks referred to, nor does it contain any admixture of fragments of these. Its junction with them is, in short, a dislocation or fault, which has a downthrow to the N.E. (see fig. 1).

The bold headland of St Abb's is composed entirely of crystalline and fragmental igneous rocks, some of these being bedded and contemporaneous, and others amorphous and intrusive, while the igneous rocks at Coldingham Shore and Coldingham Bay appear to

be exclusively intrusive in character. I shall describe the rocks of those two areas separately, although they probably all belong to the same period of volcanic activity.

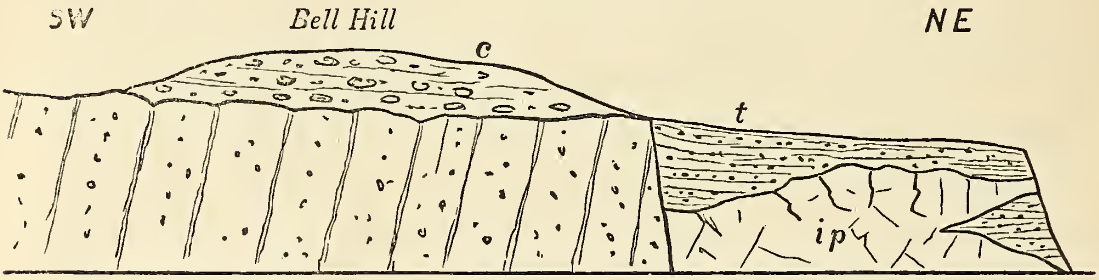


Fig. 1.—Section across Bell Hill, showing relation of Basement Conglomerate (*c*) to Igneous Rocks of St Abb's Head (*t*, *ip*).

(*a*) *Rocks of St Abb's Head.*

The headland of St Abb's Head extends from Pettico Wick south-east to the White Heugh—a picturesque cliff and favourite resort of sea-birds—about ¼ of a mile north-west of Coldingham Shore. The headland presents to the sea a bulwark of wild rugged precipices, indented with numerous little bays and coves, only a few of which are accessible from the land. It is separated from the rolling Silurian uplands behind by a well-marked hollow that extends south-east from Pettico Wick in the direction of the White Heugh. The headland, as thus defined, is described by the Geological Survey as consisting in the south-east partly of fragmental igneous rocks, and partly of intrusive "felstone"; while between Horsecastle Bay and Pettico Wick the area is coloured as intrusive felstone alone. Various sections laid bare since the time the ground was examined by the Geological Survey show that the northern part of the headland is made up chiefly of bedded porphyrite with some intercalated layers of tuff. Of the latter the only good exposures seen are at Pettico Wick Harbour and on the side of the road leading thence to the lighthouse. The dip of these rocks is clearly towards the south-east, the whole forming an ascending series, from Pettico Wick to the Wuddy Rocks, with an approximate thickness of 1200 feet (see fig. 2). Towards the south-east the beds are invaded by larger and smaller masses and dykes of intrusive crystalline rock. As the dip of the igneous series does not exceed 18° on an average, it is obvious that the junction between these rocks and the vertical Silurian

strata can only be a fault, having its downthrow to the north-east. This is the dislocation already referred to as bringing down the

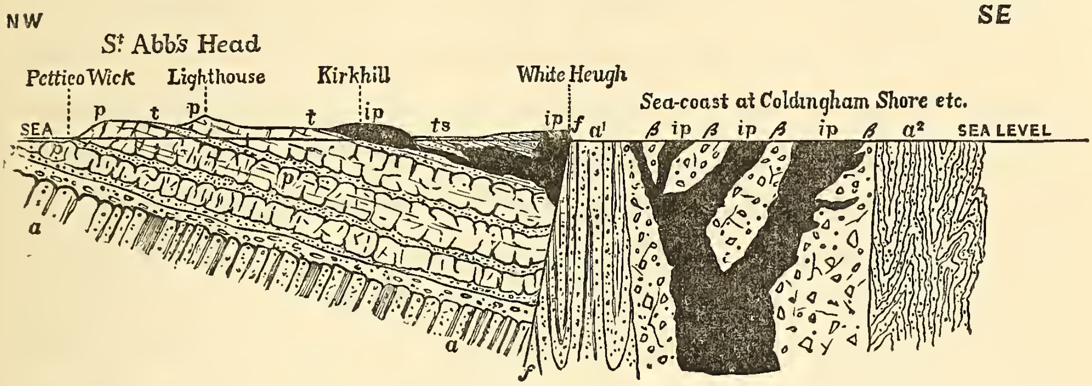


Fig. 2.—General Section across St Abb's Head to Shore of Coldingham Bay (scale 3 in. to 1 mile ; horizontal and vertical scale the same).

a, a, Probable position of Silurian strata under rocks of St Abb's Head. *b,* Conjectural position of basement conglomerate of Old Red Sandstone series. *c,* Inferred base of Volcanic series. *p, p, p,* Bedded porphyrites = andesitic lavas. *t, t, t,* Bedded porphyrite-tuff; at *ts* largely composed of small scoriæ. *β, β, β,* Agglomerate and tuff in volcanic neck. *ip, ip, ip,* Intrusive porphyrite. *f, f,* Fault at Halterem's Loup. a^1, Silurian strata somewhat altered ; a^2, Silurian strata much contorted and altered.

bedded igneous rocks of the Old Red Sandstone series against the basement conglomerate of Bell Hill. It is well seen at Rutherford Brae. Another fault, running at right angles to that just described, forms the boundary between the Old Red and the Silurian on the south side of Bell Hill. It is seen in section in the sea-cliffs at Halterem's Loup, where the Old Red Conglomerate is turned up at a high angle against the Silurian greywackés and shales (see fig. 3).

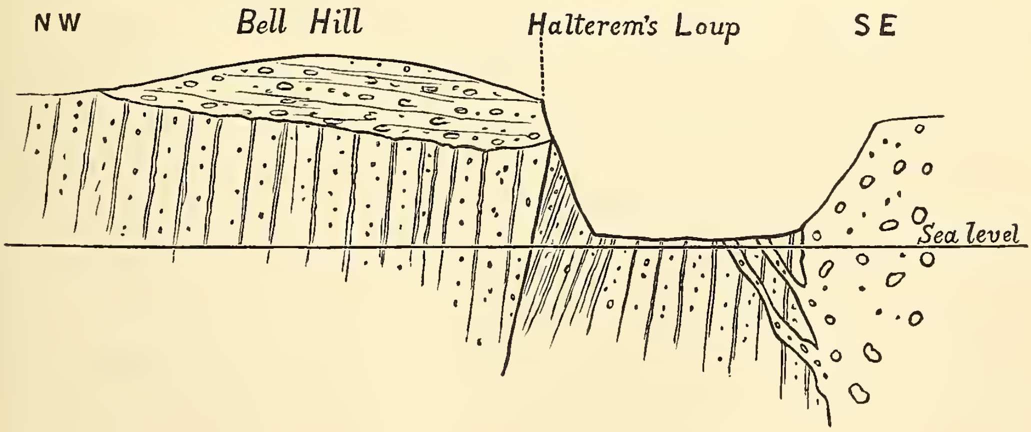


Fig. 3.—Section across Bell Hill, showing relation of Basement Conglomerate to the Silurian strata.

(1) *The Basement Conglomerate.*—The conglomerate of Bell Hill would thus appear to be the oldest member of the Old Red Sandstone series. It consists principally of water-worn shingle and gravel, set in a matrix of arenaceous and argillaceous matter. The stones, as mentioned already, have all been derived from the contiguous Silurian strata. The lower appear to be upon the whole coarser than the upper beds, stones 6 inches and more in diameter being common in the former, while the latter are rather conglomeratic and pebbly grit and sandstone than conglomerate. The beds dip towards the south-east, but are *turned up* against the N.E. and S.W. fault, while they *trend steeply down* towards the N.W. and S.E. fault. The series is probably about 100 feet in thickness.

(2) *The Igneous Series.*—The lowest beds seen on the north side of the N.W. and S.E. fault is a bedded porphyrite which is overlaid by a thick layer of agglomeratic tuff. Above this comes a succession of bedded porphyrites, about 250 feet or so in thickness, and these beds are succeeded by 40 to 50 feet of various tuffs, which dip in their turn under a second group of porphyrites. These last do not appear to exceed 250 or 300 feet in thickness, and are overlaid by some 400 feet of bedded tuffs.

Bedded Porphyrites.—These rocks having all the same character, one general description will suffice. They are for the most part fine-grained, purplish-blue or greyish-blue in colour, but frequently stained brown or red with much diffused ferric and hydrous ferric oxide. The joint-faces especially are often coated with hæmatite and limonite, while thin veins and threads of the same minerals are common. The rocks do not differ in general appearance from the porphyrites of Old Red Sandstone and Lower Carboniferous age which occur elsewhere in Scotland. They are, upon the whole, not so markedly porphyritic with plagioclase as the porphyrites of other districts, but closely resemble such fine-grained rocks as that of Blackford Hill, and similar rocks met with in the Braids and Pentlands. They are often highly scoriaceous and amygdaloidal above and below, and not infrequently contain, both in their upper and under portions, irregular areas of fine-grained tuff, consisting of amorphous, dust-like material, and comminuted débris and small lapilli of highly porous porphyrite (see fig. 4). In the upper parts of some of the old lava-flows this tuff appears

to fill up irregular clefts and vein-like cavites in the porphyrite, while in other places it appears involved in the porphyrite in such a way as to suggest that it may probably consist of portions of the shattered scoriaceous crust of the porphyrite, broken up and incorporated in the underlying mass while that was still in a fluid or pasty condition. Occasionally so much of this tuff-like matter is enclosed in the porphyrite that one is sometimes in doubt as to whether the whole rock is not fragmental. Microscopic examination, however, clearly shows that this is not the case—the angular and sub-angular lapilli and cinder-like fragments being completely surrounded by or embedded in finely crystalline rock. The porphyrites are all more or less weathered and earthy to some depth, and it is thus difficult to obtain very fresh fractures. They form a series of low broken escarpments facing the north-west, each escarpment marking the outcrop of a bed. The beds appear to be of variable thickness, some measuring about 15 feet, while others may reach as much as 50 feet or more.

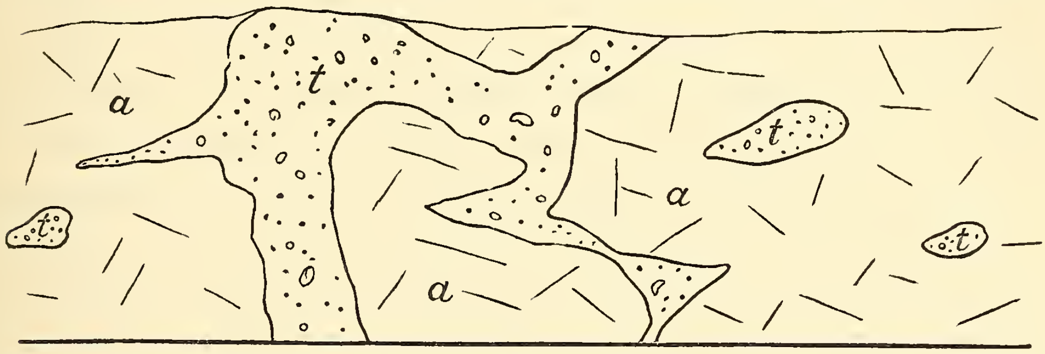


Fig. 4.—Red Tuff (*t*, *t*, *t*), enclosed in Porphyrite (*a*, *a*, *a*). Enclosures vary from an inch or less up to a foot or more in diameter.

Examined under the microscope, these rocks show a ground-mass of colourless microliths and minute lath-like crystals of plagioclase, diffused through which there is usually more or less non-differentiated red ferritic matter. In some cases the ground-mass seems to be composed chiefly of this unindividualised matter, with microliths and small rods of plagioclase scattered less abundantly through it. This is more especially the case with the amygdaloidal parts of the rock, where occasionally the ground-mass consists almost exclusively of non-differentiated matter, only a few recognisable microliths making their appearance. There can be little doubt that this unindividualised substance is simply the result of devitri-

fication, and that originally these rocks contained no inconsiderable proportion of glassy matter, especially in their upper and under portions. In a number of the sections examined, however, no trace of devitrified matter was observed, the ground-mass in such cases consisting of an aggregate of microliths and minute crystals of plagioclase, closely felted together, but containing interstitially abundant granules of ferrite, or magnetite passing into ferrite, along with minute granules of pale greenish-yellow, and yellow serpentinous matter and limonite, which are doubtless alteration products replacing hornblende or pyroxene, or both. The porphyritic ingredients of these rocks are seldom prominent. Small and large lath-like crystals of plagioclase are not uncommon, the larger crystals seldom exceeding 1 mm. in length, and they are mostly broken. They sometimes show fine zonal structure. The larger crystals seem to be most common when the ground-mass is composed chiefly of devitrified matter, with very minute microliths. Pseudomorphs after hornblende and augite appear more or less plentifully. These consist partly of yellow or greenish-yellow serpentine, sometimes veined with chrysolite, and partly of limonite. Not infrequently the pseudomorph is composed internally of serpentine, the outlines of the crystalline form being defined by limonite, or magnetite and limonite. Very often the limonite forms a meshwork of veins running through the serpentine, and occasionally these veins approximate in direction to the cleavage-planes of the original mineral. In many cases the shape of the pseudomorph gives one no hint as to whether the replaced mineral was augite or hornblende. Very often, however, the form is that of hornblende. This is specially the case with such pseudomorphs as have well-marked ferritic borders. In these one sees that the original mineral must have been broken, and more or less corroded, the ground-mass having often eaten into the heart of the crystal. A few pseudomorphs show very distinctly the form of augite, and some of these also contain inclusions of the ground-mass. Many of the pseudomorphs are quite amorphous, some composed almost entirely of serpentine, others almost exclusively of limonite: what proportion of these may represent hornblende, and what proportion augite, it is impossible to say. Of pseudomorphs having more or less definite forms, those after hornblende appear to be the most

numerous, and this is probably the case with the amorphous ones also. Fluidal structure is occasionally marked, the microliths and lath-like crystals of plagioclase being grouped round the larger porphyritic ingredients. In some of the rocks minute amygdaloidal cavities abound, and this even at a distance from the upper and under surfaces of the flows. The cavities are filled generally with calcite, or with calcite and quartz, or chalcedony. More rarely zeolites are present. The larger amygdules in the more scoriaceous portions of the rocks consist of the same minerals, calcite predominating. Most of the rocks are more or less deeply stained with red ferritic matter.

The tuffaceous areas in the porphyrites afford an interesting study. Occasionally they consist of broken or crushed scoriæ of one and the same kind of rock completely embedded in porphyrite. These small lapilli or scoriæ are generally finely and abundantly porous, and show a dark devitrified ground-mass in which minute microliths are more or less plentiful. They might quite well represent fragments of the more scoriacous and glassy portions of the same rock as that in which they are enclosed. In some places the tuffaceous matter consists of finely comminuted débris of the same or some closely similar rock, together with finer grit, and rounded pseudomorphs of serpentine and limonite after hornblende or pyroxene. The larger scoriæ and lapilli rarely exceed a hazel-nut in size. They are enclosed in the porphyrite in such a way as to show that this rock was in a fluid or pasty condition at the time they became embedded, for the crystalline and microlithic ground-mass lies between and among them. The tuffaceous areas now described appear to be confined to the upper and lower parts of the lava-flows, but they occasionally occur nearer the middle. They are generally distinguished from the rock in which they are enclosed by their deeper red colour, a character which they have in common with the beautiful red tuffs of Horsecastle Bay.

Bedded Tuffs.—These rocks present themselves amongst the porphyrites at three horizons, but it is quite possible that thin layers of similar fragmental materials, concealed by turf and superficial débris, may occur at other levels. A continuous section across the outcrops of the igneous series would probably show that each bed of porphyrite was underlaid by tuff or tuffaceous deposits. The

lowest bed of tuff is seen at Pettico Wick Harbour. This is a coarse tuff or agglomerate of fragments of porphyrites, which are generally dark purplish blue and red in colour, and more or less highly amygdaloidal. The stones are angular and subangular in form, and show no regular arrangement. They vary in size from small lapilli up to blocks of more than 1 foot across—some measuring 2 feet in diameter. Examined under the microscope, these porphyrites show the same structure and composition as those already described. The tuff rests upon a very irregular surface of porphyrite, and is overlaid, in a like irregular manner, by a bedded porphyrite, which is very vesicular and amygdaloidal at the line of junction. The junction is somewhat confused in places by minor slips and faults.

On the side of the road leading from Pettico Wick to the lighthouse various porphyrites are exposed,—some of which show the curious vein-like and irregular inclusions of fine tuff already described. These beds are overlaid by a considerable thickness of well-bedded shaley tuffs, generally red in colour—the tuff being fine-grained, and composed of comminuted débris of porphyrites. Some of the beds contain many lapilli of larger size scattered through their mass. They rest upon a dark purplish-blue amygdaloidal porphyrite, and are traversed intrusively by a thin sheet of fine-grained porphyrite. The junction is slightly confused, as at Pettico Wick, by faulting.

But by far the most extensive succession of tuffaceous strata is that which occurs at the south-east end of the headland of St Abb's. These rocks are generally well bedded, and have a prevalent red colour. They vary in texture from tuffaceous mudstones, in which only grit and small lapilli occur, up to coarse-grained tuffs, in which the included fragments may reach 1 foot or more in diameter. The most common rock is a tuff composed of comminuted débris and small lapilli, often not larger than a hazel-nut in size, but sometimes measuring 2 or 3 inches across. The grit and lapilli are of all shades of red, purple, yellow, and blue—the red strongly predominating—so that the resulting tuff is finely mottled. In many cases decomposition products, particularly calcite, permeate the rock in all directions—giving rise to irregular white splatches, veinings, and tangled thread-like areas—which show well on the warm red ground of the tuff. Other parts of the tuff might be described as a breccia of subangular and angular fragments, chiefly of amygdaloidal

porphyrites—the stones being set in a meagre matrix of comminuted débris. These rocks dip S.E. at 18° to 20°. They are abundantly pierced and traversed by dykes and irregular sheets and masses of porphyrite, which will be described later on. In some places, especially in the area extending from Raven's Brae to Horse Castle, the tuffs have been considerably baked and altered, so that it is sometimes difficult in hand-specimens to distinguish between what is tuff and what intrusive porphyrite. The fragmental character of the altered tuff, however, is quite apparent under the microscope.

The fragments in these tuffs appear to consist exclusively of varieties of porphyrite; at all events I could find no other rock. Most of the lapilli are vesicular and amygdaloidal—very many are highly so, and have all the appearance of scoriæ. The amygdules being usually white are generally very conspicuous. A microscopic examination of many of these included fragments shows that they consist exclusively of porphyrite—the ground-mass being generally vitreous, stony, or devitrified, but occasionally microlithic. The description given above of the small lapilli, which occur in the tuffaceous areas of the bedded porphyrites, holds equally true of the lapilli of these bedded tuffs. A great number of the small stones are evidently fragments of a vitreous scoriaceous porphyrite, and from their highly vesicular character they might well have floated in water at the time of their ejection—they are, in short, mere cinders. The fragments which show a microlithic ground-mass often contain also pseudomorphs after hornblende or pyroxene, and are evidently true porphyrites. Red ferritic matter saturates most of the stones; but not a few of these are dark grey or blue, and it is such fragments which show best the original vitreous character of the ground-mass. Here and there amongst the fine comminuted matter, or dust and grit, of the tuffs, small patches of serpentine and limonite occur, and these, in some cases at least, almost certainly represent hornblende or pyroxene.

The tuffs are thus composed essentially of volcanic detritus. In none of the specimens examined by me did I meet with any trace of ordinary terrigenous sediment. Even the finest-grained portions appear under the microscope to consist of minute fragments of porous scoriaceous vitreous rock. There is no admixture of quartz-grains and argillaceous matter, such as might have been derived from

the adjacent Silurian rocks. If such derivative matter occur at all it cannot be common, or I should hardly have missed it.

Intrusive Rocks.—Intrusive rocks, as already remarked, are well developed in the headland of St Abb's. These consist principally of a compact fine-grained blue porphyrite, which is often reddened with ferritic matter. It is a hard, tough rock, very irregularly jointed. Here and there it is sparingly porphyritic with "ferrite" —evidently pseudomorphous after pyroxene. The rock is a good deal weathered, and fresh specimens are not readily obtained. Under the microscope it shows a crypto-crystalline ground-mass—apparently composed entirely of small crystals of plagioclase, but owing to weathering the characteristic form and striation of the crystals are not well seen. There is no trace of non-individualised basis—nothing resembling the vitreous and devitrified basis of the bedded porphyrites. Scattered through this ground-mass occur occasional large crystals of plagioclase, and here and there more or less abundant pseudomorphs of limonite and serpentine after hornblende or pyroxene—the latter apparently being most common. Diallage, partially altered into limonite and serpentine, appears now and again; and probably many of the pseudomorphs just referred to may represent this mineral. Other decomposition-products diffused through the base in some sections are secondary quartz and chalcedony, and, in thin veins, calcite and hæmatite or limonite.*

(*b*) *Rocks of Coldingham Shore and Coldingham Bay.*

The igneous rocks of this area are separated from those of the region just described by a narrow belt of vertical Silurian strata which occupy the cliffs and foreshore between Halterem's Loup and the Long Carr. At Halterem's Loup the junction between the Old Red Sandstone and the Silurian is a well-marked fault. The grey-wackés are considerably hardened and shattered, more especially as they approach the igneous rocks of Coldingham Shore. At one or

* At Bell Hill the conglomerate is traversed by a dyke of mica-trap or minette. This rock shows under the microscope a micro-crystalline ground-mass full of felspar microliths and black magnetite dust. Scattered abundantly through this ground-mass are small scales and larger crystals of biotite, many of which are broken and twisted, and contain inclusions of the ground-mass. Orthoclase appears sparingly, and a few irregular crystalline granules of quartz are present. Magnetite is very plentiful.

two places, near their junction with the latter, patches of tuff and agglomerate occur upon the foreshore, as if resting directly upon the Silurian; but these patches are evidently of an intrusive character, and appear to occupy vertical fisures in the surrounding greywackés, which are extremely hardened and altered and very much reddened. The actual junction of these greywackés with the main mass of the igneous rocks presently to be described is likewise strongly suggestive of the intrusive character of the latter. Similar appearances mark the junction between these rocks and the Silurian greywackés on the shores of Coldingham Bay. At that place the greywackés are much jumbled, broken, and hardened, and saturated with red ferritic matter. The Old Red igneous rocks do not overlie, but are intrusive in the Silurian strata.

These igneous rocks consist partly of tuffs and agglomerates and partly of porphyrite. The fragmental rocks are, for the most part, quite unstratified, although here and there some trace of rude bedding may be noted. They are of a dull red colour, and are made up of angular and subangular fragments of all shapes and sizes up to blocks measuring over a yard in diameter. These are set in a matrix of smaller stones and comminuted débris. The whole appearance of this coarse tuff, which in many places is quite an agglomerate—that is to say, a coarse breccia of large stones and blocks—is similar to that of those tuffs and agglomerates which elsewhere in Scotland are found occupying the necks or throats of old volcanic orifices. All the fragments consist of varieties of porphyrite—a great many of which are precisely similar to the bedded porphyrites of St Abb's Head; others, however, are more markedly crystalline and porphyritic, especially with plagioclase felspar. A number of these fragments were examined under the microscope, but no new features of importance were disclosed. Highly amygdaloidal and scoriaceous fragments are common enough, but are not so strikingly prevalent as in the well-bedded tuffs of Horsecastle Bay, &c. One may say that, while in these latter scoriæ predominate and fragments of less porous rock do not abound, in the agglomerates of Coldingham Shore it is just the reverse. From the lapilli and blocks of this agglomerate, however, I did not succeed in getting any fresh specimens. Most of the rocks are much altered, so much so indeed that all one can determine is the fact that clouded crystals of plagioclase occur more or less

numerously in a dull ferritic base. Patches and irregular aggregates of ferric oxide are common enough also, and probably represent hornblende or pyroxene. The less altered specimens often show a devitrified base, crowded with microliths and small crystals of plagioclase. Scattered through this ground-mass are many larger crystals of plagioclase. Upon the whole, highly porphyritic rocks appear to be of more frequent occurrence in this agglomerate than they are in the bedded series of porphyrites and tuffs at St Abb's Head.

The unstratified agglomerate is irregularly traversed by masses of close-grained porphyrite, which has the same general character as the intrusive porphyrite near St Abb's Head. This porphyrite is, for the most part, much reddened with iron oxides, and considerably weathered, so that fresh fractures are not readily obtained. Where least weathered, it is a somewhat compact blue or purplish rock. Here and there plagioclase felspar is conspicuous as a porphyritic ingredient, and now and again pyroxene (augite or diallage) or pseudomorphs after pyroxene are visible. Thin veins and threads of limonite and hæmatite are common.*

IV. General Conclusions as to the Old Red Sandstone Series.

The basement beds of this series, consisting of the conglomerate of Bell Hill, are a mere fragment, and we cannot say much about the conditions under which they were accumulated. They show us, what indeed may be learned in many other parts of Scotland, that the Lower Silurian strata had already been folded, crumpled, and contorted, and excessively denuded before Old Red Sandstone times. As the conglomerates contain not a single fragment of igneous rock, it is most probable that their formation preceded the outbreak of volcanic action in this neighbourhood.

The coarse tuff and agglomerate of Coldingham Shore fill up an old volcanic orifice, from which the porphyrites and porphyrite-tuffs of St Abb's had previously been ejected. The great intrusions of porphyrite which penetrate, not only the agglomerate of the "neck," but the bedded tuffs and porphyrites, evidently belong to

* A dyke of basalt-rock crosses the intrusive porphyrites at the harbour, Coldingham Shore. Unfortunately, I neglected to bring away a specimen of the rock for closer examination. But I hope soon to revisit the district for further study, and to supply the omission in a subsequent paper to the Society.

a closing stage in the same period of volcanic activity. The dykes of Bell Hill and the harbour are certainly of much more recent date, and need not be considered in the present paper.

The south-east dip of the bedded igneous rocks of St Abb's I take to be due to the two large faults which form their boundary lines. The original inclination of the strata, which need not have been great, would necessarily be away from the neck of Coldingham Shore. It is quite impossible to say what the extent of these faults may be, but it is probably considerable, as the following considerations will show. Neither the top nor the bottom of the bedded rocks is seen, but the actual thickness displayed is not less than 1200 feet. At present the beds dip towards S.E. at about 15°. If, therefore, we take the beds at Pettico Wick to represent the basement beds of the igneous series, which they certainly are not, then, to restore the beds to horizontality, it is evident that the tuffs at the south-east end of the headland of St Abb's would require to be lifted up for some 1200 feet. But, on the supposition that all these bedded rocks have been derived from the volcanic neck at Coldingham Shore, and would at first, therefore, have a dip towards the north, it is obvious that the subsequent downthrow produced by the N.W. and S.E. fault must considerably exceed 1200 feet—say, 1300 or 1400 feet. A glance at the section seen at Pettico Wick, however, shows us that the beds there are not the basement beds of the series, for they are faulted down against the Silurian. The actual base of the series is submerged. If, therefore, the present S.E. dip of the porphyrites and tuffs be due mainly, as I believe, to the fault at present referred to, the downthrow of that dislocation must increase from N.W. to S.E., until it reaches not less than 1300 feet. To the effect produced by this fault we have to add that of the fault at Halterem's Loup, which has a downthrow to N.W., and has therefore had its share in bringing about the S.E. dip of the bedded porphyrites and tuffs. It is impossible, however, to form even an approximate estimate of the amount of downthrow produced by this latter fault. From the fact that it seems to cut off the other, no trace of which occurs in the rocks to the south, we may infer that its downthrow is considerable. Be that as it may, it is quite certain that the tuffs of Horsecastle Bay occupy a much higher horizon than the agglomerates exposed upon the beach at Coldingham Shore.

While the geological structure of St Abb's Head thus leads to the conclusion that the bedded tuffs and porphyrites formerly dipped in a northerly direction, and may thus have been ejected from the volcanic focus in their neighbourhood, the petrological evidence lends additional support to this conclusion. Fragments of precisely the same porphyrites as those of St Abb's Head occur abundantly in the rock at Coldingham Shore. A few blocks and fragments of greywacké were observed in the agglomerate, and may, perhaps, be more plentiful than they seem to be, for, of course, the agglomerate is only partially exposed; but with these exceptions all the stones I saw were porphyrites. Sections of the finer-grained portions of the tuff examined under the microscope showed in like manner that these are composed of the comminuted débris of porphyrites.

From the fact that the bedded porphyrite-tuffs of St Abb's Head have evidently been arranged by and accumulated under water, we may infer that the whole series, so far as that is exposed, is of subaqueous origin—that, in the rocks now described, we have the relics of an old subaqueous volcano of Old Red Sandstone times. The bedded porphyrites offer many analogies with those of the Cheviot Hills, the Sidlaws, and other regions of Old Red Sandstone volcanic rocks. They are, in fact, only altered andesites, which, in their microscopic structure, reproduce exactly what one sees in the andesites of the Western Territories of North America. The intrusive porphyrites, however, which intersect the agglomerate and the bedded rocks hardly resemble the intrusive masses of the Sidlaws, the Cheviots, &c., those of the Sidlaws being mainly diabase (altered basalt-rock), while those of the Cheviots are chiefly granite and felsite. Again, the bedded scoriæ-tuffs of St Abb's Head are hardly paralleled by any tuffs met with either in the Cheviots, the Sidlaws, or any other region of Old Red Sandstone volcanic rocks known to me. Of course, lapilli of highly porous and amygdaloidal porphyrites occur commonly enough in many of those districts, but nowhere, so far as I have seen, have we such a depth of fragmental materials consisting almost exclusively of scoriæ or cinders. These form the highest beds of the series, and possibly represent the latest ejections from the adjoining volcanic orifice. But they have suffered great denudation, and certainly attained a greater thickness, and overspread

a much wider area than they now occupy. This is evident from the appearance at the surface of the intrusive porphyrites near St Abb's Head. These, there can be little doubt, cooled and consolidated under the surface—the tuff which formerly covered them has been washed away. The intrusion of these porphyrites marks the closing stage in the volcanic activity of Old Red Sandstone times. It was not until after lavas and fragmental materials had ceased to be erupted, and the throat of the old volcano had become plugged with angular débris and blocks, that the rocks in question were injected. They evidently cooled under some pressure, but were probably not of very deep-seated origin. Whether they ever rose to the actual surface of the old volcano and overflowed, we cannot tell, for the whole of that surface has of course disappeared. It is obvious, however, that the molten masses from which they came must have been of essentially the same composition as that from which the bedded porphyrites were derived.

V. Glacial Phenomena.

The district described in this paper has been entirely overflowed by ice. This is proved by the generally glaciated contour of the rocks, which are smoothed from N.W. to S.E. The escarpments of porphyrites that faced the ice-flow have been bevelled off, and, notwithstanding the weathered character of the rocks, well-marked striæ are met with here and there, the trend of which is E. 35 S. Glaciated stones and clay occur in patches in the hollows of the headland, and a considerable mass of till occupies the hollow that separates the headland from the Silurian uplands. This is well exposed in section at Pettico Wick. It presents no features that require to be noted here, but is made up of the débris of rocks which have come from the west or north-west.

PRIVATE BUSINESS.

A ballot was taken, and the following gentlemen were duly elected Fellows of the Society:—Mr Alexander Goodman More, Alexander M. M'Aldowie, M.D., Mr James Hunter, Mr William Gilmour, Mr Herbert H. Ashdown, M.B., and Mr J. Mackay Bernard.

Monday, 18th April 1887.

SIR WILLIAM THOMSON, President, in the Chair.

Professor Rowland's Photographs of the Solar Spectrum were exhibited by the Astronomer-Royal for Scotland.

The following Communications were read:—

1. On Ship-Waves. By Sir W. Thomson.

2. On the Instability in Fluid Motion. By the Same.

3. Experimental Research in Magnetism. By Mr D. S. Sinclair. Communicated by Principal Jamieson.

4. On the Summation of certain Series of Alternants. By A. H. Anglin, M.A., LL.B., &c.

1. The summations referred to in the title depend on two theorems—one already known, the other not hitherto published. The first of these is to the effect that *the quotient of any simple Alternant by the difference-product of the variables is expressible as a determinant whose elements are sums of homogeneous products of the variables.* For example,

$$\begin{vmatrix} 1 & a^p & a^q & a^r \\ 1 & b^p & b^q & b^r \\ 1 & c^p & c^q & c^r \\ 1 & d^p & d^q & d^r \end{vmatrix} = \zeta^{\frac{1}{2}}(abcd) \begin{vmatrix} (p-1) & (p-2) & (p-3) \\ (q-1) & (q-2) & (q-3) \\ (r-1) & (r-2) & (r-3) \end{vmatrix},$$

where, generally, (n) denotes the sum of the homogeneous products of a, b, c, d of n dimensions.

The other theorem may be enunciated, in a particular instance, as follows :—

If three rows of four quantities each be taken, as

$$\begin{matrix} a_1 & a_2 & a_3 & a_4 \\ b_1 & b_2 & b_3 & b_4 \\ c_1 & c_2 & c_3 & c_4, \end{matrix}$$

and every possible determinant of the third degree be formed from this matrix by deleting the last column of a row and the first element of the other rows, the sum of these determinants is equal to the determinant got from the matrix by deleting the second column: thus

$$\begin{vmatrix} a_1 & a_2 & a_3 \\ b_2 & b_3 & b_4 \\ c_2 & c_3 & c_4 \end{vmatrix} + \begin{vmatrix} a_2 & a_3 & a_4 \\ b_1 & b_2 & b_3 \\ c_2 & c_3 & c_4 \end{vmatrix} + \begin{vmatrix} a_2 & a_3 & a_4 \\ b_2 & b_3 & b_4 \\ c_1 & c_2 & c_3 \end{vmatrix} = \begin{vmatrix} a_1 & a_3 & a_4 \\ b_1 & b_3 & b_4 \\ c_1 & c_3 & c_4 \end{vmatrix}$$

and a like series equal to the determinant got by deleting the third column, namely

$$\begin{vmatrix} a_1 & a_2 & a_3 \\ b_1 & b_2 & b_3 \\ c_2 & c_3 & c_4 \end{vmatrix} + \begin{vmatrix} a_1 & a_2 & a_3 \\ b_2 & b_3 & b_4 \\ c_1 & c_2 & c_3 \end{vmatrix} + \begin{vmatrix} a_2 & a_3 & a_4 \\ b_1 & b_2 & b_3 \\ c_1 & c_2 & c_3 \end{vmatrix} = \begin{vmatrix} a_1 & a_2 & a_4 \\ b_1 & b_2 & b_4 \\ c_1 & c_2 & c_4 \end{vmatrix}.$$

2. The proof of any case of this latter theorem depends on the case before it. Thus, taking the matrix

$$\begin{matrix} a_1 & a_2 & a_3 \\ b_1 & b_2 & b_3 , \end{matrix}$$

the identity for the case of determinants of the second order is

$$\begin{vmatrix} a_1 & a_2 \\ b_2 & b_3 \end{vmatrix} + \begin{vmatrix} a_2 & a_3 \\ b_1 & b_2 \end{vmatrix} = \begin{vmatrix} a_1 & a_3 \\ b_1 & b_3 \end{vmatrix} = | a_1 \ b_3 | \quad . \quad . \quad (1),$$

the truth of it being self-evident.

Turning to the case for determinants of the third order, and expanding each determinant in the left-hand side in terms of the elements of the first column, we see that the coefficient of a_1 is $| b_3 \ c_4 |$, while the coefficient of a_2 consists of the sum of two determinants which by (1) is equal to $| b_2 \ c_4 |$; and likewise for the coefficients of the elements involving b's and c's. Hence we get

$$\begin{vmatrix} a_1 & a_3 & a_4 \\ b_1 & b_3 & b_4 \\ c_1 & c_3 & c_4 \end{vmatrix} + \begin{vmatrix} a_2 & a_2 & a_4 \\ b_2 & b_2 & b_4 \\ c_2 & c_2 & c_4 \end{vmatrix};$$

and thus

$$\Sigma \begin{vmatrix} a_1 & a_2 & a_3 \\ b_2 & b_3 & b_4 \\ c_2 & c_3 & c_4 \end{vmatrix} = \begin{vmatrix} a_1 & a_3 & a_4 \\ b_1 & b_3 & b_4 \\ c_1 & c_3 & c_4 \end{vmatrix} = | a_1 \ b_3 \ c_4 | \quad . \quad . \quad (2);$$

and in like manner,

$$\Sigma \begin{vmatrix} a_1 & a_2 & a_3 \\ b_1 & b_2 & b_3 \\ c_2 & c_3 & c_4 \end{vmatrix} = \begin{vmatrix} a_1 & a_2 & a_4 \\ b_1 & b_2 & b_4 \\ c_1 & c_2 & c_4 \end{vmatrix} = | \, a_1 \;\; b_2 \;\; c_4 \, | \quad . \quad . \qquad (3).$$

3. Taking the next case, arising out of the matrix

$$\begin{matrix} a_1 & a_2 & a_3 & a_4 & a_5 \\ b_1 & b_2 & b_3 & b_4 & b_5 \\ c_1 & c_2 & c_3 & c_4 & c_5 \\ d_1 & d_2 & d_3 & d_4 & d_5 , \end{matrix}$$

consisting of four rows of five quantities each: we have to find the value of

$$\Sigma \begin{vmatrix} a_1 & a_2 & a_3 & a_4 \\ b_2 & b_3 & b_4 & b_5 \\ c_2 & c_3 & c_4 & c_5 \\ d_2 & d_3 & d_4 & d_5 \end{vmatrix} \text{ or } \Sigma_1 ,$$

where the suffix-order (1 2 3 4) occurs once in each determinant and Σ_1 consequently consists of four terms. Expanding the determinants as before, we see that the coefficient of a_1 is $| \, b_3 \;\; c_4 \;\; d_5 \, |$, while the coefficient of a_2 consists of three determinants whose sum by (2) is equal to $| \, b_2 \;\; c_4 \;\; d_5 \, |$; and similarly for the coefficients of the elements involving b, c, d. Hence we get

$$\begin{vmatrix} a_1 & a_3 & a_4 & a_5 \\ b_1 & b_3 & b_4 & b_5 \\ c_1 & c_3 & c_4 & c_5 \\ d_1 & d_3 & d_4 & d_5 \end{vmatrix} + \begin{vmatrix} a_2 & a_2 & a_4 & a_5 \\ b_2 & b_2 & b_4 & b_5 \\ c_2 & c_2 & c_4 & c_5 \\ d_2 & d_2 & d_4 & d_5 \end{vmatrix} ;$$

and thus

$$\Sigma_1 = | \, a_1 \;\; b_3 \;\; c_4 \;\; d_5 \, | \quad . \quad . \quad . \quad . \quad . \quad . \qquad (4).$$

Again, to find the value of

$$\Sigma \begin{vmatrix} a_1 & a_2 & a_3 & a_4 \\ b_1 & b_2 & b_3 & b_4 \\ c_2 & c_3 & c_4 & c_5 \\ d_2 & d_3 & d_4 & d_5 \end{vmatrix} \text{ or } \Sigma_2 ,$$

consisting of six terms, in each of which the suffix-order (1 2 3 4) occurs twice. Expanding in the same manner as before, it will be

seen that the coefficient of a_1 consists of the sum of three determinants which by (2) is equal to $|\,b_2\ c_4\ d_5\,|$, while the coefficient of a_2 likewise involves three determinants whose sum by (3) is equal to $|\,b_2\ c_3\ d_5\,|$. Similar expressions holding for the coefficients of the other elements in the first columns, we get

$$|\,a_1\ b_2\ c_4\ d_5\,| + |\,a_2\ b_2\ c_3\ d_5\,|;$$

and thus

$$\Sigma_2 = |\,a_1\ b_2\ c_4\ d_5\,| \quad . \quad . \quad . \quad . \quad . \quad . \quad (5).$$

Lastly, it may be shown in like manner that, in the case of

$$\Sigma \begin{vmatrix} a_1 & a_2 & a_3 & a_4 \\ b_1 & b_2 & b_3 & b_4 \\ c_1 & c_2 & c_3 & c_4 \\ d_2 & d_3 & d_4 & d_5 \end{vmatrix} \text{ or } \Sigma_3,$$

consisting of four terms, in each of which the suffix-order (1 2 3 4) occurs three times,

$$\Sigma_3 = |\,a_1\ b_2\ c_3\ d_5\,| \quad . \quad . \quad . \quad . \quad . \quad . \quad (6).$$

The results (4), (5), and (6) constitute the theorem in the case of determinants of the fourth order, and are formed by deleting successively the second, third, and fourth columns from the matrix out of which they arise.

4. Thus, assuming the truth of the identities for the case of determinants of the $(n-1)$th order, we can establish the corresponding results for the case of determinants of the nth order; that is to say, taking the matrix

$$\begin{matrix} a_1 & a_2 & a_3 & \ldots & a_{n+1} \\ b_1 & b_2 & b_3 & \ldots & b_{n+1} \\ c_1 & c_2 & c_3 & \ldots & c_{n+1} \\ \cdot & \cdot & \cdot & & \cdot \\ \cdot & \cdot & \cdot & & \cdot \\ l_1 & l_2 & l_3 & \ldots & l_{n+1}, \end{matrix}$$

consisting of n rows of $n+1$ quantities each, we can obtain the following $n-1$ results involving determinants of the nth order, and which are formed by deleting successively the second, third, fourth, . . . , nth columns from the matrix, viz.:—

$$\Sigma\begin{vmatrix} a_1 & a_2 & \dots & a_n \\ b_2 & b_3 & \dots & b_{n+1} \\ c_2 & c_3 & \dots & c_{n+1} \\ \cdot & \cdot & & \cdot \\ \cdot & \cdot & & \cdot \\ l_2 & l_3 & \dots & l_{n+1} \end{vmatrix} \text{ or } \Sigma_1 \quad = |\, a_1 \ b_3 \ c_4 \dots l_{n+1} \,|$$

$$\Sigma\begin{vmatrix} a_1 & a_2 & \dots & a_n \\ b_1 & b_2 & \dots & b_n \\ c_2 & c_3 & \dots & c_{n+1} \\ \cdot & \cdot & & \cdot \\ \cdot & \cdot & & \cdot \\ l_2 & l_3 & \dots & l_{n+1} \end{vmatrix} \text{ or } \Sigma_2 \quad = |\, a_1 \ b_2 \ c_4 \dots l_{n+1} \,|$$

$$\Sigma\begin{vmatrix} a_1 & a_2 & \dots & a_n \\ b_1 & b_2 & \dots & b_n \\ c_1 & c_2 & \dots & c_n \\ d_2 & d_3 & \dots & d_{n+1} \\ \cdot & \cdot & & \cdot \\ \cdot & \cdot & & \cdot \\ l_2 & l_3 & \dots & l_{n+1} \end{vmatrix} \text{ or } \Sigma_3 \quad = |\, a_1 \ b_2 \ c_3 \ d_5 \dots l_{n+1} \,|$$

. . . .

$$\Sigma\begin{vmatrix} a_1 & a_2 & \dots & a_n \\ b_1 & b_2 & \dots & b_n \\ \cdot & \cdot & & \cdot \\ \cdot & \cdot & & \cdot \\ k_1 & k_2 & \dots & k_n \\ l_2 & l_3 & \dots & l_{n+1} \end{vmatrix} \text{ or } \Sigma_{n-1} = |\, a_1 \ b_2 \ c_3 \dots k_{n-1} \ l_{n+1} \,|,$$

where, generally, Σ_μ consists of ${}_nC_\mu$ determinants in each of which the suffix-order $(1\ 2\ 3 \dots n)$ occurs μ times,—${}_nC_\mu$ denoting the number of combinations of n things taken μ together.

We now propose, with the help of these identities, to effect the summation of certain series of Alternants, obtaining results which may be called *Extensions* of the known theorem referred to at the outset.

1. Employing n letters $a, b, c, \ldots h, k, l$—starting with the case of two general indices y and z, we have

$$|a^0b^1c^2 \ldots h^{n-3}k^yl^z| = \zeta^{\frac{1}{2}}(abc \ldots l)\,|(y-n+2),\ (z-n+1)|,$$

which, for shortness, may be written in the form

$$[y,\ z] = |2,\ 1|\,\zeta^{\frac{1}{2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{A}).$$

Then

$$[y+1,\ z]+[y,\ z+1] = \begin{vmatrix} 3 & 2 \\ 2 & 1 \end{vmatrix}\zeta^{\frac{1}{2}} + \begin{vmatrix} 2 & 1 \\ 3 & 2 \end{vmatrix}\zeta^{\frac{1}{2}}$$

$$= \begin{vmatrix} 3 & 1 \\ 3 & 1 \end{vmatrix}\zeta^{\frac{1}{2}} = |3,\ 1|\,\zeta^{\frac{1}{2}} \quad . \quad . \quad . \quad (1),$$

which result is the only Extension of equation (A).

Again, in the case of three general indices x, y, z, writing the equation

$$|a^0b^1c^2 \ldots g^{n-4}h^xk^yl^z| = \zeta^{\frac{1}{2}}(abc \ldots l)\,|(x-n+3), (y-n+2), (z-n+1)|$$

in the form

$$[x,\ y,\ z] = |3,\ 2,\ 1|\,\zeta^{\frac{1}{2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{B}),$$

we have

$$[x+1,\ y,\ z]+[x,\ y+1,\ z]+[x,\ y,\ z+1] = \mathrm{S}_1\zeta^{\frac{1}{2}},$$

where

$$\mathrm{S}_1 = \begin{vmatrix} 4 & 3 & 2 \\ 3 & 2 & 1 \\ 3 & 2 & 1 \end{vmatrix} + \begin{vmatrix} 3 & 2 & 1 \\ 4 & 3 & 2 \\ 3 & 2 & 1 \end{vmatrix} + \begin{vmatrix} 3 & 2 & 1 \\ 3 & 2 & 1 \\ 4 & 3 & 2 \end{vmatrix}.$$

Expanding each determinant in terms of the elements of the first column, the coefficient of $x-n+4$ is $|2,\ 1|$, while that of $x-n+3$ consists of the sum of two determinants which by (1) is equal to $|3,\ 1|$; and similarly for the coefficients of the elements involving the other indices.

Hence

$$\mathrm{S}_1 = \begin{vmatrix} 4 & 2 & 1 \\ 4 & 2 & 1 \\ 4 & 2 & 1 \end{vmatrix} + \begin{vmatrix} 3 & 3 & 1 \\ 3 & 3 & 1 \\ 3 & 3 & 1 \end{vmatrix},$$

and thus

$$\Sigma[x+1,\ y,\ z] = |4,\ 2,\ 1|\,\zeta^{\frac{1}{2}} \quad . \quad . \quad . \quad . \quad . \quad (2).$$

Further, we have

$$[x+1,\ y+1,\ z]+[x+1,\ y,\ z+1]+[x,\ y+1,\ z+1] = \mathrm{S}_2\zeta^{\frac{1}{2}},$$

where

$$\mathrm{S}_2 = \begin{vmatrix} 4 & 3 & 2 \\ 4 & 3 & 2 \\ 3 & 2 & 1 \end{vmatrix} + \begin{vmatrix} 4 & 3 & 2 \\ 3 & 2 & 1 \\ 4 & 3 & 2 \end{vmatrix} + \begin{vmatrix} 3 & 2 & 1 \\ 4 & 3 & 2 \\ 4 & 3 & 2 \end{vmatrix},$$

which, in the same manner as before, may be shown to be equal to

$$\begin{vmatrix} 4 & 3 & 1 \\ 4 & 3 & 1 \\ 4 & 3 & 1 \end{vmatrix} + \begin{vmatrix} 3 & 3 & 2 \\ 3 & 3 & 2 \\ 3 & 3 & 2 \end{vmatrix},$$

and thus

$$\Sigma[x+1,\ y+1,\ z] = |\,4,\ 3,\ 1\,|\,\zeta^{\frac{1}{2}} \quad . \quad . \quad . \quad (3).$$

These two results are the Extensions of equation (B), and are formed by deleting successively the second and third figures from the series 4, 3, 2, 1; that is to say, the second and third columns from the matrix

$$\begin{matrix} x-n+4, & x-n+3, & x-n+2, & x-n+1 \\ y-n+4, & y-n+3, & y-n+2, & y-n+1 \\ z-n+4, & z-n+3, & z-n+2, & z-n+1. \end{matrix}$$

2. Again, in the case of four general indices u, x, y, z, writing the equation

$$|a^0b^1c^2\ldots f^{n-5}g^uh^xk^yl^z| = |(u-n+4),(x-n+3),y-n+2),(z-n+1)|\,\zeta^{\frac{1}{2}}$$

in the form

$$[u,\ x,\ y,\ z] = |\,4,\ 3,\ 2,\ 1\,|\,\zeta^{\frac{1}{2}}, \quad . \quad . \quad . \quad . \quad (C),$$

we have

$$\Sigma[u+1,\ x,\ y,\ z] = S_1\zeta^{\frac{1}{2}},$$

Σ consisting of four terms in each of which there is one index of the form $\lambda+1$, and where

$$S_1 = \begin{vmatrix} 5&4&3&2 \\ 4&3&2&1 \\ 4&3&2&1 \\ 4&3&2&1 \end{vmatrix} + \begin{vmatrix} 4&3&2&1 \\ 5&4&3&2 \\ 4&3&2&1 \\ 4&3&2&1 \end{vmatrix} + \begin{vmatrix} 4&3&2&1 \\ 4&3&2&1 \\ 5&4&3&2 \\ 4&3&2&1 \end{vmatrix} + \begin{vmatrix} 4&3&2&1 \\ 4&3&2&1 \\ 4&3&2&1 \\ 5&4&3&2 \end{vmatrix}.$$

Expanding these determinants as before, it may be shown by the application of equation (2) that

$$S_1 = |\,5,\ 3,\ 2,\ 1\,| + |\,4,\ 4,\ 2,\ 1\,|;$$

and we thus have

$$\Sigma[u+1,\ x,\ y,\ z] = |\,5,\ 3,\ 2,\ 1\,|\,\zeta^{\frac{1}{2}} \quad . \quad . \quad . \quad (4).$$

Further, we have

$$\Sigma[u+1,\ x+1,\ y,\ z] = S_2\zeta^{\frac{1}{2}} \text{ suppose,}$$

where Σ consists of six terms in each of which there are two indices

of the form $\lambda+1$, and where by equations (2) and (3) it will be found that

$$S_2 = |\,5,\ 4,\ 2,\ 1\,| + |\,4,\ 4,\ 3,\ 1\,|;$$

and consequently

$$\Sigma[u+1,\ x+1,\ y,\ z] = |\,5,\ 4,\ 2,\ 1\ \zeta^{\frac{1}{2}} \quad . \quad . \quad . \quad (5).$$

Lastly, it may be shown in like manner that

$$\Sigma[u+1,\ x+1,\ y+1,\ z] = (\,|\,5,\ 4,\ 3,\ 1\,| + |\,4,\ 4,\ 3,\ 2\,|\,)\,\zeta^{\frac{1}{2}}$$
$$= |\,5,\ 4,\ 3,\ 1\,|\,\zeta^{\frac{1}{2}} \quad . \quad . \quad . \quad . \quad . \quad (6).$$

The results (4), (5), and (6) are the Extensions of equation (C), and their right-hand members are formed by deleting successively the second, third, and fourth figures from the series 5, 4, 3, 2, 1; that is, by deleting these columns successively from the corresponding matrix. We may further observe that if we increase by unity *one* index in the left-hand side of (C), the sum of the resulting alternants is obtained by increasing by unity the elements of *one* column in the right-hand side, thus giving the identity (4); while an increase in two and three indices produces respectively a corresponding increase in the elements of two and three columns, thus furnishing equations (5) and (6).

3. To obtain the Extensions involving any number (ν) of indices we should thus assume the corresponding results for $\nu-1$ indices, and then deduce those for ν.

Now, in the case of $n-3$ general indices $r,\ s,\ t,\ \ldots\ z$, we have

$$|\,a^0 b^1 c^2 d^r e^s \ldots l^z\,| = |\,(r-3),\ (s-4),\ \ldots,\ (z-n+1)\,|\,\zeta^{\frac{1}{2}},$$

which may be written in the form

$$[r,\ s,\ t,\ \ldots\ z] = |\,3,\ 4,\ 5,\ \ldots\ n=1\,|\,\zeta^{\frac{1}{2}},$$

the Extensions of which are

$$\Sigma[r+1,\ s,\ t,\ \ldots\ z] = |\,2,\ 4,\ 5,\ \ldots\ n-1\,|\,\zeta^{\frac{1}{2}} \quad . \quad . \quad . \quad . \quad (1)'$$
$$\Sigma[r+1,\ s+1,\ t,\ \ldots\ z] = |\,2,\ 3,\ 5,\ \ldots\ n-1\,|\,\zeta^{\frac{1}{2}} \quad . \quad . \quad . \quad (2)'$$
$$\Sigma[r+1,\ s+1,\ t+1,\ u,\ \ldots\ z] = |\,2,\ 3,\ 4,\ 6,\ \ldots\ n-1\,|\,\zeta^{\frac{1}{2}} \quad (3)'$$

. . . .

while generally

$$\Sigma_\mu = |\,2,\ 3,\ 4,\ \ldots\ \mu+1,\ \mu+3,\ \ldots\ n-1\,|\,\zeta^{\frac{1}{2}} \quad . \quad (\mu)'$$

and lastly,

$$\Sigma[r+1,\ s+1,\ \ldots\ y+1,\ z] = |\,2,\ 3,\ 4,\ \ldots\ n-3,\ n-1\,|\,\zeta^{\frac{1}{2}} \ldots (n-4)'$$

where, in general, Σ_μ consists of ${}_{n-3}C_\mu$ terms in each of which there are μ indices of the form $\lambda+1$.

To deduce the Extensions involving $n-2$ general indices $q, r, s, \ldots z$ from these $n-4$ results,—we have

$$|a^0b^1c^qd^r \ldots l^z| = |(q-2), (r-3), \ldots, (z-n+1)|\zeta^{\frac{1}{2}},$$

that is, in the above notation,

$$[q, r, s, \ldots z] = |2, 3, 4, \ldots n-1|\zeta^{\frac{1}{2}} \quad . \quad . \quad (D).$$

Then, if

$$\Sigma[q+1, r, s, \ldots z] \text{ or } \Sigma_1 = S_1\zeta^{\frac{1}{2}},$$

on writing down the determinants in S_1 and expanding them in terms of the elements of the first column in each, it may be shown by equation (1)′ that

$$S_1 = |1, 3, 4, \ldots n-1| + |2, 2, 4, 5, \ldots n-1|,$$

and thus

$$\Sigma_1 = |1, 3, 4, \ldots n-1|\zeta^{\frac{1}{2}} \quad . \quad . \quad . \quad . \quad (1).$$

Again, if

$$\Sigma[q+1, r+1, s, \ldots z] \text{ or } \Sigma_2 = S_2\zeta^{\frac{1}{2}},$$

we shall find in like manner by equations (1)′ and (2)′ that

$$S_2 = |1, 2, 4, 5, \ldots n-1| + |2, 2, 3, 5, \ldots n-1|,$$

and consequently

$$\Sigma_2 = |1, 2, 4, 5, \ldots n-1|\zeta^{\frac{1}{2}} \quad . \quad . \quad . \quad . \quad (2),$$

while by the application of (2)′ and (3)′ it may further be shown that

$$\Sigma[q+1, r+1, s+1, t, \ldots z] \text{ or } \Sigma_3 = |1, 2, 3, 5, \ldots n-1|\zeta^{\frac{1}{2}} \,. \; (3).$$

Generally, to find the corresponding value of Σ_μ, having ${}_{n-2}C_\mu$ terms in each of which there are μ indices of the form $\lambda+1$,—suppose

$$\Sigma_\mu = S_\mu\zeta^{\frac{1}{2}}.$$

Now since in Σ_μ there are ${}_{n-3}C_{\mu-1}$ terms with the index $q+1$, and ${}_{n-3}C_\mu$ terms with the index q, on writing down and expanding the determinants in S_μ as before, it will be seen that the coefficients of $q-1$ consists of the sum of ${}_{n-3}C_{\mu-1}$ determinants which by equation $(\mu-1)'$ is equal to $|2, 3, 4, \ldots \mu, \mu+2, \ldots n-1|$, while the coefficient of $q-2$ consists of the sum of ${}_{n-3}C_\mu$ determinants which by equation $(\mu)'$ is equal to $|2, 3, 4, \ldots \mu+1, \mu+3, \ldots n-1|$; and

likewise for the coefficients of the elements involving the indices $r, s, t, \ldots z$. Hence we have

$$S_\mu = | 1, 2, 3, \ldots \mu, \mu + 2, \ldots n-1 |$$
$$+ | 2, 2, 3, \ldots \mu + 1, \mu + 3, \ldots n-1 |,$$

and thus

$$\Sigma_\mu = | 1, 2, 3, \ldots \mu, \mu + 2, \ldots n-1 | \zeta^{\frac{1}{2}} \quad . \quad . \quad (\mu).$$

Lastly, the corresponding value of

$$\Sigma[q+1, r+1, \ldots y+1, z] \text{ or } \Sigma_{n-3}$$

is deduced in like manner by the use of equation $(n-4)'$, when we get

$$\Sigma_{n-3} = | 1, 2, 3, \ldots n-3, n-1 | \zeta^{\frac{1}{2}} \quad . \quad . \quad (n-3).$$

The results (1), (2), (3), . . . $(n-3)$ are the Extensions of equation (D), and their right-hand members are formed by deleting successively the second, third, fourth, . . . , $(n-2)$th figures from the series 1, 2, 3, . . . $n-2$, $n-1$; that is, by deleting these columns successively from the corresponding matrix. And we may further notice that the coefficients of $\zeta^{\frac{1}{2}}$ in these $n-3$ identities (corresponding respectively to an increase by unity in 1, 2, 3, . . . $n-3$ indices), are also formed by increasing by unity the elements of 1, 2, 3, . . . $n-3$ columns respectively of the determinant $| (q-2), (r-3), (s-4), \ldots, (z-n+1) |$.

5. Note on Cobaltic Alums. By Mr Hugh Marshall, B.Sc.

From a peculiar change of colour observed in the electrolysis of a solution of copper-cobalt potassium sulphate, the author was recently induced to make some experiments on the behaviour of cobalt sulphate solution when electrolysed in such a manner that the reducing product of the decomposition could not act on the solution. For this purpose a divided cell was used, so that the two electrodes were practically in separate vessels. The apparatus will be fully described in a future communication.

It was found that an acid solution of cobalt sulphate alone is not changed when submitted to electrolysis in the apparatus. If, however, ammonium or potassium sulphate be also present, the solution passes through a series of changes of colour, ultimately becoming greenish-blue, and this, it was found, is due to oxidation

of the cobalt from the cobaltous to the cobaltic state. This seemed to point to the probable formation of an alum. Experiments were therefore made in order to obtain crystals. The action was allowed to go on for several days, so as to concentrate the liquid; when, in the solution with ammonium sulphate, dark blue crystals separated out. These were octahedral, showing also faces of the cube and rhombic dodecahedron. The analysis of these showed that they were ammonium-cobalt alum.

		Theory.
Co_2O_3	17·87	17·05
$(NH_4)_2O$	5·18	5·36
SO_3	32·81	33·02
H_2O (By Diff.)	44·14	44·57
	100·00	100·00

When thoroughly dried the crystals are not unstable, but when dissolved in water are quickly reduced. The solution in sulphuric acid is not so soon decomposed. When the acid has been completely removed from the crystals, water seems to resolve them first into the constituent sulphates. The oxygen liberated during decomposition is partially in the form of ozone.

No satisfactory result was at first obtained with the potassium sulphate solution, chiefly owing to the slight solubility of that salt. If sufficient be not added the alum is decomposed. If, on the other band, too much be added, it crystallises out. The happy medium can only be obtained by the addition of one or other salt. After several experiments, crystals were obtained similar to those of the ammonium alum, but they could not be separated from the potassium sulphate with which they were mixed. Afterwards a finely crystalline deposit was obtained, which on analysis appeared to be the alum mixed with potassic sulphate.

Under similar conditions, nickel sulphate undergoes no change whatever.

Other salts of cobalt undergo oxidation in presence of corresponding alkali salts, forming evidently double salts; these are at present undergoing examination.

6. On the Effect produced on the Polarisation of Nerve by Stimulation. By Mr G. N. Stewart.

In this short paper I wish merely to give a summary of the chief results of experiments made by me in February and March last in the Physiological Institute of Berlin. Du Bois Reymond has recently made an elaborate investigation of the galvanic polarisation of muscle. Many years ago he made similar observations on nerve. The question which occurred to me was, How does tetanus affect the amount of polarisation in nerve? Apparently there has been no previous work at this subject. It is not necessary to describe here the arrangements used, nor the manner of making the observations, as a detailed account, with discussion of the facts brought out, will appear in the *Journal of Physiology*. When a current is passed through a living nerve, the polarisation produced may be either negative or positive. When it is negative, the polarisation current is, of course, in the opposite direction to the polarising current. Positive polarisation gives a current in the same direction as the polarising stream. It depends upon the "density" of the polarising current, and upon the time of flow, whether the deflection due to the polarisation is purely negative, purely positive, or of double sign. When it is of double sign, there is first a negative kick, which is followed by a more persistent positive deflection. As a matter of fact, there is every ground for believing, that in general the two polarisations exist side by side in the polarised nerve.

The influence of stimulation on the polarisation, so far as my experiments go, may be stated thus:—

1. *In every case the effect produced by stimulation is in the direction of diminution of the positive polarisation.*—One says "in the direction of," because increase of negative polarisation would equally well explain the result. It is probable, however, for reasons which need not be given here that it is the positive polarisation which is affected.

2. *The effect is, within limits, greater the longer the time of flow of the polarising stream.*—This has only been shown for weak currents, because strong currents depress the vitality of the nerve when they are allowed to flow for any length of time. If, in the case of a weak

polarising stream, a curve be constructed whose ordinates correspond to amount of change of the polarisation, the abscissæ representing time of flow, this curve is found to have a maximum; *i.e.*, when the current is kept constant, the effect first increases with the time, and then diminishes. Here the stimulus is, of course, also kept constant.

3. *The effect increases, within limits, with the density of the polarising stream.*—The curve plotted with current density as abscissa, and amount of stimulation effect as ordinate, also reaches a maximum, when the time of flow is kept constant.

4. *The effect is less, as might be expected, the longer the interval between breaking the polarising current and closing the galvanometer circuit.*—The persistency of it is sometimes, however, astonishing.

5. *The effect increases with the strength of the stimulus.*—Result No. 1 has an important bearing on Hermann's explanation of the apparent diminution of resistance in a tetanised nerve, which I propose to discuss at another time.

I must add that, in the case of a strong current, result No. 4 does not always hold, the nerve apparently requiring some time to regain its maximum of excitability.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. XIV. 1886-87. No. 124.

Monday, 2nd May 1887.

Sir DOUGLAS MACLAGAN, Vice-President, in the Chair.

The following Communications were read :—

1. The Objective Cause of Sensation. Part III.—The Sense of Smell. By Prof. John Berry Haycraft.

The end-organs of the special senses are all built up on the same type. The history of their development from simple ectodermic cells suggests that similar agencies have been at work to produce them. Both sapid and odorous substances, and indeed all gaseous and liquid molecules, are now known to be in constant vibration, and this vibration is more or less characteristic of the substance examined.

The above considerations have led me, for the last five years, to teach that, in all probability, it will be possible to connect *quality* of taste and smell with the *kind* of vibrating stimulus, and that it will be possible to demonstrate, as has already been done in the case of sight and hearing, the truth of this general statement—that quality of sensation will depend (the sensorium being in a normal condition) upon the kind or character of the vibrating stimulus. There is nothing very new in this idea. Without seeking for its germs at an earlier period, we find it clearly enunciated by both Hobbes and Hartley ; and in more recent times Mr Herbert Spencer has lent the weight of his great authority in the same direction. But of experimental proof, without which we cannot rest content,

nothing has been advanced. Casual allusions to the probable or possible relationship between the tastes or smells of bodies and their chemical natures are sometimes though rarely found in the text-books of physiologists, but nothing more. While so much important work has been carried on during the last few years by Helmholtz, Preyer, Maxwell, Rayleigh, and others, in connection with both sound and sight, no one, until quite recently, has turned his attention to the investigation of either taste or smell.

In a most interesting and suggestive article in *Nature* (June 22, 1882), Prof. Ramsay brought forward many facts tending to demonstrate the dependence of *smell* upon the vibratory motion of odorous particles. He drew attention to the fact that many gases and vapours of low specific gravity—their molecules vibrating therefore with great rapidity—are perfectly odourless, and he saw in this an analogy to the rapid vibrations of the ultra-violet rays of the spectrum, and the rapid vibrations of an insect's wing, both incapable of producing any impression on the eye and ear. He also described classes of substances alike in chemical and physical properties, such as the alcohols or fatty acids, as having generic smells; the higher members of the groups producing sensations more powerful and characteristic than those of the lower ones. It will be my endeavour in the present paper to extend more fully this inquiry, and to demonstrate by experimental methods the fact that smell, like sight and hearing, depends for its production on the vibrations of the stimulating medium, the quality of the sensation depending, in all cases, upon the kind of vibration which produces it.

In a paper read before the British Association in 1885, and printed subsequently in the *Proceedings of the Royal Society of Edinburgh* (1886), I was able clearly to demonstrate these points for the sense of taste. That paper and the present one will be found to run on exactly parallel lines; one is almost a recapitulation of the other, for what is true of taste is also true of smell. In order to avoid unnecessary recapitulation, I have touched lightly on many questions more fully discussed in the other paper, which should therefore be consulted.

An investigation into the odorous properties of substances is to a certain extent limited, as many of them are without smell, especially those found in the inorganic world. In a description of odours one

is met with this difficulty, that there is no nomenclature familiar to every one. Hundreds of terms expressing the well-known colours of familiar objects, enable one to describe by a single term almost any tint and shade. We have cardinal, rose, magenta, maroon, carmine, crimson, scarlet, and a dozen other shades of red alone, and all of these can be expressed by words. The smells, however, and especially those of the chemist's museum, are so unfamiliar, and often so peculiar, that we are forced to speak of them simply as the odours of the substances which produce them, or to say that they are like, though never identical, with that of some other and better known substance.

No two observers quite agree in their descriptions of a given odour, and the information readily at hand in the text-books, but culled from a hundred sources, is therefore not reliable. I have, for this reason, availed myself freely of the kindness of my colleague Prof. Tilden and my friend Prof. Ramsay, who have placed at my disposal their private collections of chemical compounds. In almost every case the description of a smell given in this paper is derived from personal observation.

In the first place, let us study those few substances found among inorganic compounds which have distinct smells. It is well known that many substances, like arsenic, chlorine, sulphur, bromine, and their compounds, have characteristic odours. Can we associate the odours of these substances with any chemical or physical properties they may possess, and show that when similar odours are produced by two or more substances, then we have some similar chemical or physical property present at the same time?

In recent years a remarkable discovery of Newlands has opened up a fresh point of departure in the science of chemico-physics. His observations led him to formulate a law which he termed the law of octaves. Lothar Meyer, Mendelejeff, and Carnelley, extending his work, have shown that the "periodic-law," as it is now called, is one of vast application and importance. The nature of this periodic-law is now so well known, thanks to the many recent publications of Professor Carnelley, that it would be superfluous to attempt more than roughly to sketch out its main features. If we arrange the elements in the order of their atomic weights, beginning with that which has the lowest, and passing to that which has the highest, we

shall find a periodic recurrence of property or function in the series. The first element is a monad, the second a dyad, the third a triad, and the fourth a tetrad. Then we find the fifth a triad, the sixth a dyad, and the seventh once more a monad. Then follows a second series of seven elements, showing the same variation in atomicity; this repeats itself right through the list of elements. This periodic recurrence of function is seen not only in the case of atomicity, but it may be also observed in the atomic volumes, the fusibility and the electrical and other properties of the elements. There is then a general resemblance in physical properties between the first, eighth, fifteenth, &c., and between the second, ninth, and sixteenth elements. Mendelejeff has arranged the elements in the convenient tabular form given on the opposite page, which indicates these and some other important facts.

Those elements which resemble one another, and which we can pick out by taking every eighth one from that one from which we elect to start, form what he calls a "group," and are arranged vertically. The sets of seven elements each, arranged horizontally, form twelve "series."

There is yet another point of importance. The elements of a "group," which are in an even "series," are especially related to one another; so in like manner elements in an odd series of the same group are similarly allied. Thus Li, Na, K, Cu, Rb, Ag, Cs, Au, have all these properties in common; but in this group Na, Cu, Ag, Au are most alike, and Li, K, Rb, and Cs, in like manner, are most closely related.

In the paper to which I have already alluded I was able to demonstrate the fact that elements in the same group are capable of producing similar or related tastes. The power of producing a given taste is then a property which, like the ordinary physical qualities of the elements, follows the periodic law. As will now be shown, the same obtains for smell.

In studying the facts of the case, let us start with Group VI. We find here, in odd series, three well-known substances whose compounds have strong and characteristic odours. Sulphuretted, seleniетted, and teluretted hydrogen have all a disagreeable odour like that of rotten eggs. The compounds of the elements of this group with methyl and ethyl are disagreeable and alliaceous. In

TABLE OF NATURAL CLASSIFICATION OF ELEMENTS—*After Mendelejeff.*

Groups.	I.	II.	III.	IV.	V.	VI.	VII.	Group VIII.
Series.	Monads.	Dyads.	Triads.	Tetrads.	Triads or Pentads.	Dyads or Hexads.	Monads or Heptads.	
1.	H = 1	...	...	...	...	...	...	
2.	Li = 7	Be = 9	B = 11	C = 12	N = 14	O = 16	F = 19	
3.	Na = 23	Mg = 24	Al = 27	Si = 28	P = 31	S = 32	Cl = 35·5	
4.	K = 39	Ca = 40	Sc = 44	Ti = 48	V = 51	Cr = 53	Mn = 55	Fe = 56. Co = 59. Ni = 59.
5.	Cu = 63	Zn = 65	Ga = 69	...	As = 75	Se = 79	Br = 80	Ru = 104. Rh = 104. Pd = 106.
6.	Rb = 85	Sr = 87	Y = 89	Zr = 90	Nb = 94	Mo = 96	...	
7.	Ag = 108	Cd = 112	In = 113	Sn = 118	Sb = 120	Te = 125	I = 127	
8.	Cs = 133	Ba = 137	La = 139	Ce = 142	Di = 147	...	...	
9.	...	...	...	...	Er = 166	...	...	
10.	...	...	Yb = 173	...	Ta = 182	W = 184	...	Os = 193. Ir = 193. Pt = 195.
11.	Au = 197	Hg = 200	Tl = 204	Pb = 207	Bi = 210	...	...	
12.	...	...	...	Th = 234	...	U = 240	...	

Group VII. chlorine, bromine, and iodine have very similar smells, and so have the acids they form with hydrogen, and their compounds with methyl, ethyl, ethylene, &c. Although similar in all cases, yet they are not the same. One can distinguish the odour of iodine from that of bromine or chlorine. It may be described as having more flavour, and not so chlorous. Bromine is like them both, having an odour intermediate between the two in quality. From a study of the above substances, this is seen to hold good in all cases, the odour uniformly changing, often to a slight degree, as we pass from the lowest to the highest member of a group. A very marked change is seen in the formyle compounds of Group VII. Chloroform has a fragrant and characteristic smell. So has bromoform, but it has something else in addition, which is recognised as being the odour of iodoform. Bromoform thus connects chloroform and iodoform, these latter substances being very unlike one another.

There may then be so little difference in the odours of compounds of the same group of elements that they may with difficulty be distinguished. On the other hand, the differences may be great, there being intermediate sensations produced by intermediate members of the group. One is forcibly reminded of the changes in sensation experienced in allowing the eye to traverse the spectrum from one end towards the other. In a drawn-out spectrum, only a part of which is visible, one passes, say, from orange into yellow, and these colours are recognised as being different, and at the same time alike. In a shorter spectrum the eye may pass from the yellow into the red. The two sensations are quite different, but the orange is seen to connect them. The importance of the above statements—and they may be verified in the case of the few odorous compounds in Group V.—will become apparent when they are placed in juxtaposition with identical facts mentioned in the previous paper on taste, and some recent observations of Professor Carnelley.

In those groups of elements whose compounds are sapid, we find that the same change in taste sensation is apparent as we pass from lower to higher members of the group. Let us take as an example Group VII., which furnishes us both with sapid and odorous bodies

GROUP VII.

Element.	Potassium Compound.	Formyle Compound.
F	Salt and *saline.*	...
Cl	Salt, saline, *bitter.*	Characteristic odour of chloroform.
Mn	——	——
Br	Salt, saline, and *bitter.*	Intermediate odour.
I	Saline, bitter.	Characteristic odour of iodoform.

Potassium fluoride is salt—like common salt in taste. In addition it is slightly saline—like nitre. Chloride of potassium has a suspicion of a bitter taste as well, and so has potassium bromide. Potassium iodide has lost the taste of common salt, being a saline, and bitter.

So far, then, we have seen that the power of producing taste is a property or function of elements. Our knowledge of matter is derived from its power of producing sensation within us. In the case of sight and hearing we have associated quality in sensation with the kind or character of the vibrating stimulus. If it can be shown that elements belonging to the same "group" are capable of vibrating in a way which is similar or related to one another, then we have grounds upon which to draw an analogy between smell, taste, sight, and hearing.

On account of our incomplete knowledge of the ultra-red and ultra-violet regions of the spectrum, a final answer to this question cannot perhaps be given. Only rough indications are to hand, but these point in the same direction.

The chlorides of the alkaline earths have spectra which are nearly related. The spectra of Group I. are not dissimilar, especially potassium and rubidium, with the five groups of lines. Then, again, the chlorides, bromides, and iodides of calcium and barium are similar, *the lines shifting towards the red end of the spectrum* in a way which is nearly proportional to the increase of atomic weight.

The colour of a substance is an index to the pitch of the vibrations of its molecules. In a paper on the colour of chemical compounds recently published in the *Philosophical Magazine* (July 1874), Professor Carnelley demonstrated the existence of a relationship between the salts of the same group of elements in respect to colour. The salts, say the chlorides, of a group of

metals may not be of the same colour, but we find that, in passing to higher members of the group from the lower ones, a uniform *change* in colour is to be observed. This change is produced by a gradual shifting of the absorption towards the red end of the spectrum, the molecules vibrating more and more slowly with increase of atomic weight. This can be illustrated by the following diagram taken from his paper:—

Metal.	Cl	I	Metal.	CrO_4	AsO_4
Na	White	White	Mg	Lemon yellow	White
Cu	White	Cream	Zn	Yellow	White
Ag	White	Light yellow	Cd	Orange yellow	White
Au	Yellow white	Golden yellow	Hg	Red	Yellow

It is probable, then, that metals of the same group vibrate in a similar way. This vibration we know is complex, consisting of many wave-lengths of different pitch. When a metal vibrates, and one of its vibrations falls within the scale of the visible spectrum, we shall find the corresponding wave of another member of the same group in the neighbourhood. If a higher member of the group, it will absorb the light nearer the red end; if a lower, nearer the blue end of the spectrum. Together with this alteration of pitch, we have corresponding alterations in the sensations produced, whether they be of sight, taste, or smell. Whatever reason we have for associating quality of colour with the pitch of vibration, we shall likewise have for associating quality of smell and taste with the same physical cause.*

Amongst organic substances many are so closely allied that they fall into distinct classes or groups. Thus we have the fatty acids, alcohols, &c. If these be arranged in homologous series, commencing with that which has the lowest, and passing to that which has the highest molecular weight, a uniform change will be observed in many physical properties on ascending the series. Thus the lower ones may be gaseous, the middle ones liquid, and the higher ones solid.

* If a curve be constructed in which the ordinates represent the atomic weights of the positive elements, and the abscissæ a chromatic scale arising from blue, green, &c., to black, we shall obtain a curve indicating that the colours of the compounds are a periodic function of the elements arranged in atomic series. This is well seen in the case of the normal iodides (Carnelley).

Professor Ramsay examined several of these groups, and came to the following conclusions:—In the first case, that the smell of a group was generic; and, in the second case, that the smell became more distinct, and gained in flavour in ascending from the lower to the higher members. My observations are not quite in accordance with the first statement, for I do not believe any uninitiated person would find any resemblance, say, between the odours of ethyl alcohol and octyl alcohol, or of acetic and valeric acids, which would prompt him in any way to class them together. I find that in ascending the organic series, as in ascending one of Mendelejeff's groups, the odour changes. This change may be slight, so that it may be said with truth that there is a generic smell belonging to the series; or, more frequently, the change is so great that it is only by a study of intermediate members that any continuity of sensation can be made out.

These statements may be verified by a study of the following tables. They are not as complete as I could have wished, owing to my inability to obtain some of the rarer acids and alcohols.

Monatomic Alcohols.

$CH_3.OH$	Methyl alcohol	=	Faint alcoholic odour.
$C_2H_5.OH$	Ethyl ,,	=	Alcoholic odour.
$C_3H_7.OH$	Propyl ,,	=	Alcoholic odour with flavour.
$C_4H_9.OH$	Isobutyl ,,	=	Flavour becomes more marked, and the alcoholic odour less and less.
$C_5H_{11}.OH$	Amyl ,,	=	
$C_8H_{17}.OH$	Octyl ,,	=	

The term "flavour" expresses very badly what is meant. It is only possible by experiment to become acquainted with the nature of a smell.

Among the fatty acids another flavour equally characteristic gradually supersedes the acetic odour of the first two members of the group.

Fatty Acids.

$CHO.OH$	Formic acid	=	Acetic odour.
$C_2H_2O.OH$	Acetic ,,	=	Acetic odour.
$C_3H_5O.OH$	Propionic ,,	=	Acetic, together with a flavour.

$C_4H_7O.OH$ Butyric acid	=	Slightly acetic, with a well-marked flavour.
$C_5H_9O.OH$ Valeric ,,	=	No longer acetic, the flavour alone is present.

In this case the smell has altogether changed in character. The same holds good with the following series.

Acetates.

$C_2H_5.C_2H_3O_2$ Ethyl acetate	=	Acetic and ethereal smell.
$C_3H_7.C_2H_3O_2$ Propyl ,,	=	Acetic ethereal, with a flavour.
$C_4H_9.C_2H_3O_2$ Butyl ,,	=	Slightly acetic, with a pine-apple flavour.
$C_5H_4.C_2H_3O_2$ Amyl ,,	=	Pine-apple flavour, and not acetic.

If we pass to quite another group, the hydrocarbons, and starting with benzene, replace first one, then two, and finally three atoms, with methyl, the ethereal aromatic odour will be found progressively to change in a manner which it is impossible to describe, but which can readily be demonstrated.

Hydrocarbons.

C_6H_6	Benzene	Have a progressing and aromatic and ethereal odour.
$CH_3.C_6H_5$	Methyl benzene	
$2(CH_3).C_6H_4$	Dimethyl benzene	
$3(CH_3).C_6H_3$	Trimethyl benzene	

The way in which the sensation changes, analogous to that observed in studying Mendelejeff's groups, can in a similar way be explained on the vibration hypothesis.

I am not aware of any odorous organic series possessing at the same time colour, although some of them have very weak absorption bands. From a study of these latter, and from inferences drawn from a study of other coloured series, it is possible to obtain an insight into the state of vibrational activity of the substances in the tables above. Dr W. J. Russell has investigated the absorption bands of ammonia, alcohol, &c. These substances absorb light, but to so slight an extent that long columns of the liquids have to be examined before the bands are distinctly seen. Under these conditions, ammonia gives several distinct and characteristic bands.

If now an atom of hydrogen of the ammonia be replaced by methyl, the ammonia bands are still visible, but they are shifted somewhat towards the red end of the spectrum. Replacing the hydrogen by the larger molecule of ethyl, the bands are seen to pass still nearer to the end of the spectrum.

NH_3	Ammonia	Produce bands which shift to red end of spectrum in ascending the series.
$CH_3.NH_2$	Methylamine	
$C_2H_5.NH_2$	Ethylamine	

In the same way we find that common alcohol possesses absorption bands, seen also in the higher members of the group, but shifting towards the red end of the spectrum in ascending the group.

$C_2H_5.OH$	Alcohol	Produce bands which shift to the red end of the spectrum in ascending the series.
$C_3H_7.OH$	Propyl alcohol	
$C_4H_9.OH$	Butyl alcohol	

In the case of coloured acids, such as chromic and picric acids, the salts too are coloured. If the bands of these acids be examined, and if they be then converted into salts, the absorption will shift towards the red end of the spectrum. It seems that the molecule, having a certain vibrational character depending upon its structure, is weighted by the added metal, the vibrations of which do not probably appear at all in the visible spectrum, and, in consequence, its pitch is lowered. If an odorous substance like acetic acid be combined with another odorous substance, it is generally possible to detect the two intermingled sensations in the compound. Ethyl acetate is ethereal and acetous at the same time. Allyl sulphide is like allyl alcohol, and has the odour of a sulphur compound as well. In this case it is probable that those vibrations in each substance which produce smell are not so much lowered in pitch by the new substance with which they are combined as to change the character of the sensations they are each capable of producing. The ammonia vibrations are shifted towards the red end of the spectrum in methylamine, but not enough to produce another sensation. In other compounds of two odorous substances it may not be possible to distinguish the original odours, and for the reason that the pitch has shifted, as we have seen it often does, so as to produce quite a different sensation.

It may be urged as an objection to some of these conclusions, that the same odours are often produced by substances, chemically speaking, quite unlike each other. Thus benzoic aldehyde smells very much like nitrobenzene. In the case of taste, too, there are many examples of totally dissimilar bodies having indistinguishable acid or sweet tastes. In answer to this objection one has only to remember that there are instances, equally numerous, of very different substances which produce the same colour sensations. One may produce exactly the same tint with either a chromate, a picrate, or an aniline dye. It would be strange, indeed, if among the complex vibrations of a compound, or even of an element, some tones were not of the same pitch, as some of the vibrations of substances quite dissimilar in general properties. When these tones fall within the scale of the visible spectrum, the scale of taste, or smell sensations, we have, according to the vibration theory, a similar sensation produced.

In this paper I have endeavoured to avoid all questions which are matters of speculation. I have dealt only with already ascertained facts, or those which can readily be verified. I do not attempt to offer any hypothesis to account for the action of vibrating matter on the olfactory end-organs. It may or may not be a mechanical or a chemical action. This question is not raised. We know next to nothing as to how it is that ether vibrations stimulate the cones of the retina, still less can we guess at the action of vibrating atoms and molecules of ordinary matter on the sensitive end-organs of the nose. My aim has been to establish the fact that, just as we have reason to connect differences in colour sensations with differences in the vibration of the ether, so, in like manner we have reason to connect differences in smells with differences in the vibrations which call them into existence. This analogy is established upon the following grounds:—

(1) In passing from the lower to the higher members of one of Mendelejeff's groups, such molecular vibrations as have been investigated tend to become lower in pitch. At the same time the colour, taste, and smell sensations alter in character when present.

(2) In passing from the lower to the higher members of an organic series, such as the alcohols, such molecular vibrations as have been investigated tend to become lower in pitch. When pre-

sent, the colour, taste, and smell sensations alter in character in the manner that I have described.

2. On the Physics of Noise. By Professor Crum Brown.

(*Abstract.*)

The noises considered in this paper are uniform, continuous noises, such as the fricatives of articulate speech : f, θ, s, שׁ, χ, &c. These sounds are considered by the author to stand in a similar relation to musical tones as lights with continuous spectra do to lights with bright-line spectra. Methods were proposed for analysing these uniform continuous noises, and also for imitating them by synthetic means.

3. On the Physical Properties of Methyl-Alcohol. By Professor Dittmar and C. A. Fawsitt, Esq.

4. On the Instability of the Double Sulphates $M''SO_4.R'_2SO_4 + 6H_2O$ of the Magnesium Series. By W. Dittmar.

By a number of observations made incidentally in the preparation of two of the double salts referred to in the heading, namely, the compounds of sulphate of potash with sulphate of magnesia and sulphate of ferrous oxide respectively, I had long come to suspect that these *two* salts at any rate are *not* perfectly stable in opposition to water. To settle the question, I have caused Mr James Robson and Mr Andrew Hodge, two young chemists working in my laboratory, to inquire into the matter by systematic experiments. These were, in general at least, conducted according to the following scheme :—Starting from a known weight of sulphate of potash, this was dissolved in a proportion of hot water,* less than sufficient to hold the intended double salt in solution after cooling ; there was then added a known weight of the di-valent sulphate amounting to exactly 1 or

* In the case of $FeSO_4$ the water was acidified with a few drops of sulphuric acid to prevent precipitation of ferric compounds.

1·1 or 1·2 . . . 1·5 times MgO or FeO per $1K_2O$ used, the solution filtered hot into a basin, allowed to crystallise, and the crop of crystals produced examined.

The results were similar in both cases, but I prefer to state them in reference to the magnesia salt, because our experiments in regard to it were more exhaustive than those with the iron salt.

The crystals deposited from a solution containing one or even a little more than one MgO per $1K_2O$ may look perfectly normal *en masse*, but when examined more closely are invariably found to be contaminated with a powdery coating which looks like, and indeed substantially consists of, sulphate of potash. As the proportion of magnesia increases, the relative quantity of free sulphate of potash produced gets less and less, and at last vanishes altogether. With 1·3, sometimes even with 1·2 times MgO per $1K_2O$, we generally obtained normal-looking crystals, which contained the correct percentage of water, and consequently consisted, substantially at least, of the pure, unmixed double salt.

Similar results were obtained in the case of the ferrous double salt; in its case we determined the percentage of iron (by permanganate) as a test for the degree of purity.

In neither case, however, have I been able as yet to determine the exact conditions under which a given solution is sure to deposit perfectly normal crystals. Whether or not depends not merely on the ratio between the weights of the two bases, but also on the proportion of water, the temperature at which the crystals separate out, and other independent variables.

I refrain from giving any further details, intending to complete the investigation, to extend it to other double salts of the magnesium series, and also to the alums, and then to present a complete and detailed memoir.

5. A Diatomaceous Deposit from North Tolsta, Lewis. By John Rattray, Esq.

PRIVATE BUSINESS.

Mr H. A. Webster, Mr John S. Yeo, Mr John Cockburn, Dr A. S. Cumming, and Mr J. W. Capstick were balloted for and declared duly elected Fellows of the Society.

Monday, 16th May 1887.

Lord M'LAREN, Vice-President, in the Chair.

The following Communications were read:—

1. On the Increase of Electrolytic Polarization with Time. By W. Peddie, B.Sc.

(*Abstract.*)

This paper dealt only with cases in which the electromotive force used is insufficient to produce decomposition. In such cases the electrodes act as condensers. But the observed law of variation of current-strength with time is not exactly that which holds in the charging of ordinary condensers. The author showed that the deviation could be largely accounted for by the continual increase of transition resistance, the existence of which he proves in a separate paper on that subject.

2. On the Blood of *Myxine*. By Professor D'Arcy W. Thompson.

3. On the Larynx and Stomach in Cetacea. By Professor D'Arcy W. Thompson.

4. On Transition Resistance at the Surface of Platinum Electrodes, and the Action of Condensed Gaseous Films. By W. Peddie, B.Sc. (Plate VII.)

The question of the existence of true transition resistance at the surface of certain electrodes in given liquids has been in debate for many years, but has never yet been conclusively settled. It is admitted universally that in certain cases such resistance does occur, as, *e.g.*, when a non-conducting oxide is formed on the surface of the metal; but the point in dispute is whether or not it occurs

when no direct chemical action takes place between the metal and the products of electrolysis, or between it and substances dissolved in the liquid. The great difficulty in all experimental inquiry regarding the resistance of electrolytes is the difficulty of distinguishing between the effects of true resistance and the effects of true reverse electromotive force of polarization. For, if E be the direct electromotive force producing a current x in a conducting circuit which includes an electrolyte and has resistance R, while e is the type of the polarization electromotive force, so that $\Sigma(e)$ is the total reverse electromotive force, we have

$$E - \Sigma(e) = Rx .$$

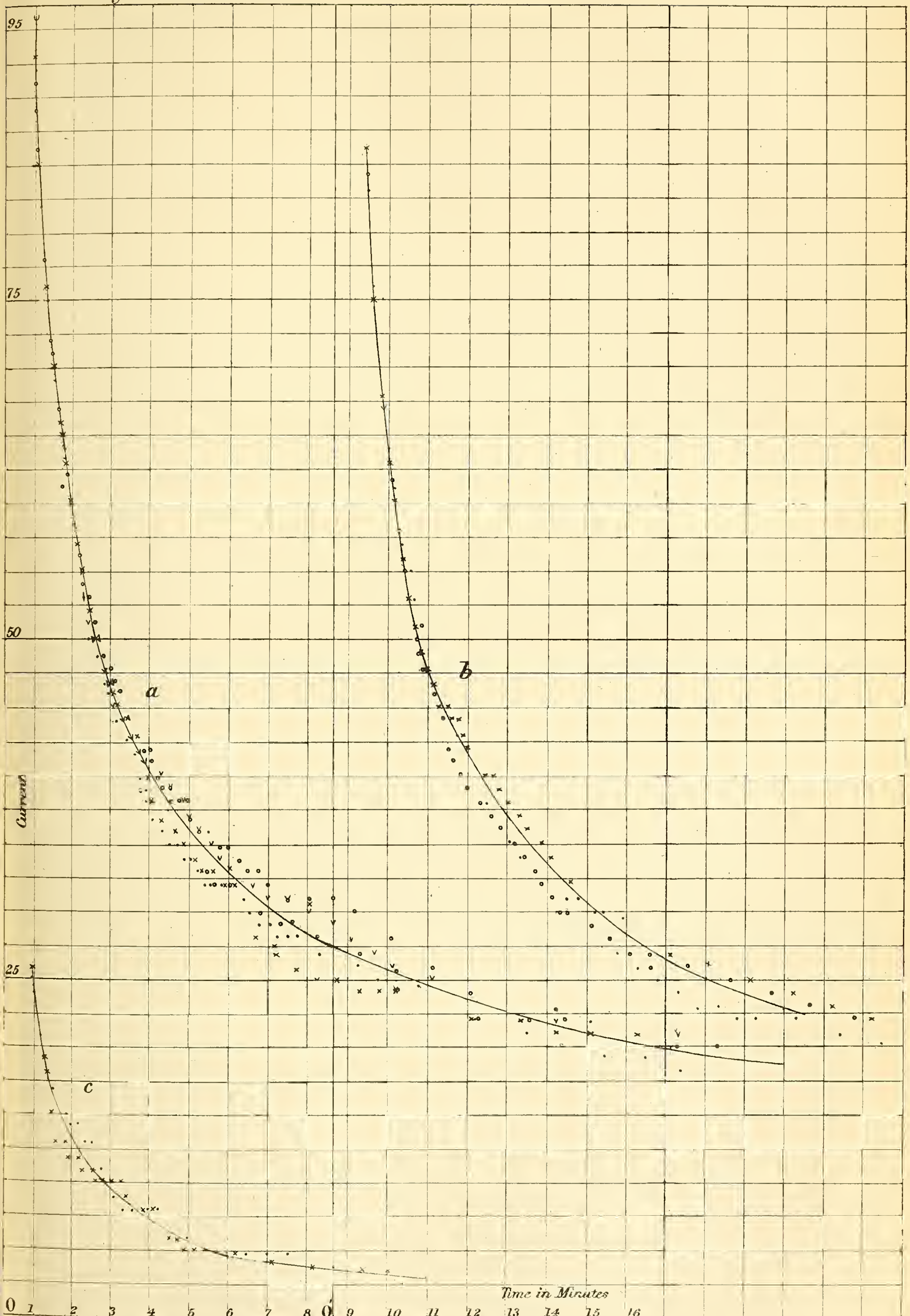
But, if e contains a term proportional to x, we can write this equation in the form

$$E - \Sigma(e') = (R + \Sigma(p))x ,$$

where p is the constant of proportionality. This shows that the value of the resistance in the circuit, as determined by any of the ordinary methods, may include a term which does not correspond to a true resistance actually existing. Hence, usually, in measuring the resistance, polarization is prevented by the use of alternating currents—as in Kohlrausch's method; or an attempt is made to eliminate its effects by keeping it constant while the resistance is varied—as in Horsford's method. And, in addition to this, resistance at the surfaces of the electrodes must be distinguished from other resistance in the circuit. Attempts have been made to do this by means of measurements of the heat developed at the surfaces; but this method is unsatisfactory, as there may be development of heat at the surfaces from other causes than the presence of resistance.

Method of Observation.

In making experiments on the law connecting current-strength and time, when one Daniell cell was placed, along with a galvanometer, in circuit with an electrolytic cell having platinum electrodes and containing a solution of sulphuric acid, I noticed that the result was very different according as the platinum plates had been newly heated to redness or had been left for some hours in the liquid. In the latter case the strength of current at a given time was, *cæteris paribus*, much weaker than in the former (see first series of experi-



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ments, and Plate VII.). This was most probably due to alteration of resistance or to alteration of capacity of the electrodes regarded as condensers. To settle the point, I used a Helmholtz galvanometer capable of indicating a current of the one ten-millionth part of an ampère. It was necessary to use an instrument of such sensibility; for the current, although it started with a comparatively large value, very rapidly fell to an exceedingly small fraction of its original strength because of polarization. Evidently the initial value of the current-strength is independent of polarization; and this constitutes the great advantage of the present method. In order to bring the first deflection of the index on the scale, it was necessary to shunt the galvanometer. Of course, the deflection could not be read with accuracy for some time after joining the battery in the circuit because of oscillations of the galvanometer needle; but, by proper shunting, the interval could be made as small as 10 seconds when required. Readings of the deflection (to which the current-strength is proportional) were taken at short intervals, and the results exhibited graphically. The curve drawn through the points so obtained could easily be continued backwards to cut the axis parallel to which current was measured, thus giving the initial value. From this the amount of resistance in the circuit could be obtained. I did not, however, actually determine the resistance in this way. I obtained the curve for the case when the electrodes had been left unheated for some time; then I washed the plates and heated them to redness, and repeated the experiment under such conditions as to obtain a curve coinciding practically with the former. The results showed that in all cases a considerable additional resistance had to be placed in the circuit to produce coincidence. This shows conclusively that a transition resistance exists; and also gives an estimate of its amount, for the transition resistance must be equal to the resistance added in the second case. It is necessary to remark that the resistance of the electrolyte was the same in both cases; and, further, its total amount was very small in comparison with the resistance of the rest of the circuit. Since only one Daniell cell was used, the liquid was not decomposed, and the current-strength after two or three seconds was only of the order of thousandths or ten-thousandths of an ampère. Hence, although the temperature coefficient of the resistance of the liquid is large, it was quite im-

possible that any appreciable rise of temperature could occur, the mass of liquid being very large.

Experiments.

In the first series of experiments no additional resistance was placed in the circuit when the plates had been heated. The results obtained are shown graphically in Plate VII. The series of points marked *a* give the results when the plates were newly heated. Those marked *b* give the results when the plates had been left in the liquid for half an hour after heating and performing the experiments corresponding to *a*. Those marked *c* were obtained after the plates were left in the liquid for a number of hours—about twenty, more or less. A Tray-Daniell cell was used so as to give constant electromotive force. Half a minute was allowed to elapse before taking the first reading.

Experiment 1*a*.—Plates heated.

Deflection at intervals of 10 secs.—92, 82, 75, 69, 66, 64, 59, 56, 53, 50, 49, 47, 46, 44, 43, 42, 40, 39, 38, 37, 36, 35, 35, 34, 34, 33.

At intervals of $\frac{1}{4}$ min.—32, 32, 32, 31, 30, 29, 29, 28, 28, 28.

At intervals of $\frac{1}{2}$ min.—27, 27, 26, 25, 24, 24, 23.

At intervals of 1 min.—22, 21, 20, 19, 19, 18.

Experiment 1*b*.—Plates connected by a copper wire for 30 min.

10 secs. interval.—83, 70, 65, 60, 56, 53, 50, 48, 46, 45, 43, 42, 41, 40, 39, 38, 37, 37, 36.

$\frac{1}{4}$ min. interval.—35, 35, 34, 33, 32, 31, 30, 30, 29, 29.

$\frac{1}{2}$ min. interval.—28, 27, 26, 25.

1 min. interval.—24, 23, 21, 20, 20, 19.

Experiment 2*a*.—Plates heated.

At intervals of 10 secs.—93, 85, 76, 65, 63, 60, 57, 55, 52, 50, 48, 47, 46, 45, 44, 43.

$\frac{1}{4}$ min. interval.—40, 38, 37, 36, 35, 34, 33, 33, 32, 32.

$\frac{1}{2}$ min. interval.—28, 27, 26, 25, 25, 24, 24, 24.

1 min. interval.—22, 21, 21, 20, 20, 20.

Experiment 2b.—Plates connected for 30 min.

10 secs.—83, 76, 67, 64, 61, 57, 55, 53, 51, 49, 47, 46, 44, 43, 42, 41, 40.

¼ min.—38, 36, 35, 34, 34, 33, 33, 32, 31, 31, 30, 30.

½ min.—27, 26, 25, 24, 23, 23, 22, 22.

1 min.—22, 21, 20, 19, 19, 19, 19.

Experiment 2c.—Plates connected for about 20 hours.

10 secs.—24, 21, 18, 17, 15, 15, 14, 14, 13, 13.

¼ min.—11, 9, 8, 8, 8, 8, 7, 7.

½ min.—6, 5, 5, 5, 5, 5.

1 min.—4, 3, 3.

Experiment 3a.—Plates heated.

10 secs.—91, 86, 78, 72, 67, 62, 60, 58, 55, 53, 51, 49, 48, 47, 46, 44, 43, 42, 41, 40, 39, 39, 38, 38, 37.

¼ min.—36, 36, 35, 35, 34, 33, 33, 32.

½ min.—31, 31, 31, 30.

1 min.—28, 26, 24, 24, 23, 22, 21, 20, 20.

Experiment 3b.—Plates connected for 30 min.

10 secs.—86, 75, 68, 63, 60, 56, 53, 51, 49, 48, 47, 45, 45, 44, 43, 42.

¼ min.—40, 40, 39, 38, 37, 36, 35, 34.

½ min.—32, 32, 31, 30.

1 min.—27, 26, 25, 24, 23, 22.

Experiment 3c.—Plates connected for about 20 hours.

10 secs.—26, 21, 19, 15, 13, 13, 12, 12, 11, 11, 10, 10, 10, 10.

¼ min.—9, 8, 8, 7, 6, 6, 5, 5.

½ min.—5, 5.

1 min.—4, 4, 4.

Experiment 4a.—Plates heated.

10 secs.—96, 85, 76, 70, 66, 62, 59, 56, 53, 51, 50, 48, 47, 45, 44, 43, 42, 41, 41.

$\frac{1}{4}$ min.—40, 39, 38, 37, 36, 35, 34, 33, 32, 32.
$\frac{1}{2}$ min.—31, 31, 30, 29, 28, 27, 26.
1 min.—25, 23, 22, 22, 21, 21, 21, 20, 20, 19, 19.

Experiment 4*b*.—Plates connected for 30 min.

10 secs.—84, 76, 67, 62, 58, 55, 52, 50, 49, 48, 46, 44, 42, 41, 40, 39.
$\frac{1}{4}$ min.—38, 37, 36, 35, 34, 33, 32, 31, 30, 30.
$\frac{1}{2}$ min.—29, 28, 27, 27.
1 min.—26, 25, 24, 23, 22.

Experiment 4*c*.—Plates unconnected for about 20 hours.

10 secs.—34, 27, 23, 21, 18, 16, 14, 13, 12, 11, 11, 10, 10, 10.
$\frac{1}{4}$ min.—10, 10, 9, 9.
$\frac{1}{2}$ min.—9, 9, 9, 9.
1 min.—8, 8, 7, 6, 6.

These experiments show that when the plates are left in the liquid for a considerable time, some cause produces a great diminution of current-strength. This may be either due to increase of resistance or to decrease of capacity at the surface layers where "condenser-action" occurs. The above series of experiment does not determine to which cause the diminution is due, since half a minute elapsed between starting the current and taking readings; but the following series shows that, whether or not there be alteration of capacity, there is great alteration of resistance. The galvanometer was shunted, and readings were taken every 10 secs. after joining the battery into the circuit.

Experiment 1.—Plates connected for about 40 hours.

Deflection.—19, 13, 10, 9, 8, 7, 7, 6.

Experiment 1*a*.—Plates heated, and 48 ohms added.

Deflection.—18, 14, 13, 12, 12, 11, 10, 9.

Experiment 1*b*.—Plates heated, and no additional resistance.

Deflection.—25, 20, 15, 14, 13, 13, 12.

Other experiments of the same kind were made, and gave nearly the same result. So that evidently in the course of 40 hours a resistance of about 40 or 50 ohms appeared in the circuit. The next point to be determined was whether or not this resistance followed the law of inverse proportionality to the surface. One or two preliminary experiments were made, which showed that if the plates were connected by a short copper wire for 15 minutes, all previous polarization was discharged,—at least so far as the galvanometer could detect it. In what follows the plates were raised roughly about one-half out of the liquid, so that the transition-resistance should be approximately doubled if it follows the above-mentioned law. The numbers in this series of experiments cannot be compared directly with those in the first, as the arrangement of resistance in the circuit was different.

Experiment 1.—Plates connected for about 40 hours.

Deflection.—5, 4, 3, 3, 2.

Experiment 1*a*.—Plates heated, and 96 ohms added.

Deflection.—5, 3, 2, 2, 1·5.

Experiment 1*b*.—Plates connected for half an hour, 110 ohms added.

Deflection.—5, 4, 4, 3, 3, 3, 2.

Experiment 1*c*.—Plates connected for half an hour, 110 ohms added.

Deflection.—5, 4, 4, 3, 3.

Experiment 1*d*.—Plates connected for one quarter of an hour; no resistance added.

Deflection.—18, 17, 16, 15, 14.

Other similar experiments giving much the same results were made, but need not be quoted. Evidently the resistance is inversely proportional to the area of the plates.

Time during which the Resistance Increases.

Experiments were made in which the plates were left half out of the liquid for 20 hours. In this case a resistance of from 80 to 90 ohms, roughly, was found. (The numbers must be given roughly,

for the apparatus did not admit of great accuracy, the galvanometer being shunted by a wire of very small resistance.) In two or three days the resistance was about 200 ohms. Once the plates were left for eleven days, when a resistance of about 250 ohms was observed. The process, therefore, goes on slowly for a long time.

Origin of the Resistance.

Most probably the resistance is due to the condensation, on the surface of the electrodes, of gases dissolved in the liquid. To test this, I left the plates in air instead of placing them in the liquid. The resistance in this case was of the same magnitude as before, and was evidently due to the condensation of atmospheric gases. To obtain a stronger proof, however, I placed the plates, after being heated, in an atmosphere of oxygen for about two hours. A resistance was found in this case about equal to that which was caused by leaving the plates in air for twenty hours. Leaving the plates in *air* for two hours made no appreciable change in resistance. Next, I connected the plates to the positive pole of a battery of two Bunsen cells for two minutes, and decomposed water with them, so that oxygen was developed on them. They were then connected together for a quarter of an hour to get rid of polarization, and then a resistance was observed equal to that got by leaving them in oxygen for two hours. Nearly, but not quite, the same resistance occurred if the plates were joined to the negative pole of the Bunsens, so as to develop hydrogen on them. There can be no doubt, then, but that the resistance is due to condensed films of gas.

Firmness of the Gaseous Deposit.

That the deposit clings with excessive firmness to the surface of the metal, is evident from the fact that nothing but heating to a red heat destroyed the resistance. No amount of rubbing of the plates, however hard, made any observable diminution.

Specific Resistance of the Films of Gas.

In order to determine the specific resistance of the deposit, a knowledge of its thickness is necessary. Or, if one can assume that the thickness is the same in different specimens of platinum, it might be obtained from the above experiments (which can give

the value of the *product* of the specific resistance and the thickness) combined with experiments made on the resistance of a very fine platinum wire, first, when newly heated, and, secondly, when left unheated for some time. The latter experiments would give the value of the *ratio* of the two quantities mentioned.

Meanwhile, if it may be assumed that the thickness of the condensed gases is of the same order as the thickness of condensed moisture and gases on the surface of glass (given by Quincke as $5(10)^{-5}$ c.m.—*Pogg. Ann.*, 1859, and *Wied. Ann.*, 1877), the above experiments show that the order of the specific resistance is the same as that of ordinary dielectrics.

Remarks.

If the film of gas had acted as a perfect dielectric no alteration of resistance would have been perceptible. The fact that the resistance appeared shows that to a great extent, if not entirely, the gas assumes the potential of the metal on which it is condensed. Given that the gases do assume the potential of the electrodes, it is rendered highly probable that the resistance of the film will alter with the potential; for the particles of the gas become mutually repellent because of their similar electric charge, and so tend to alter their relative positions against the attraction of the metal. That such change of resistance with potential did occur was evident in the experiments I made; for if the initial deflection was obtained after the plates were left unheated for some hours, and was again obtained after they had been connected for a short time to discharge polarization, it was smaller in the latter case than in the former, showing that the gas had not yet assumed its previous physical condition. Again, if the deflections be got as before, and the plates be heated and an equal resistance put in circuit (so as to obtain the same initial current), and the deflections again be taken, the rate of decrease of current with time is always somewhat greater in the former case than in the latter. This might be due to alteration of resistance with potential, for the potential of the plates increases with time after the battery is joined in. On the other hand, it might be due to alteration of capacity.

Note.—In the Plate the point marked 1 corresponds to the first reading of the galvanometer for the curves a and c. The point o' is related to the curves b in the same way as o is to a and c.

5. Researches on the Problematical Organs of the Invertebrata—especially those of the Cephalopoda, Gasteropoda, Lamellibranchiata, Crustacea, Insecta, and Oligochæta. By Dr A. B. Griffiths, F.R.S. (Edin.), F.C.S. (Lond. & Paris), Principal, and Lecturer on Chemistry and Biology, School of Science, Lincoln; late Lecturer on Chemistry, Technical School, Manchester, &c.

Being convinced that a thorough examination (both from a chemical and physiological point of view) of the various *problematical* organs of the Invertebrata will throw much light on their physiology and their relationship to the Vertebrata, these investigations have been undertaken with that object in view.

I have already shown that the so-called "liver" of *Sepia officinalis* is a true *pancreas*, and not a liver (*Proc. Roy. Soc. Edin.*, vol. xiii. No. 119, p. 120).

A. (I.) *Nephridium of Cephalopoda.*

Taking a fresh *Sepia officinalis* as a type of the Cephalopoda, it was found that its nephridia are true kidneys, or renal organs. The venous blood, as it passes from the vena cava, is distributed by a number of afferent branchial vessels which communicate with the sacculated and glandular chambers (the nephridia). The blood passes to the gills and then back to the heart.

After dissecting the nephridia from the bodies of several fresh cuttle-fishes, the secretion of these glands was found to be acid to litmus paper, the liquid deposits, upon standing a short time, earthy matters. These earthy deposits were submitted to chemical analysis. They are insoluble in distilled water, but readily soluble in acetic acid. On neutralising a portion of the acetic acid solution with ammonium hydrate, and then adding ammonium oxalate, a white precipitate was obtained, indicating the presence of calcium. Another portion of the acetic acid solution was neutralised, and to the neutral solution silver nitrate added: a yellowish precipitate was obtained. To another portion of the solution, ammonium hydrate was added until alkaline, and to this alkaline solution a

small quantity of an aqueous solution of magnesium sulphate, with the production of a white precipitate of ammonio-magnesium phosphate. The presence of phosphoric acid in the earthy deposits was confirmed by using the ammonium molybdate and the uranium nitrate tests. Therefore, it must be concluded that these earthy deposits consist of calcium phosphate. No calcium carbonate, magnesium carbonate, nor any other compound was found in the deposit.

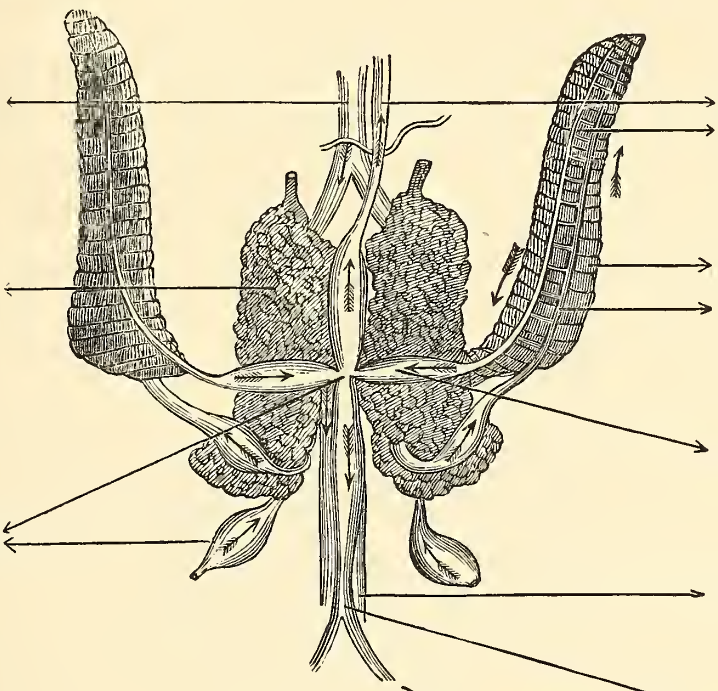


Fig. 1.—Nephridium of *Sepia officinalis.*

The liquid portion of the secretion of the nephridia was examined by two separate methods :—

(*a*) The clear liquid from the nephridia (after the separation of the calcium phosphate) was treated with a hot dilute solution of sodium hydrate, then on adding hydrochloric acid, a slight flaky precipitate is obtained; and on examining these flakes under the microscope, they were seen to consist of small crystals in rhombic plates, prismatic needles, and stellar-shaped crystals. On treating the secretion with alcohol, the rhombic crystals are deposited; these crystals are soluble in water. When these crystals are treated with nitric acid, and heated gently with ammonia, the reddish purple *murexide* [$C_8H_4(NH_4)N_5O_6$] is obtained, which was found crystallised in prisms.

(*b*) Another method was applied to the clear liquid secreted by the nephridia. The liquid was boiled in distilled water, and evaporated carefully to dryness. The residue so obtained was treated with absolute alcohol, and filtered. Boiling water was poured upon the residue on the filter paper, and to the aqueous filtrate an excess of acetic acid was added. After standing $6\frac{3}{4}$ hours, crystals of uric acid were deposited, and recognised by the chemico-microscopical tests already mentioned above. Further, it was found that there was a small quantity of uric acid in the blood of the vena cava, before it entered the nephridia; but the blood after passing into the branchiæ contains no uric acid. From these reactions, the secretions of the nephridia contain uric acid and calcium phosphate, and prove that the nephridia of the Cephalopoda are true renal organs getting rid of the nitrogenous waste matters, in the form of uric acid, contained in the pure blood as it is brought to these organs (nephridia) by the vena cava.

(II.) *On the Renal Organs of* Astacus fluviatilis, Anodonta cygnea, Limax flavus, Helix aspersa, *and* Periplaneta orientalis.

It will be remembered that in a paper (*Proc. Roy. Soc.*, vol. xxxviii. No. 236, p. 187) before the Royal Society of London, I have shown that the secretions of the so-called "green glands" of *Astacus fluviatilus* (crayfish) can be made to yield uric acid ($C_5H_4N_4O_3$) and guanin ($C_5H_5N_5O$), showing these glands are analogous in physiological function to the kidney of the higher forms of animal life.

Mr Harold Follows, F.C.S., and myself (*Chemical News*, vol. li. p. 241, and *Jour. Chem. Soc.* [Abstracts], 1885, p. 921) have established the renal functions of the organs of Bojanus in *Anodonta cygnea* (fresh-water mussel), by the isolation of uric acid and urea from the secretions of those organs.

The isolation of uric acid crystals from the problematical renal organs of the Invertebrata, commenced by myself (in my Royal Society's paper on the green gland of Astacus) led Dr C. A. MacMunn, M.A., F.C.S. (*Journal of Physiology*, vol. vii. No. 2, p. 128) to prove the renal function of the Malpighian tubes of *Periplaneta orientalis*, and in the nephridia of *Helix aspersa* and *Limax flavus*.

(III.) *Renal Organs of the Lamellibranchiata and Crustacea.*

The organ of Bojanus or nephridium of *Mya arenaria* (as well as *Anodonta cygnea*) contain uric acid and urea in their secretions, and there is also a small quantity of calcium phosphate present in the secretion of the organ of Bojanus. Mr Follows and myself (*Chemical News*, vol. li. p. 241, and *Jour. Chem. Soc.* [Abstracts], 1885, p. 921) found "*a salt of calcium in minute quantities*," but we could not make out the acid in combination; subsequently it was found to be *phosphoric acid.* The "green glands" of *Homarus vulgaris* (lobster) easily yield uric acid crystals and small quantities of the base guanin.

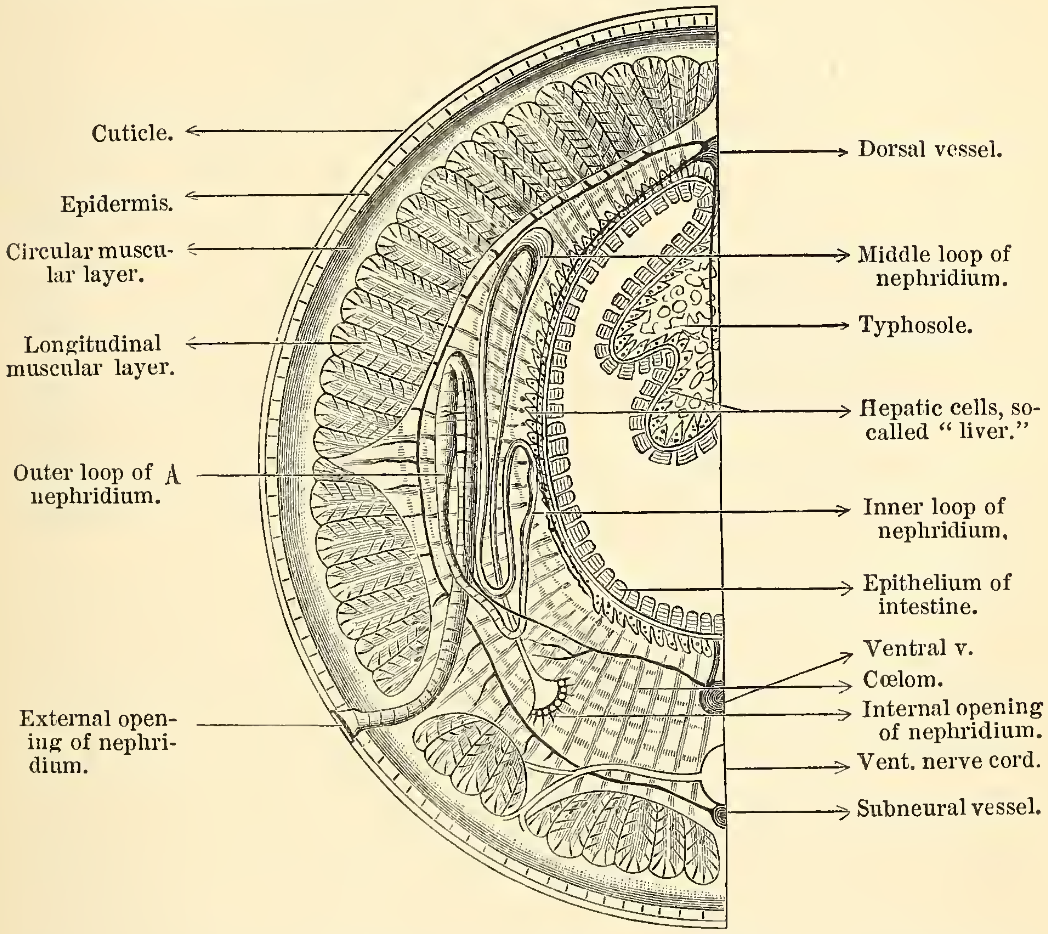


Fig. 2.—Diagram of Nephridium of *Lumbricus terrestris.* A, the secretion of the outer loop of the nephridium (segmental organ) contains the largest quantity of uric acid.

(IV.) *Renal Organs of the Oligochæta.*

After treating the segmental organs (nephridia) of freshly killed *Lumbricus terrestris* (earthworm) in a similar manner to the

nephridia of the Cephalopod, yields uric acid, but no guanin, urea, or calcium phosphate. Therefore the "segmental organs" of *Lumbricus* are renal in function, getting rid of the nitrogenous waste matters contained in the blood, in the perivisceral cavity.

The largest amount of uric acid was found in the secretion contained in the muscular part of the "segmental organ" (fig. 2, outer loop of nephridium).

The next table is a summary of the constituents of the nephridia of certain divisions of the Invertebrata.

(V.) *Renal Organs and their Constituents.*

	Cephalopoda.	Gasteropoda.	Lamellibranchiata.	Crustacea.	Insecta.	Oligochæta.
Uric acid, . .	present.	present.	present.	present.	present.	present.
Urea, . . .	absent.	...	present.	absent.	...	absent.
Guanin, . .	absent.	...	absent.	present.	...	absent.
Calcium phosphate,	present.	...	present.	absent.	...	absent.

B. (I.) *Salivary Glands of Gasteropoda and Insecta.*

The secretions of the "salivary glands" of the *Insecta* (Orthoptera) were investigated by taking as an example the *Periplaneta orientalis* (cockroach).

The salivary glands of *Periplaneta* are situated on each side of the œsophagus and crop, and extend posteriorly as far as the abdomen. They are about $\frac{1}{3}$ of an inch in length, and composed of acini (fig. 3, *b*). Accompanying the glands are two salivary receptacles, one on either side of the crop. A quantity of the secretion was extracted by crushing about sixty glands of freshly killed cockroaches. It was alkaline to test-papers. A portion of the secretion was added to a small quantity of starch, the starch being converted into glucose sugar in 12 minutes. The presence of sugar was proved by the formation of red cuprous oxide by the action of Fehling's solution.

Another portion of the secretion was distilled (with the utmost care) with dilute sulphuric acid; and to the distillate ferric chloride added, which gave a red colour indicating the presence of sulpho-

cyanates. The inorganic constituent, as far as I could make out, consists only of calcium phosphate.

Turning once more to the soluble zymase (ferment) contained in the secretion, it can be isolated by precipitating the secretion with dilute phosphoric acid, adding lime-water, and filtering. The precipitate was dissolved in distilled water, and then reprecipitated by alcohol. This precipitate converts starch into glucose sugar.

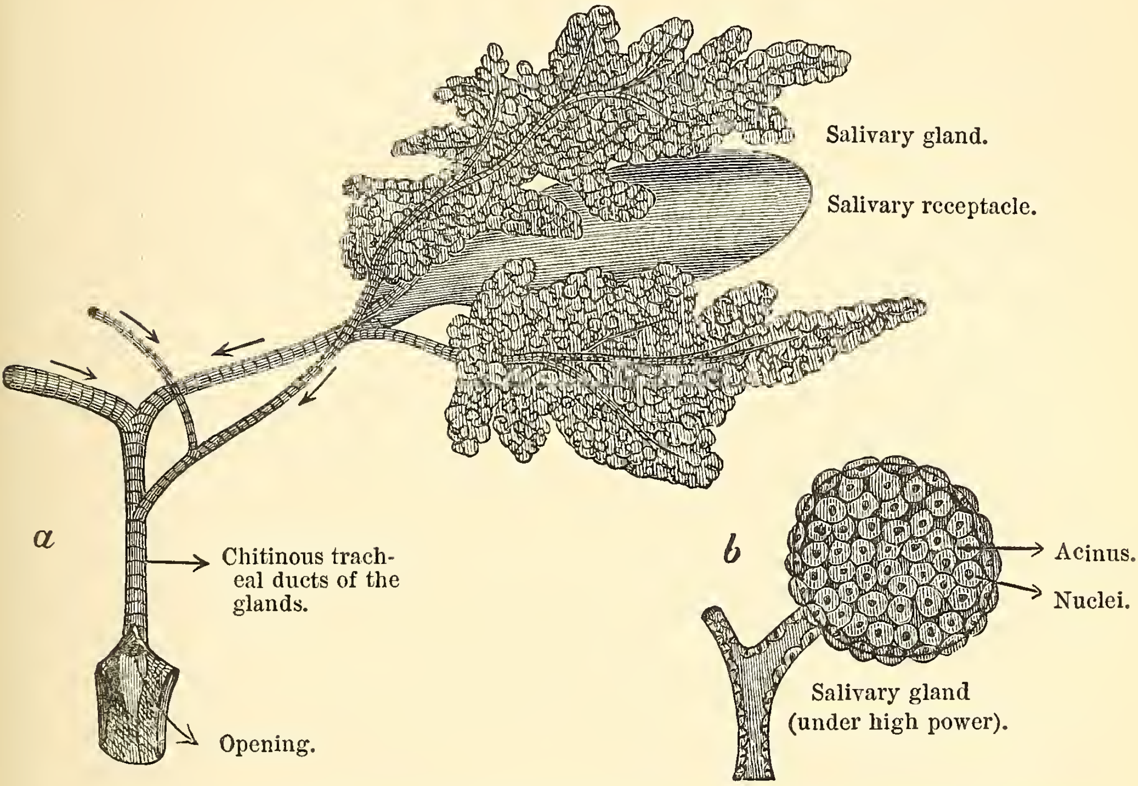


FIG. 3.—*a* and *b*, Salivary gland of *Periplaneta orientalis* (much enlarged).

I have already mentioned that the secretion of the salivary glands of *Periplaneta* are alkaline. Out of 80 animals, I found 4 with the secretion decidedly acid. This acid property is most probably due to pathological changes in the secretion of the said four animals.

The largest quantity of diastatic or "soluble" ferment was to be found in the secretion obtained from the glandular portion of the organ and not from the salivary receptacles (fig. 3, *a*).

The salivary glands of *Helix aspersa* (snail) yielded a soluble ferment, capable of converting starch into glucose sugar. The ferric-chloride test failed to show the presence of sulpho-cyanates. The mineral ingredients found were calcium and chlorine; but I

could not detect the presence of phosphates or carbonates in the salivary glands of *Helix*.

Therefore, from these investigations, the salivary glands of the Insecta and Gasteropoda are similar in physiological function to the salivary glands of the higher animals.

The following table gives the constituents found in these two divisions of the Invertebrata :—

(II.) *Salivary Glands and their Constituents.*

	Insecta (Orthoptera).	Gasteropoda.
Soluble diastatic ferment,	present, . . .	present.
Sulphocyanates, . .	present, . . .	?
Calcium phosphate, .	present, . . .	?
Calcium, . . .	present, . . .	present.
Chlorine, . . .	absent, . . .	present.

C. (I.) *On the "Liver" of the Gasteropoda, Lamellibranchiata, Crustacea, and Insecta.*

I have already proved the so-called "liver" of the Cephalopoda is a true pancreas (*Proc. Roy. Soc. Edin.*, vol. xiii. No. 119, p. 120).

The secretion of the "liver" of *Astacus fluviatilus* when fresh gives an acid reaction.

(*a*) The secretion of the organ acts upon starch paste. The starch granules disappear with the exception of their celluloid covering; and on treating with water, and then adding Fehling's solution, sugar in the dextrose form was obtained.

(*b*) The secretion forms an emulsion with oils and fats yielding subsequently fatty acids and glycerol.

(*c*) The action of the secretion upon milk was to render it transparent.

(*d*) When a few drops of the secretion of the organ were examined with chemical reagents under the microscope, the following

reactions were observed:—On running in between the side and cover-slip a solution of iodine in potassium iodide, a brown deposit was obtained, and on running in concentrated nitric acid on another slide containing a drop or two of the secretion, a yellow coloration was formed, due to the formation of xanthoproteic acid. These two reactions show the presence of albumen in the secretion of the organ in question.

(*e*) The soluble ferment was extracted according to the Kistiakowsky method (Pflüger's *Archiv für Physiologie*, vol. ix. pp. 438–459). The ferment converts fibrin into leucin (α-amido-caproic acid, $C_6H_{13}NO_2$) and tyrosin (oxyphenylamidopropionic acid, $C_9H_{11}NO_3$).

(*f*) No glycocholic and taurocholic acids could be detected by the Pettenkofer and other tests. No glycogen was found in the organ or its secretion.

(*g*) The secretion contains about 5 per cent. of solids.

(*h*) The secretion contains leucin and tyrosin.

Similar reactions were obtained with the secretion of the pyloric cœca ("liver") of *Periplaneta orientalis*, which substantiate and further extend the investigations of Krukenberg, Plateau (*Bull. de l'Acad. Roy. de Belgique*, xli. 1874), Hoppe-Seyler, and others.

The secretion of the so-called "livers" of *Helix aspersa*, *Limax maximus*, *Limax flavus*, *Mya arenaria*, *Anodonta cygnea*, and *Lumbricus terrestris* all yield similar reactions to those of the secretions of the "liver" of *Astacus fluviatilis.*

From these investigations the conclusions to be drawn are, that the so-called "livers" of the Gasteropoda, Lamellibranchiata, Crustacea, Insecta, and Oligochæta are pancreatic in function, *i.e.*, their secretions are more like the secretions of the pancreas of the Vertebrata than the secretions of a liver.

In conclusion, I may say that the present work will be continued on other problematical organs of the Invertebrata, and their analogy or otherwise with organs whose functions are well established in the Vertebrate division of animal life; for one cannot forget Pope's words—

"All are but parts of one stupendous whole."

Appendix.

List of Dr Griffiths's published Papers on the Physiology of the Invertebrata.

I. "On the Extraction of Uric Acid Crystals from the Green Gland of *Astacus fluviatilis*," *Proc. Roy. Soc.*, vol. xxxviii. p. 187; *Jour. Chem. Soc.*, 1885, p. 680; *Science Gossip*, No. 255, p. 57.

II. "A Peculiar Excretory Product found in the 'Liver' of *Sepia officinalis*," *Chem. News*, vol. xlviii. p. 37; *Jour. Chem. Soc.* [Abstracts], 1884, p. 94.

III. "Chemico-Physiological Investigations on the Cephalopod Liver, and its identity as a true Pancreas," *Proc. Roy. Soc. Edin.*, No. 119, vol. xiii. p. 120; *Chem. News*, vol. li. p. 160; *Chem. Soc. Jour.*, 1885, p. 829.

IV. "Chemico-Biological Examination of the Organs of Bojanus in *Anodonta*," *Chem. News*, vol. li. p. 241; *Jour. Chem. Soc.*, 1885, p. 921; *Manchester Guardian*, May 30, 1885.

V. "On some Points in the Physiology of Certain Organs of the Alimentary Canal of *Blatta periplaneta*," *Chem. News*, vol. lii. p. 195.

Also, since the present paper.

VI. "On the Nephridia and 'Liver' of *Patella vulgata*," *Proc. Roy. Soc.*, vol. xlii. p. 392.

VII. "On the Nephridia of *Hirudo medicinalis*," read before Royal Society of Edinburgh, July 4, 1887.

6. The Nephridia of *Lanice conchilega*, Malmgren. By J. T. Cunningham.

(Abstract.)

The excretory system in this species has never been adequately described; it presents an extremely interesting condition from a morphological point of view. There are four well-developed nephridia in somites 6–9, inclusive. Each of these commences by an internal aperture or nephrostome, and consists of a bent tube or loop, the inner side (the side nearer the median plane) of the

loop being connected with the nephrostome, while the outer passes downwards and opens into a longitudinal tube common to all the four nephridia of a side. Four openings, corresponding to the four nephridia, place the longitudinal tube in communication with the exterior; these openings are close behind the upper ends of the 2nd to the 5th uncinigerous tori respectively; the 1st uncinigerous torus being in the 5th somite. The longitudinal tube is continued backwards on each side through somites 10–13, representing four more coalesced nephridia; but in this region there are neither internal nor external openings, nor any loops similar to those in the more interior region; the longitudinal tube is simple, almost cylindrical, showing slight indentations between the successive somites, which mark where the successive nephridia have coalesced. The outer side of the whole longitudinal tube is in contact with the ventral longitudinal muscles, while the upper and inner side is beneath the oblique muscles. The internal openings already mentioned are situated immediately behind the notopodial fascicles of setæ of somites 5–8 inclusive. The longitudinal tube extends into the 5th somite, but I could not find there an external opening. Behind the 1st to the 4th somites are traces of incomplete septa, of which that behind the 4th is the most complete. Attached to the front of the latter septum is a nephrostome, but I could not trace any connection between this and the part of the tube in the 5th somite. There are two other well-marked nephrostomata attached to the septa behind somites 2 and 3, and these openings lead into tubes seen in somites 3 and 4. I could not find external openings in the two latter somites. There are thus eleven nephridia represented altogether,—three rudimentary, in somites 3, 4, 5; four perfect, in somites 6–9; and four imperfect, in somites 10–13; the eight posterior being all in communication, their distal parts having fused to form a longitudinal tube. This is the first case in which such a longitudinal coalescence of nephridia has been discovered, and its morphological similarity to the condition in Vertebrates is obvious.

The Astronomer-Royal for Scotland exhibited specimens illustrating Ives's process of Isochromatic Photography.

By permission of the Meeting, Professor Tait stated to the Society

that he had just received a letter from Professor Amagat of Lyons, containing an account of the solidification of tetrachloride of carbon 6 p. 79° C., $C_2Cl_4[CCl_4]$ by pressure only at ordinary temperatures.

Monday, 6th June 1887.

JOHN MURRAY, Ph.D., Vice-President, in the Chair.

The following Communications were read:—

1. On a Furnace capable of melting Nickel and Cobalt. By J. B. Readman, Esq.

2. On the Fossil Flora of the Radstock Series of the Somerset and Bristol Coal Fields. Concluding Part. By R. Kidston, Esq.

3. On the Discharge of Albumen from the Kidneys of Healthy People. By Prof. Grainger Stewart, M.D.

Great diversity of opinion exists as to the frequency of the occurrence of albuminuria in healthy people, and elaborate inquiries have led different observers to conspicuously contradictory conclusions. Posner has said that his observations satisfy him that traces of albumen exist in every normal urine, and may be demonstrated if sufficiently delicate methods are employed. One of the most distinguished authorities on the subject, Dr Senator of Berlin, says that his observations supply good reason why he should consider it not improbable that, if we were to examine the urine for long periods at different hours of the day, and with great care, we should sooner or later find it to contain albumen in the case of every healthy man. Dr Kleudgen, in the course of a special study of albuminuria in relation to epilepsy, came to the conclusion that traces of albumen could be demonstrated in any urine above a certain degree of concentration. Dr de la Celle de Chateaubourg found albumen in the urine of 592 out of 701 healthy people whom

he examined, that is in 84 per cent. Dr Capitan found that among 98 French soldiers 44 or 44·9 per cent. had albuminuria. Professor Lenbe, on the other hand, found among 119 German soldiers whom he examined that only 4 per cent. showed albumen on rising in the morning, and 16 per cent. in the forenoon after a march of several hours' duration. Dr Van Noorden states that he found it vary under different conditions among healthy German soldiers from 3 to 35 per cent. Dr Munro found albuminuria in 24 out of 220, that is in 10·9 per cent., presumably healthy people examined for life insurance in the United States of America. And Dr Leroux found it only 19 times among 330 children, or in 5·76 per cent.

Such contrariety of results made me think it desirable to make a fresh series of observations upon this point, with the view of determining (first) whether Posner is right in saying that albumen is present in every urine; (second) what proportion of presumably healthy people have albumen in the urine in quantity sufficient for demonstration by the tests ordinarily in use; and (third) what effects various physiological conditions, such as diet, exercise, severe exertion, and cold bathing, produced upon the discharge.

I have, with the aid of Dr Stevens, made some experiments with the view of determining the first of these questions, and have tried to repeat Posner's observations. I do not feel sure that our results were absolutely satisfactory, but the conclusion to which I am led in the meantime is that albumen, if present at all in normal urine, is in such extremely minute amount as to be barely discernible, or not discoverable at all, with the most delicate tests, even after considerable concentration. The minute trace which appears sometimes to be present is probably accounted for by the epithelial and other cellular elements from the urinary passages which are present in greater or less amount in every urine.

With the view of obtaining evidence as to the second question, that is the proportion of presumably healthy people who have albumen in their urine in quantity sufficient for demonstration by the tests ordinarily in use, I have examined, with the assistance of Dr Stevens and Mr Boddie, several series of presumably healthy individuals. By the kindness of Dr Mills and Mr Fayrer, medical officers of Edinburgh Castle, and of the Colonel and Adjutant of

the Seaforth Highlanders, I was enabled to examine a series of 205 soldiers and applicants for admission to the army.

I also got specimens of urine from 74 healthy male adults engaged in civil employments. By the kindness of Dr Sinclair and his resident assistant Dr Helme, I examined 80 healthy inmates of Craiglockhart Poorhouse; and by the kindness of Dr Halliday Douglas and Mr Munro, I had opportunity of examining the urine of a large number of the inmates of the Orphan Hospital. We had thus in all 407 presumably healthy individuals, with regard to whose urine we made the most careful examination, sometimes on one, sometimes on several occasions.

The plan of testing adopted was in all cases the same. Urines which were cloudy from any cause were carefully filtered. Those which were clear were tested as passed. Each specimen was tested, first with nitric acid by the contact method, by which, as previous experiment had shown, we could discover albumen in the proportion of 0·003 per cent., or 0·01311 of a grain per ounce; and by picric acid, using the contact method, by which we could discover albumen in the proportion of 0·00015 per cent., or 0·0006555 of a grain per ounce. Each specimen was also carefully tested for peptones, using Fehling's solution by the contact method, a plan which certainly shows the presence of peptones very distinctly when they are added to urine, and probably is a reliable test in cases of peptonuria.

Taking specimens of urine passed by 407 presumably healthy individuals, during the forenoon or about midday, we found that albumen was present in 129, or a little over 31·7 per cent. Of these it was in quantity sufficient to be discovered by the cold nitric acid test in 66, in lesser quantity in 63. In Table I. the greatest results are shown—

TABLE I.—*Showing incidence of Albuminuria in 407 presumably healthy individuals (forenoon or noon specimens).*

Urines Examined.	Albumen shown by HNO_3.	Albumen shown by Picric Acid.	Total.	Per cent.
407	66	63	129	31·7

But it was evident that a marked difference existed between

various groups of individuals examined, as between soldiers and men of corresponding life following civil occupations, and between children and men about or above sixty. It is therefore necessary to consider these groups separately. Among the soldiers and recruits examined, 205 in number, 77, or 37·56 per cent., had albuminuria; while of 74 adults in civil employments, 8, or between 10 and 11 per cent., showed the symptom. Of the former group it was shown by nitric acid in 47, or 22·92 per cent.; by the picric acid only in 30, or 14·63 per cent. Of the latter group it was shown by nitric acid in 5, or 6·75 per cent.; by picric acid only in 3, or 4·05 per cent. Table II. shows these results.

Table II.—*Showing the incidence of Albuminuria in Soldiers and Civil Population.*

		With HNO_3.	With Picric Acid only.	Total.	Per cent.
Soldiers, . .	205	47	30	77	37·56
Civil Population, . .	74	5	3	8	10·8

In seeking to compare the facts in the case of children and old people, I thought it desirable to get access to individuals in similar position in life, and living under somewhat similar conditions, and I was glad to avail myself of the opportunity afforded of examining the inmates of Craiglockhart Poorhouse. We got specimens of the urine of 40 men, about or above sixty years of age, resident in the poorhouse, but not on the sick list. I found that albumen was present in 27 of them, that is in 67·5 per cent. We also examined a series of 40 children under puberty, and found that it was present in 7, or in 17·5 per cent. Nitric acid showed it in 9, that is in 22·5 per cent. of the old men. Picric acid in other 18, or 45 per cent.; while in the children nitric acid showed it in 2, or 5 per cent., and picric acid in other 5, or 12·5 per cent.

When these results are shown in a tabular form, we see at a glance how striking is the contrast between the two groups.

It thus appears that of the four groups the old men in the poorhouse showed albuminuria most frequently, the soldiers next, the children in the poorhouse next, and the least frequently apparent were the young men engaged in civil occupations.

TABLE III.—*Showing incidence of Albuminuria in 40 Children and 40 Old People (presumably healthy), inmates of Craiglockhart Poorhouse.*

	With HNO_3.	With Picric Acid.	Total.	Per cent.
Children under puberty, . .	2	5	7	17·5
People about or above sixty, .	9	18	27	67·5

It was not in my power to determine the cause of the albuminuria in the persons examined, but I took care to exclude cases of the accidental accumulation of mucus or pus in the urinary tract, and have included only four, viz., two soldiers and two of the old men. In none of the cases was the albuminuria due to cardiac or pulmonary diseases, and in very few was there occasion to suspect the existence of Bright's disease. On the other hand, there were few cases whose clinical history corresponded to Pavy's cyclical albuminuria or Moxon's albuminuria of adolescents.

Being anxious to supplement these observations, I asked two of my former assistants, who are well known to me as careful and accurate observers, Dr James Ritchie and Dr Graham Brown, to give me the results as to albuminuria met with in the last 200 cases which had been proposed for insurance in the two companies for which they are medical referees. The tests employed had been heat or cold nitric acid, and it was found that in one series of 200, 5 per cent. showed albumen, and in the other series only 1 per cent. did so. The former result corresponds pretty closely to what nitric acid revealed in my own series of young men following civil employments, but is considerably below the results brought out by Dr Munro in his American statistics. The second series gives a much lower percentage.

It is interesting to compare the results obtained in my other categories with those given by other observers. Leube found, among German soldiers examined during the forenoon and after marching, 16 per cent. albuminuric. Van Noorden, at the same time of day, found it in 35 per cent. Capitan found it among French soldiers 44·9 per cent., and I have found it among the Highlanders (including recruits) in 37·55 per cent.

The Craiglockhart children gave a result less favourable than that obtained by Leroux, for while he found albuminuria in only 5·76 per cent., I found it in 17·5.

I am not aware of the publication of any series of observations on old men corresponding to my Craiglockhart series.

In answer, then, to our second question, it appears that a trace of albumen may be discovered by delicate tests in the urine of nearly 1 in 3 of the male population, if it be examined during the active period of the forenoon, an hour or two after breakfast, although before breakfast the proportion would be considerably smaller.

The third question is as to the effects produced by diet, exercise, severe exertion, and cold bathing upon the discharge of albumen.

In order to determine the effects of diet, I obtained specimens of the urine of 32 soldiers before and after breakfast, and found that of these 15, or 5·625 per cent., had albuminuria on rising in the morning; while 13, or 40·525 per cent., showed it after the morning meal. Thus, 8 who had not had albuminuria in the morning acquired it after breakfast.

Among the 40 old men examined in Craiglockhart Poorhouse we find that 15, or 37·5 per cent., showed albuminuria before breakfast; while after that meal 27, or 67·5 per cent., showed it. Thus 12 who had not had albuminuria on rising in the morning acquired it after breakfast.

Among the 40 children we find that 5, or 12·5 per cent., showed it before breakfast, and 17, or 17·5 per cent., showed it after breakfast. Thus 2 who had not albuminuria on rising in the morning acquired it after breakfast. Among 48 boys, inmates of the Orphan Hospital, we found that before breakfast albumen was present in 7, or 14·6 per cent.; after breakfast, in 10, or 20·83 per cent. Taking the four groups together, we have a series of 160 cases examined before and after breakfast, and we find that of these, 32, or 20 per cent., discharged albumen before breakfast; while 57, or 30·5 per cent., showed it afterwards.

I have put these various results in a tabular form, which shows very clearly that at all ages, and in the various conditions investigated, the taking of breakfast is followed by an increased frequency of albuminuria, but that the increase is greatest among the old men and soldiers.

TABLE IV.—*Showing the Influence of Breakfast on the Discharge of Albumen from the Kidneys.*

	No.	Before Breakfast.		After Breakfast.	
		No.	Per cent.	No.	Per cent.
Soldiers, .	32	5	15·625	13	40·625
Old Men, .	40	15	37·5	27	67·5
Children (Craiglockhart), .	40	5	12·5	7	17·5
Children (Orphan Hospital) .	48	7	14·6	10	20·83
Total, .	160	32	20	57	35·6

In connection with this it is worthy of notice that in most of the cases of after-breakfast albuminuria the quantity of albumen was too minute to be shown by the cold nitric acid test, and also that when it was present before, it was generally increased in amount after the meal. But, on the other hand, there were two cases among the children in which breakfast was followed by the disappearance of albuminuria which had been present on rising. I have met with facts corresponding to this in some of my albuminuric patients. A gentleman who is at present under my care shows copious albumen in the morning urine, and a comparatively small quantity after breakfast.

Contrary to what one might expect, considering what is usually taken for breakfast, as compared with what is taken for the other meals, it appears that breakfast more frequently induces albuminuria, or an increase of albumen, than the other meals. As to the explanation of the influence of food in this respect, it is difficult to speak positively. I shall not at present seek to determine whether an alteration of the blood, or the blood pressure, or of the vascular walls, or epithelial structures, is at fault. It may also be remarked that the mucin in the urine also increases after food, although not to the same extent.

The next point investigated was the effect of muscular exercise on albuminuria. It appeared desirable to distinguish between the effects of moderate exercise and of severe and prolonged exertion. Observations were therefore made upon soldiers before and after their weekly march of seven to ten miles, and before and after the fatigue duty of coal-carrying.

Of 63 soldiers about to start for their weekly march of from seven to ten miles in heavy marching order, 18, or 29 per cent., were found to have albumen in their urine. After their march the urines of 58 of these men were examined, and 11, or 19 per cent., showed albumen. The march out, therefore, distinctly diminished the albuminuria. But as the march is taken in the forenoon, it occurred to me that some of those who got rid of their tendency during the march might have had a temporary albuminuria induced by breakfast. I therefore examined the urine of 32 soldiers before breakfast, after breakfast, and on their return from the march. It was found that before breakfast albumen was present in 5, or 15·623 per cent.; after breakfast in 13, or 40·625 per cent.; and after the march in 9, or 28·125 per cent. It was noticed also that in several cases the amount of albumen diminished, although it did not wholly disappear. It was thus shown that in a considerable proportion of cases the march removed the dietetic albuminuria, and other observations which I have made justify the conclusion that the march out exerts a favourable influence. It must, however, be observed that in some cases the march induced albuminuria. In one of the nine cases it occurred only after the march, the urine having been quite free from albumen on rising and after breakfast, and in at least one other case the amount of albumen was distinctly less after breakfast than it was after the march. It is thus clear that the effort of marching is sufficient to induce the symptom in some people.

But while marching proved on the whole beneficial, the fatigue duty of coal-carrying brought out a very different result. This work, as carried on in Edinburgh Castle, obliges two men to carry a bucket containing 80 lbs. of coal for several hundred feet up a rather steep incline, and then up barrack stairs to the different floors. Each pair of soldiers makes six or seven such journeys during the forenoon in which they are told off to this duty. Of 36 soldiers engaged in this work we found that 16, or 44 per cent., had albuminuria before the labour commenced; while 23, or 64 per cent., had albumen at the end of it. On another day, when we were able to get the urine of 17 men engaged in this coal-carrying, 7 had albuminuria, equal to a little over 41 per cent., although the observations were made, not at the end, but in the course of their work.

I have put in tabular form the facts elicited in this connection.

TABLE V.—*Showing effects of Exercise and of severe Exertion, also of Breakfast and Exercise.*

	No. Examined.		Before.		After.			
	Before.	After.	No.	P. cent.	No.	P. cent.		
March of 8 miles,	63	58	18	29	11	19		
Fatigue duty—coaling, .	36	36	16	44	23	64		
			Before	Br'kfast	After	Br'kfast	After	March.
Breakfast and march, .	32	32	5	15·6	13	40·6	9	28·1

From the facts thus given it is shown that violent exertion may produce albuminuria, while moderate exercise tends rather in many cases to diminish it. Statements have been made as to the urine of the performers of pedestrian feats which confirm this experience. Weston's urine is said to have contained both albumen and tubecasts at the end of one of his prolonged walks.

A very interesting observation has been made by Dr W. A. Stirling, in a thesis sent in for the M.D. degree this year, and he has permitted me to make use of it on this occasion. He found in the course of an investigation as to the incidence of albuminuria in 369 boys, who are being educated in the training-ship at Grays, Essex, that the boys who played wind instruments in the band exhibited albuminuria in a much larger proportion than the others. Thus, while, out of 64 boys so employed, 38, or 59·4 per cent., had albuminuria, out of 305 boys, otherwise under like conditions, but not in the band, only 39, or 12·8 per cent., showed the symptoms.

These results may, as he remarks, be very naturally referred to altered blood pressure due to habitual use of musical instruments.

With the view of testing this, I examined 24 boys who play wind instruments in the band of the Orphan Hospital, and 24 boys in that Institution who are otherwise similarly placed, except in not being members of the band. It appears, so far as their numbers serve us for the purpose, that albuminuria is more frequent among the band boys than among the others, but that there is a diminution rather than increase at the end of an hour's practice

with the instruments. I have put the facts in tabular form, and it is clear that no such discrepancy exists as in the training-ship boys; but still the statistics lend a certain measure of support to Dr Stirling's observations.

TABLE VI.—*Showing incidence of Albuminuria in 24 Wind-Instrument Band Boys and 24 other Boys (Orphan Hospital).*

	No.	Before Breakfast.				After Breakfast.				After Playing.			
		HNO_3	Pic. A.	Total.	P.C.	HNO_3	Pic. A.	Total.	P.C.	HNO_3	Pic. A.	Total.	P.C.
Band Boys,	24	2	3	5	20·8	2	4	6	25·0	1	2	3	12·5
Other Boys,	24	0	2	2	8·3	1	3	4	16·6	...	...	...	...

Some years ago Dr George Johnson of London drew attention to the fact that albuminuria is sometimes induced by cold bathing. In order to get some information upon this question, I got the urine of 21 boys passed on rising at 6 A.M., and that passed at 8 after a cold plunge bath. It was found that when, before bathing, 4, or 19·05 per cent., showed albumen, after it 5, or 23·8 per cent., showed it.

Among the boys so examined only a small number showed albuminuria, and the amount of albumen was slight, for nitric acid failed to detect it, but there was an increase both in the number of cases affected and in the intensity of the condition, although the effect was not very pronounced.

In Table VII. I have stated the results of these observations.

TABLE VII.—*Showing effect of Cold Bathing on 21 Boys (Orphan Hospital).*

Before Bath (6 A.M.).				After Bath (8 A.M.).			
With HNO_3.	Only with Pic. A.	Total.	Per cent.	With HNO_3.	Only with Pic. A.	Total.	Per cent.
0	4	4	19·05	0	5	5	23·08

I have not been able as yet to test the effects of mental excitement or emotion upon any considerable number of healthy individuals, but no doubt an investigation in suitable quarters might elicit interesting results. This is indicated by the occurrence of such cases as those recorded by Furbringer, of a medical man who never showed

albuminuria as the result of long and fatiguing work, nor from the use of a diet rich in albumen, nor from the free use of alcohol, but constantly showed it in large amount when exposed to mental excitement with depression.

The remarks which I have made apply only to the ordinary forms of albumen and serum-albumen. With regard to the occurrence of peptones, we discovered them in only 3 out of the whole series of 771 specimens which were carefully examined in the course of the investigations.

From the facts recorded, we seem entitled to conclude—

1. That albuminuria is much more common among presumably healthy people than was formerly supposed, being demonstrable by delicate tests in nearly one-third of those examined.

2. That there is no sufficient proof that albumen is normally discharged from the human kidneys.

3. That the frequency of albuminuria increases as life advances, being rare in children and young adults, and common in men at or above sixty years of age.

4. That it is more common among those whose occupations involve arduous bodily exercise than among those who have easy work.

5. That albuminuria frequently follows the taking of food, especially of breakfast.

6. That moderate muscular effort rather diminishes than increases albuminuria, except in rare cases.

7. That violent or prolonged exertion often induces albuminuria.

8. That cold bathing produces or increases it in some individuals.

9. That the existence of albuminuria is not of itself a sufficient ground for the rejection of a proposal for life insurance.

10. That the discharge of peptones from the kidneys is exceedingly rare in the presumably healthy.

4. The Salinity and Temperature of the Moray Firth, and the Firths of Inverness, Cromarty, and Dornoch. By Hugh Robert Mill, D.Sc., Scottish Marine Station. (Plate VIII.)

The recently published results obtained by the German gun-boat "Drache" in the North Sea enabled a very good chart to be compiled

of the distribution of salinity* in the central, southern, and eastern parts of that sea. No observations had been made in the great bight known as the Moray Firth, and this part of the map was accordingly left blank. The general distribution of salinity is as follows:—Water with more than 3·55 per cent. of salts in solution comes in from the Atlantic between the Orkney and Shetland Islands; the centre of the North Sea is filled with water with over 3·50 per cent. of dissolved salts; while to the south and all round the coasts fresher water is found. The line of 3·50 salinity was found to approach the Scottish coast at Berwick and again at Peterhead, but between these it swept out in a wide curve to the north-east. There were no available data by which the German Hydrographic Office could determine whether the Moray Firth was in the area of water over or under 3·50 per cent. salinity.

Dr Macadam in 1866 made some observations in the Moray Firth; Dr J. Gibson in 1883 made a number on behalf of the Fishery Board for Scotland;† and in 1885 Mr Ritchie and I examined that part of the region about the mouth of the Spey.‡ In the discussion which follows these results are considered, but most of the data used are derived from the cruise of the "Garland" in August 1886.

Dr Gibson and I proposed to the Fishery Board, in June 1886, that they should extend and repeat the physical observations which had been already made in the Moray Firth and the smaller sea-inlets in connection with it. To this the Board acceded, and we drew up a plan for carrying on the work. The steam-tender "Garland" was only available for three weeks from August 1st; Dr Gibson was unable to take part in the expedition, but I was ably assisted by Mr F. Maitland Gibson, and, for part of the time, by Mr T. Morton Ritchie, B.Sc.

The methods of working were similar to those which I have previously employed and frequently described to the Society. Negretti and Zambra thermometers were used, fitted in the Scottish frame, and the water-bottle described to the Royal Society in January 1886 §

* *Ergebnisse der Untersuchungsfahrten S.M. Knbt.* "*Drache*" *in der Nordsee in den Sommern* 1881, 1882, *und* 1884. Berlin, 1886. Abstract in *Scottish Geographical Magazine*, August 1887, iii. pp. 385–398.

† Fourth Annual Report, Fishery Board for Scotland, App. F, No. 12.

‡ *Proc. Roy. Soc. Edin.*, 1885, xiii. pp. 460–485.

§ *Proc. Roy. Soc. Edin.*, 1886, xiii. p. 545.

was at first employed. This was afterwards modified and greatly improved in one particular. The three locking springs clasping the base-plate in the original instrument were removed, and their place taken by two similar springs, emerging through windows in an outer tube and clamping the bottle by pressing on the top of the collar of the slip-cylinder after it had closed.

An exact copy of all the individual observations made during the trip of August 1886 is given in the Report to the Fishery Board presented by Dr Gibson and me. The present paper is merely intended to summarise the results, and point out some of the more general bearings of these observations.

The Moray Firth.—This great bay possesses a very interesting configuration. The northern shore (Caithness) is rocky and steep; depth increases rapidly to over 20 fathoms, and then remains as a broad submarine plateau, extending southward and eastward at an average distance of 25 fathoms beneath the surface. The western shore is shallow, the slope for some miles from land being slight; and the same remark applies to the western half of the south coast (Morayshire); the eastern half of this coast (Aberdeenshire) is again rocky, with deeper water close to. A tongue-shaped depression runs in from the north-east along the southern portion of the firth, forming a deep furrow in the plateau-like sea-bottom. This has a maximum depth of 100 fathoms in a hole 10 miles north of Troup Head, and brings water over 30 fathoms deep as a very narrow trough a considerable distance west of Burghead, and close to the south shore.

Isolated observations at various times had shown that the salinity of the great mass of water in the Moray Firth approached 3·50 per cent. very nearly. The density (at 15°·56 C.) corresponding to this proportion of dissolved salts is 1·0260, and the density usually found for both bottom and surface water by Dr Gibson in 1883, and by me in 1886, was from 1·0257 to 1·0259. The agreement of all the observations taken at intervals during three years is remarkable, and indicates that beyond the distance of a few miles from land the influence of the variations of weather (rainfall particularly), from one season to another, on the salinity is very insignificant. Temperature observations naturally do not agree so closely, for one season may easily be a few weeks in advance of another, or behind it; and the fact that the temperature is a few degrees higher or lower at any

given date than it was at the same date in some other year is of trifling importance. What must be compared, with regard to variations of temperature at one place, is not the absolute degree of warmth at a particular depth, but the vertical distribution of warmth and the annual changes of this in form and amount.

The following statement of the conditions of a section, from the Ord of Caithness to Burghead on August 19th, will illustrate the distribution of salinity in the open firth at that date. Station I. was about $1\frac{1}{4}$ miles south-east of the needle of Ord, the others each 10 miles farther south, the last being close to Burghead.

TABLE I.

Station.	I.	II.	III.	IV.
Density of surface water at 15°·56 C.,	1·02588	1·02581	1·02585	1·02510
,, bottom ,, ,,	1·02585	1·02587	1·02585	1·02573
Depth in fathoms, . . .	19	26	26	10
State of tide, ...	$4\frac{3}{4}$ hrs. fld.	$\frac{1}{4}$ hr. eb.	$2\frac{3}{4}$ hrs. eb.	4 hrs. eb.

This shows a very slight freshening towards the southern shore. The increase of salinity seaward was shown, by many isolated observations, to be gradual but steady, although no regular east-and-west section was made. The temperature from north to south, on August 19, was as follows, the figures given being corrected to readings of the Kew standard :—

TABLE II.

		I.	II.	III.	IV.
Hour, . . .		13.45	15.10	17.15	18.30
Air temp., . . .		55·2	57·0	55·5	58·0
Temp. of sea,	0 fthm.,	55·0	55·3	54·3	56·3
,,	1 ,,	—	55·2	—	56·2
,,	2 ,,	54·7	—	54·2	56·2
,,	3 ,,	54·3	54·2	—	—
,,	5 ,,	53·5	53·8	53·5	54·7
,,	6 ,,	—	53·8	—	—
,,	7 ,,	51·9	53·2	—	—
,,	8 ,,	—	53·1	—	—
,,	9 ,,	—	51·4	—	53·2
,,	10 ,,	—	—	52·3	
,,	12 ,,	—	51·2	—	
,,	15 ,,	—	51·1	—	
,,	18 ,,	51·2	—	—	
,,	25 ,,	...	51·0	50·5	

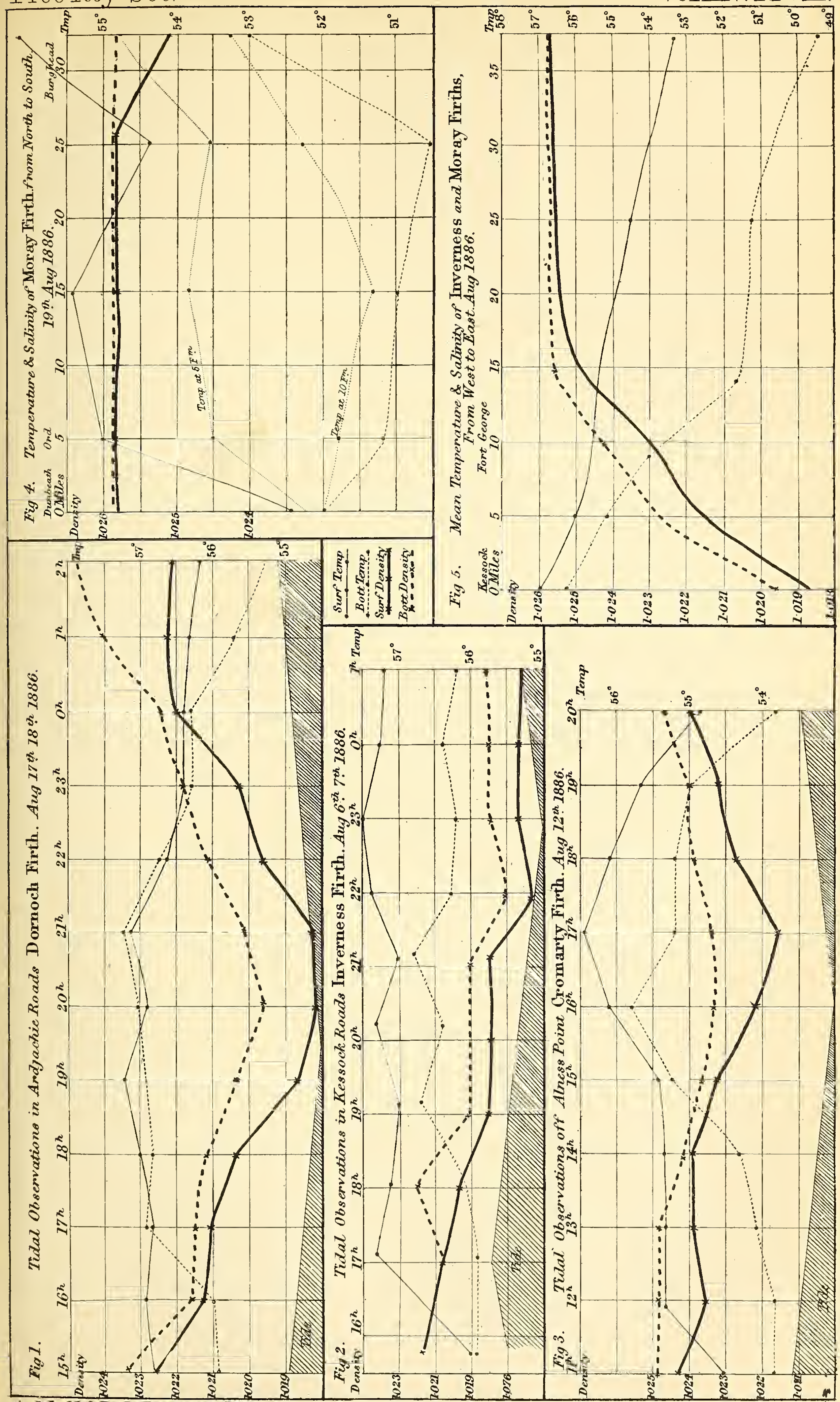
An observation made off Dunbeath Castle (further north than the Ord), in 10 fathoms, gave temperature of 52°·5 at the surface, and 52°·0 at bottom. Excepting this sounding, the observations of temperature showed a warm layer falling from 55° or more at the surface to 52° at 6 fathoms on the Caithness coast, and at 12 fathoms on the Morayshire side. The minimum bottom temperature was 50°·5 just on the verge of the deep trough off Burghead. Plate VIII., fig. 4, shows graphically the distribution in this section of surface and bottom density, and of temperature at surface, 5 fathoms, 10 fathoms, and bottom. In the depression off Troup Head, at a depth of 50 fathoms, an observation on August 23rd gave a temperature of 54°·8 on the surface and 50°·4 on the bottom. The lowest temperature of the trip was found on August 10, at the bottom of the depression off Covesea Skerries, in 33 fathoms, the thermometer reading being 49°·5. A section made from Fort George to this point (30 miles) showed a perfectly horizontal and parallel arrangement of the isotherms of 50°·5, 51°, 51°·5, 52° and 52°·5, contrasting with the dip to southward in the north-and-south section.

During the month of August the sea temperature on the west coast of Scotland was 52°·5, from surface to bottom, off the Mull of Cantyre, and in the Arran Basin, 53° or 54° on the surface, falling to 47°·5 or 48° at 30 fathoms. The Moray Firth thus appears to have been warmer than the western waters during this period.

The observations made in 1883, although not very numerous, are sufficient to show that the bottom water was practically of the same salinity then as in 1886; while the surface water near the entrance of the Inverness Firth was much fresher at the earlier date. This is quite as might be expected, since the summer of 1886 was exceptionally dry in the north-east of Scotland, and the rivers and streams were unusually low.

Taking into consideration the facts that have been ascertained, we conclude that the water of the Moray Firth is the saltest which can be found near land in the North Sea, except on the bottom of the Norwegian Gully, and possibly in the neighbourhood of the Strait of Dover, where no observations have been made. The influence of estuaries and rivers entering the Moray Firth appears to effect a local and very superficial freshening.

The data available for the three tributary firths—of Inverness,



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Cromarty, and Dornoch—are practically only those obtained in 1886, and the time over which their collection extended was much too short to make them of more than comparative value. Important series of hourly observations, extending over the greater part of a tide, were made in each firth, and to these more particular atten tion may be paid, as they confirm and extend the results obtained on the Spey in 1885, and on the estuary of the Forth at Kincardine in May 1886.*

Inverness Firth.—This inlet is narrow and full of sand-banks, which divide it up into tortuous channels of very slight depth. Near Fort George the water is a little over 10 fathoms deep, but further up 5 fathoms is about the average; and the Beauly Basin in which the firth terminates is much shallower. During the days on which observations were made, the temperature of water in the Inverness Firth was about 56° on the surface. The bottom temperature was nearly the same in the shallower part of the firth, but it fell in a very marked manner towards the sea, being 52°·7 at the bottom off Fort George, and 51°·5 off Nairn. The average density of the water at stations about 5 miles apart was as follows, but the individual readings varied greatly with the tidal phase:—

Table III.

Place.	Kessock.	Avoch.	Fort George.	Off Nairn.
Surface density at 15°·56 C.,	1·01870	1·02161	1·02269	1·02465
Bottom ,, ,,	1·01950	1·02281	1·02397	1·02547
Number of cases, . .	16	6	4	2

This shows a progressive increase of salinity seawards, and a distinctly greater salinity for bottom water at all points. The diagram, Pl. VIII. fig. 5, represents graphically the distribution of water density and temperature at surface and bottom, from Kessock, past Fort George, out into the Moray Firth, to a position off Covesea Skerries. Compared with the Firth of Forth, the rate of increase of salinity is very rapid, and the difference between surface and bottom more marked in the seaward reaches of the firth.

Numerous observations were made in the anchorage at Kessock Roads, at various depths. The data for surface and bottom only need be given here, but these are of considerable importance. The

* *Proc. Roy. Soc. Edin.*, 1886, xiii. pp. 790–799.

relations will be made more apparent by the graphic treatment adopted in Pl. VIII. fig. 2. The density given is that by a small hydrometer, and is not corrected for temperature, except in the case of the observation on the 6th, at 15^h45. The force of the wind is expressed in degrees of Beaufort's scale. The weather throughout this set of observations was clear and dry.

TABLE IV.—*Observations in Kessock Roads, off Clachnaharry.*

Date.	Hour.	Wind.	Tide.	Depth.	Temperature.			Density.	
					Air.	Surface.	Bottom.	Surface.	Bottom.
				fm.	°	°	°		
Aug. 6	15.45	WSW., 5	4½ h. fld.	5½	...	56·0	55·9	1·02028	1·02041
,,	17.10	W., 4	H. W.	6½	67·0	57·3	55·9	1·0202	1·0200
,,	18.5	...	1 h. eb.	6¾	62·5	57·1	56·1	1·0198	1·0210
,,	19.10	W., 2	2 ,,	6¾	60·4	57·0	56·7	1·0190	1·0195
,,	20.10	W., 2	3 ,,	6	58·0	57·3	56·4	1·0190	1·0195
,,	21.10	W., 1	4 ,,	5½	...	57·0	56·8	1·0190	1·0195
,,	22.0	0	5 ,,	5	58·0	57·4	56·3	1·0177	1·0185
,,	23.0	...	6 ,,	4¾	...	57·5	56·1	1·0178	1·0190
Aug. 7	0.0	...	¾ h. fld.	5	58·1	57·3	56·4	1·0180	1·0190
,,	1.0	...	2 ,,	5½	...	57·2	56·2	1·0180	1·0200
,,	5.30	...	H. W.	6½	...	...	...	1·0215	1·0210

This corresponds with results previously obtained at Kincardine, and shows most of the features more prominently brought out by observations in the firths of Cromarty and Dornoch.

Cromarty Firth.—The straight coast line running south-westward from Tarbat Ness, and bordered by a band of water under 10 fathoms in depth, is broken by the abrupt hills which define the entrance to the Cromarty Firth. Between them there is a depth of over 25 fathoms; and a clearly-cut channel, with steeply sloping sides, and more than 10 fathoms deep, runs straight west through the wide shallows on either side to Alness Point, 10 miles from the Sutors. The depth diminishes rapidly above Alness, and the channel is much choked by sandbanks. Strong tidal streams

run in this firth, and considerable variations were observed in the salinity of the water at high and at low tide. Temperature was found to fall uniformly towards the sea, the average being 55°·5 at Alness, and 54°·5 at the Sutors, on the surface; while on the bottom it was 54°·5 and 53°·8 respectively.

TABLE V.—*Average Density of Water in Cromarty Firth.*

Position.	Alness.	Invergordon.	Bet. Sut'rs.	2 m. out. Sut'rs.
Surface density at 15°·56 C.,	1·02308	1·02331	1·02465	1·02515
Bottom ,, ,,	1·02405	1·02445	1·02530	1·02546
Number of cases, . .	12	7	5	4

This shows a variation of density almost exactly the same for the 15 miles seaward from Alness as for the 15 miles seaward from Queensferry in the Firth of Forth. The identity extends to bottom as well as to surface water; but it must be remembered that the data compared are not really comparable, since they are on one side, the salinity of the Cromarty Firth in the middle of August 1886, and on the other the mean salinity of the Firth of Forth determined by numerous observations in 1884, 1885, and 1886. Also, it must be pointed out that the salinity two miles off the Sutors of Cromarty is about equal to that at the Isle of May; while five miles out in the Moray Firth, a salinity is found which, according to the German charts, is not to be met with nearer than 30 or 40 miles east of the Isle of May.

The serial tidal observations at Alness are of considerable interest. There were two sets of these. The first on 5th August, for six hours, during the last three hours of flood-tide and the first three of ebb, brought out the exact equivalence of the curves of temperature and salinity, so that, substituting "diminution of salinity" for "increase of temperature," any statement with regard to tidal influence on temperature would be true of salinity also. This series was taken rather near the mouth of the Alness river, and sudden rushes of warm fresh water produced variations on the surface which were not found at any depth beneath it. The second series was taken from 11^h0 to 20^h0 on August 12th, and as low water was at 16^h0 it comprised five hours of ebb and four of flood. The resulting figures are given in Table VI., and represented graphically in Pl. VIII. fig. 3. The densities of the

Table were all determined by the delicate hydrometer, and are reduced to their value at 15°·56 C. Weather was dull, with some showers, until $13^{h}0$, thereafter bright and dry.

TABLE VI.—*Observations off Alness Point, Cromarty Firth.*

Date.	Hour.	Wind.	Tide.	Depth.	Temperature.			Density.	
					Air.	Surface.	Bottom.	Surface.	Bottom.
				fm.	°	°	°		
Aug. 12	11^{h} 0	WSW., 2	1½ h. eb.	9	...	54·4	53·8	1·02431	1·02480
,,	12.0	WSW., 2	2½ ,,	9	56·2	55·3	53·8	1·02357	1·02482
,,	13.0	0	3½ ,,	8¼	57·8	55·3	54·0	1·02373	1·02470
,,	14.0	0	4½ ,,	8	...	55·3	54·3	1·02370	1·02422
,,	15.0	0	5½ ,,	6	58·0	55·4	55·2	1·02319	1·02369
,,	16.0	0	L. W.	6½	58 7	56·1	55·8	1·02224	1·02344
,,	17.0	E., 2	1 h. fld.	6½	58·3	56·4	55·2	1·02162	1·02336
,,	18.0	E., 2	2 ,,	6¾	56·0	56·1	55·2	1·02290	1·02391
,,	19.0	0	3 ,,	7	56·3	55·7	55·0	1·02317	1·02411
,,	20.0	0	4 ,,	7	53·9	54·9	54·0	1·02396	1·02472

Note.—The position was changed a few yards nearer the north shore between the $14^{h}0$ and $15^{h}0$ observations.

This series also shows the temperature and salinity to be in close association. Hence, considering either the one quantity or the other, the relative movements and gradual mixture or separation of the warmer and fresher upland water and the colder and salter sea water may be traced out. The salinity at the bottom remained constant for about three hours after high water, then gradually diminished until low water, and again gradually increased. Surface salinity remained practically unchanged, and very near that of the bottom water, until two hours before low tide, when it began to diminish, and came to a minimum (the surface temperature coming to a maximum) one hour after low tide. This marked the period of greatest difference between surface and bottom salinity; that at the surface proceeded to increase, presumably until high water. These observations show that flood-tide sets in first at the bottom; that the salt water first

appears there, and does not influence the surface for a considerable time.

Dornoch Firth.—This firth is shallower than that of Inverness, and in addition to its being shut off, like the Firth of Tay, by a bar at its mouth, the channels inside are narrow, tortuous, extremely shallow, and constantly changing on account of the sandbanks. The "Garland" navigated this firth under the charge of a pilot, and only two days were spent in it. It is impossible to say much regarding the variation of salinity with position, but this appeared to be more rapid than in the other inlets examined. At the Dune of Creich the density of the surface water was 1·01750, and that of bottom water 1·01919 at high tide; off Dornoch, inside the bar, the surface had a density of 1·02395, and the bottom 1·02517 at $1\frac{1}{2}$ hours ebb, the distance from the Dune being 9 miles. A few miles beyond the bar a density of 1·02588 reigns from surface to bottom. Temperature was high in the Dornoch Firth (over 57°), but rapidly fell as the sea was approached.

A very complete set of observations was made off Ardjachie Point during the last $4\frac{1}{4}$ hours of ebb tide and the whole succeeding flood, hourly readings being made for twelve consecutive hours. The data are given in Table VII., and the corresponding curves in Pl. VIII. fig. 1.

This is the most interesting record we obtained of the tidal movements of salt and brackish water past a given point. From $4\frac{1}{2}$ to $1\frac{1}{4}$ hours before low tide both surface and bottom water grew gradually fresher, while maintaining nearly the same difference in salinity, *i.e.*, the whole mass of water was moving seawards as a uniform current. At $1\frac{1}{4}$ hours before low water the rate of decrease of salinity in the bottom water diminished, that in the surface water increased, and the difference between the two grew greater. The second observation after low water showed a marked increase in the bottom salinity, while the surface was at its minimum; this shows that the current on the bottom was slowed and reversed before the outward surface current was affected. During the next hour the surface water grew salter more rapidly, and then for two hours gained on the bottom water; so that $4\frac{1}{2}$ hours after low tide the water in the channel was nearly homogeneous, as far

Table VII.—*Observations in Ardjachie Roads, Dornoch Firth.*

Date.	Hour.	Wind.	Tide.	Depth.	Temperature.			Density.	
					Air.	Surface.	Bottom.	Surface.	Bottom.
				fm.	°	°	°		
Aug. 17	15^h 0	N.W., 4	2 h. ebb.	5¾	56·5	56·8	55·9	1·02271	1·02339
„	16.0	N.W., 3	3 „	5	...	56·9	56·0	1·02116	1·02152
„	17.0	N.W., 2	4 „	5	...	56·8	56·9	1·02100	1·02137
„	18.0	N.W., 2	5 „	5	...	57·0	56·8	1·02039	1·02070
„	19.0	0	6 „	4½	...	57·2	56·9	1·01867	1·02025
„	20.0	0	½ h. fld.	3¾	...	56·9	57·0	1·01817	1·01965
„	21.0	0	1½ „	4	54·7	57·1	57·2	1·01815	1·02011
„	22.0	0	2½ „	4½	...	56·6	56·7	1·02054	1·02118
„	23.0	0	3½ „	5	53·0	56·4	56·3	1·02118	1·02184
Aug. 18	0.0	0	4½ „	5½	53·0	56·4	56·3	1·02207	1·02227
„	1.0	0	5½ „	5¾	52·5	56·3	55·7	1·02207	1·02408
„	2.0	0	H. W.	6	50·3	56·2	55·3	1·02189	1·02472

as regards vertical distribution of salinity. The following reading showed a new condition altogether; the bottom had increased in salinity very greatly, and continued to do so until high water; the surface, on the other hand, remained constant, and even showed a slight decrease. This means that after the water had been thoroughly mixed, sea-water of greater density began to push its way along the bottom, and the surface current of brackish water being no longer driven up stream by a wall of uniform salinity, resumed its downward course very slowly, and passed over the salter water without mixing with it; in fact, ebb had begun on the surface, while flood-tide continued down below. It thus appears that, so far as the tidal movement of water is concerned, the bottom of the channel in an estuary is before the surface in phase.

The question of tidal currents in estuaries is a very important one; but for its thorough investigation it requires the simultaneous work of several assistants, and a large enough staff to carry on uninterrupted observations for several successive tides. This I

have not been able to obtain hitherto; but in my work on the Spey with Mr Ritchie, on the Forth with Mr Morrison, and on the Dornoch Firth with Mr F. M. Gibson, I have fully tested the methods of studying the problem by means of observations of salinity and temperature. Salinity determinations by means of a very delicate hydrometer are certainly best in all cases; but in many, especially at certain periods of the year, the thermometer gives an equally exact picture of the state of things, with far less trouble and the cost of much less time. The collection of one sample of water from a given depth, the bottling of it, determining the density, calculating and reducing the result, occupies by my method nearly 25 minutes, and cannot be finished on the spot where observations are being made. No less-exact determination of density is of permanent value, and it is obvious that the results obtained cannot be ascertained in time to be of service in directing the course of the observations. But the temperature can be found simultaneously at three or more different depths, and the correct result arrived at in rather less than five minutes; hence, any sudden change or apparent anomaly may be detected and investigated at once. The combination of both methods is certainly best, but wherever the river water is a few degrees warmer or colder than that of the sea, I should emphatically recommend the use of the thermometer as the chief instrument for investigating the flow of the tidal currents.

No reference has been made in the foregoing to Dr Gibson's analysis of water samples collected in the region under consideration in 1883, and discussed in his Fishery Board Report. Our joint plan of work for 1886 comprised the collection of samples for chemical analysis and gravimetric determination of density. About 50 specimens of water were collected, and the analysis is now proceeding, under Dr Gibson's supervision.

I have to thank Dr Gibson for many suggestions in carrying out the part of the joint work in which I am more immediately concerned, and for his permission to publish separately the resumé of the observational results obtained.

5. On the Presence of Bacteria in the Lymph, &c., of Living Fish and other Vertebrates. By J. C. Ewart, M.D., Regius Professor of Natural History, University of Edinburgh.

During the last ten years numerous investigations have been made to ascertain whether ordinary (*i.e.*, non-specific) bacteria exist in the tissues of apparently healthy, living animals. As a result of these inquiries, it has been clearly shown that while there is no evidence of the existence of bacteria, under ordinary circumstances, in the blood of the higher Vertebrata, there is abundance of evidence of their presence in the blood of some fishes.

The existence of bacteria in fish has been specially studied by MM. Olivier and Richet. In a communication on the Microbes of Marine Fish,* Olivier and Richet point out that bacteria exist (sometimes in great numbers) during life in the peritoneal fluid, lymph, and blood of the whiting, red mullet, sand-eel, wrasse, dab, and several other fish. Of the fish examined, the authors state that (with the exception of the conger and the dog-fish) all the tissues contained numerous bacteria,—long and short bacilli being especially abundant. By cultivations it was shown that bacteria also existed in the tissues of both the conger and dog-fish. From the observations made, Olivier and Richet conclude that bacteria occur so constantly in fish that they must be almost considered as normal, and, further, that they are not putrefactive bacteria, because when they rapidly multiply after the death of their host there is no evidence of putrefaction.

In two subsequent papers (one dated 9th July and the other 17th September 1883) the original observations are confirmed, and it is further pointed out that bacteria are especially numerous in the peritoneal cavity, and less numerous in the pericardial sinus, the cerebro-spinal canal, and the blood of the heart, and that under certain conditions the bacilli are mobile, and capable of being cultivated.

I have recently had the opportunity of examining the blood, &c., of a number of both marine and fresh-water fishes, and I am able

* *Compte Rendu*, tome xcvi., Février, 1883.

to confirm to a certain extent Olivier and Richet's observations. Although I had often examined microscopically the blood and tissues of fish, it was not until recently, when at work in the Oxford Physiological Laboratory, that I was convinced that bacteria are often present in immense numbers in the peritoneal fluid, and in smaller numbers in the blood of apparently healthy fish.

I first noticed bacteria in the blood of a roach (*Leuciscus rutilus*). This roach, for some hours before it was taken from the water, had been occasionally swimming on its side at the surface,—an indication that it was in an exhausted condition. Immediately after the fish was killed, a drop of blood taken from the heart by a sterilised pipette (with all the necessary precautions) was found to contain a considerable number of slender motionless bacilli measuring from ·003 to ·008 mm. in length. On an average four bacilli were visible in the field at a time with Zeiss's F objective and No. 1 eyepiece. The peritoneal fluid, which was next examined, contained so many bacilli that it was impossible to count them; the bacilli were usually lying amongst large granular lymph cells, and they were longer and more slender than those in the blood. Similar bacilli were found in the lymphatics, spleen, liver, and kidney, and they were abundant in the muscles in contact with the peritoneum; while very few were found in the muscles under the skin of the trunk, and still fewer in the muscles of the tail. The intestine was crowded with similar bacilli to those found in the body-cavity, and in addition there were a number of large and small bacteria and micrococci. Bacilli were also found in the walls of the intestine and in the bile duct.

Believing that there was some relation between the diminished vitality of the above roach and the numerous bacilli in the tissues, I examined a considerable number of healthy roach and also other fresh-water fish, *e.g.*, trout (*Salmo levenensis*), perch (*Perca fluviatilis*), carp (*Cyprinus auratus*), and eels (*Anguilla vulgaris*). In all the healthy specimens examined, with the exception of the trout, bacilli were found in the body-cavity. Bacilli were also present in the blood of the carp, and on one occasion four bacilli were detected in a drop of blood from what appeared to be a healthy roach. In some the peritoneal fluid contained numerous bacilli, while in others only a few were visible; generally there was some relation between

the number in the body-cavity and the number in the intestine, and they were most abundant in fish which had lived for some time in aquaria without food; but in trout which had been fasting for at least ten days no bacilli could be observed in the peritoneal fluid. The carp which had bacilli in their blood had been living for some months in a small glass aquarium.

The difference between the roach first examined and those examined subsequently led me to endeavour to ascertain whether a sudden change of temperature would produce any influence in the number and distribution of the bacilli. As I anticipated, a rapid change from a spring to a summer temperature (from 48° to 65° F.) greatly diminished the vitality of all the fish experimented with, except the carp. As the fish became more and more exhausted, the bacilli gradually increased, and when the temperature was raised from 48° F. to 65° F. in two hours, the bacilli of the peritoneal fluid not only increased in the roach, perch, carp, and eel, but they made their appearance in considerable numbers in the body-cavity of the trout, and on one occasion, a number of small bacilli were found in the blood of a trout. Although the carp seemed to enjoy the rise of temperature, they were not exempt from the increase of the bacteria in the blood as well as in the peritoneal fluid. In some specimens of blood as many as eight short slender bacilli were visible in the field of the microscope at one time, and the peritoneal fluid, in some instances, swarmed with long and short bacilli, some of which were mobile.

In some of the roach, in which no organisms could be detected in the blood, bacilli were found in the muscles immediately external to the peritoneal cavity. Further, bacilli were always abundant in the muscles of roach which had suffered from a sudden rise of temperature. The above observations were confirmed by cultivations in gelatine, agar-agar, and infusions of fish muscles. In healthy active specimens of the roach and perch cultivations were easily obtained of the peritoneal bacilli, and generally also from the muscular fibres lying near the peritoneum, but in no instance did I succeed in obtaining cultivations when the blood, or the muscles from immediately under the skin, were used for infecting the culture-media.

Of the sea fish examined, I have found bacilli, sometimes long and

slender, sometimes short and thick, in the peritoneal fluid and blood of the whiting (*Gadus merlangus*), haddock (*Gadus æglefinus*), cod (*Gadus morrhua*), and herring (*Clupea harengus*), and in the peritoneal fluid only of the flounder (*Platessa flesus*), plaice (*Platessa vulgaris*), and lumpsucker (*Cyclopterus lumpus*). I have not hitherto succeeded in demonstrating the existence of bacteria in either the peritoneal fluid or blood of the skate (*Raia batis*), dog-fish (*Acanthias vulgaris*), or fishing-frog (*Lophius piscatorius*).

Perhaps the difference in the number and distribution of bacteria in the sea fish examined by Olivier and Richet and those I have recently studied may be accounted for, either by a difference in the temperature of water from which the fish were taken, or by the fish having been longer under less favourable conditions in the one case than in the other.

It is extremely desirable that a continuous series of observations should be carried on throughout the year, in order to ascertain whether bacteria are more abundant in summer than they are in winter, whether they increase or diminish before and during the spawning period, and whether the bacteria indirectly influence the migration and distribution of fish—the fish which readily suffer from an increase of the bacteria in the peritoneal cavity either remaining in comparatively cold seas or selecting cold currents when they migrate in search of food, or in obedience to their spawning instinct.

There can be no doubt that the bacteria enter the body-cavity by penetrating the walls of the intestine, neither can there be any doubt that having once established themselves in the peritoneal fluid they do their utmost to find their way into the blood and tissues. It may be taken for granted that ordinary bacteria flourish in the intestinal canal of all vertebrates, and that they assist in digestion by helping to disintegrate the food particles. Notwithstanding the presence of active bacteria in the intestinal canal and the bile and pancreatic ducts, I have failed to discover either bacilli or micrococci in the body-cavity of either amphibia, reptiles, birds, or mammals when in a healthy condition. Hence it may be taken for granted—(1) that in the higher vertebrates under ordinary circumstances the walls of the intestine form an effective filter or screen which prevents the passage of the bacteria into the body-

cavity, or (2) that the living cells of the mucous and other layers so act on the bacteria that they are destroyed before they reach the body-cavity, or (3) that the cells of the peritoneal fluid effectively sterilise the bacteria which succeed in entering, or (4) that the bacteria are destroyed as they pass along the lymphatics towards the general circulation. The results which follow the injection of septic and other solutions into the body-cavity of rabbits are considered at length in the Lumleian Lecture given by Dr Burdon Sanderson in March 1882. From the experiments referred to, it was made clear that whenever the solution could not be at once absorbed without any irritation being set up, bacteria rapidly appeared in the body-cavity, and caused death by producing poisonous bye-products. In many fish, on the other hand, bacteria not only reach uninjured the body-cavity, but continue to live there in considerable numbers without disturbing seriously, if at all, the vital processes of their host,—in other words, most fish seem capable of tolerating the presence of one or more kinds of bacteria in the peritoneal fluid, whilst others can even tolerate considerable numbers in their blood. It seems, however, that there is a limit to this toleration, for when the equilibrium is disturbed, when by a change of the surroundings the vitality of the tissues is diminished, the bacteria rapidly increase, and unless the tissues recover the position they have lost, the bacteria may directly or indirectly cause death. It has been suggested by Metschnikoff and others, that bacteria are kept in subjection chiefly through the influence of the colourless blood corpuscles. This may be so in some cases, but it may be taken for granted that the living tissues as a whole repel the advance of the destructive organisms, and that some bacteria are arrested and destroyed by one tissue, while other bacteria are sterilised by another. A very small swing of the balance may determine whether a given bacterium will develop or not. This may be inferred from the behaviour of culture-media, *e.g.*, whether gelatine will act as a suitable medium for a given bacterium may depend on its reaction or on the amount of moisture it contains. In the same way, whether a given bacterium is able to disintegrate a piece of muscle may depend on the reaction or rigidity of the muscle.

The distribution of bacilli in the tissues of fish, in which the conditions were favourable for their growth, is somewhat remark-

able. The fact that even when the bacteria have extended into numerous lymphatics, and even into the substance of the muscles surrounding the body-cavity before they are found in appreciable numbers in the blood, seem to indicate that the blood is most active in destroying bacteria. Again, seeing that although, when bacteria exist in considerable numbers in the inner layers of the myotomes of the trunk, they are often entirely absent (as proved by cultivations) from the outer layers of the same myotomes, it may be inferred that the muscles also have considerable power in preventing the spread of bacteria. From the observations made it appears that bacteria travel easiest along the lymphatic canals and spaces—the lymph cells being apparently less able to arrest their progress than the blood corpuscles.

As to the nature of the bacilli found in fish nothing has hitherto been determined. Olivier and Richet seemed to think they are neither specific nor putrefactive. At first I thought they were putrefactive, but not specific. Having made some further experiments, I am now inclined to consider them specific, and not putrefactive. I was led to believe they were putrefactive, because I found the characteristic long delicate bacilli of the body-cavity in immense numbers between the mascular fasciculi of fish in which putrefaction had already set in. A perch, *e.g.*, which died having the body-cavity and the blood well charged with bacilli, was placed in a chamber with the temperature at 38° C. Fifteen hours afterwards the mascular bundles, even near the root of the tail, were almost completely enveloped with bacilli identical to those in the body-cavity, the bacilli filling up the inter-muscular spaces, and forming large irregular patches around the bundles. In this fish, twenty-four hours after death, micrococci and bacteria were extremely few in number, but before the fish had been forty-eight hours in the warm chamber the bacilli had largely disappeared, and, in their place, busily engaged breaking up the mascular fibres, first into filaments and then into small short segments, were numerous small bacteria and micrococci. A trout, which contained bacilli in nearly all the tissues during life, was placed in a solution of phenol (5 per cent.) sufficiently long to destroy the organisms in and around the fish (the intestine having previously been removed) without reaching those in the muscles, and then transferred into

sterilised water, and kept at a temperature which varied between 50° and 65° F. Ten days afterwards the muscles had undergone no marked change; they were certainly not putrefying, and yet living bacilli were sufficiently abundant in and around the fibres composing them. The importance of the bacilli so often found in fish being non-putrefactive and being apparently non-morbific, *i.e.*, not being associated with any special disease, will be readily understood. Were they putrefactive, the preservation of fish as food would be extremely difficult, and the danger of suffering from the presence of noxious bye-products in the flesh of fish still greater than it is at present. There is scarcely any escape from the conclusion that the bacilli, as long as they survive after the death of their host, must tend to the formation of bye-products of some kind. Whether these bye-products have any influence in producing the characteristic flavour of somewhat high fish it is impossible to say, but it is extremely probable. In game in a high condition I have always found bacteria, but even in grouse which had been kept for three months during winter, very few putrefactive bacteria were found in the large pectoral muscles.

Further observations will probably show there is a relation between the facility with which bacteria penetrate into and survive in the muscles, and what might be called their innate vitality. In fish, in which relatively the percentage of water in the muscles is low, and the fatty constituents high, the bacteria may be less able to flourish than in fish in which the opposite conditions obtain. Again, there seems to be a relation between the number of bacteria present in any given fish and the time at which putrefaction takes place. This, as observed above, is apparently not necessarily a relation of cause and effect. The presence of numerous bacteria seems to be an indication of diminished vitality, an indication that the muscles will fall a ready prey to putrefactive bacteria as soon as they make their appearance.

Olivier and Richet conclude their second paper as follows:—

"En resumé, nous croyons pouvoir conclure qu'il y a toujours ou presque toujours des microbes dans les liquides lymphatiques des poissons, et per consequent dans l'intimaté de leurs tissus."

This conclusion was apparently arrived at chiefly because, by

means of cultivations, they convinced themselves that bacteria were always present in the living tissues.

It will be instructive to quote one of their culture experiments. The second experiment mentioned in the paper of the 9th July is as follows :—

"Le 19 Juin, on econche avec des ciseaux rougis la queue d'un gros Squale venant de lam er. On la trempe pendant soixante-dix secondes dans un bain de paraffine a 218, puis on l'expose quelque instants a la flamme d'une lampe de manière a brûler la peripherie. Le fragment ainsi sterilizé quant à sa surface est plongé rapidment dans un flacon rempli de paraffine liquide. Flacon et paraffine ont éte sterilizes au prealable par une temperature de 160° prolongée pendent deux heures et demie, et l'uair n'a pu y rentrer pendant le refroidissement qu'a travers un tampon d'ouate. Le flacon n'est reste librement a l'air que pendant le temps strictement necessaire pour introduire le poisson.

"Le 29 Juin la chair musculaire n'a ancune odeur. Elle presente l'aspect et l'odeur du poisson frais. Elle contient des Bacilles extrémement nombreux, peu mobiles."

From analogous experiments I have obtained somewhat different results. For example, trout, roach, and eels which were gutted immediately after death and introduced for a short time into a 5 per cent. solution of phenol, and then transferred into sterilised water, remained unchanged for weeks. When examined, dead bacteria were found on the surface of the skin and in the peritoneal lining of the body-cavity, but no living bacteria could be detected in the muscles, nor did they appear in cultivations into which fragments of muscle had been introduced. As was anticipated, when the fish were placed in ordinary water, putrefaction at once set in. The same results were gained by varying the experiment. A trout was killed, and a strip of muscle 5 inches in length was removed under antiseptic precautions from one side, and introduced into a flask of sterilised water. The flask was kept for five days at a temperature of 65° F. without any change taking place in the muscular fibres, or any bacteria making their appearance either in the fibres or in the water.

Hence in the meantime it may be taken for granted that while bacteria exist in the tissues of some fish even at a comparatively

low temperature, they are not always, if ever, present in the tissues of others.

This inquiry was carried on partly in Oxford and partly in Edinburgh. I am much indebted to Dr Burdon Sanderson, Waynflete Professor of Physiology in the University of Oxford, for affording every facility his well-equipped laboratory could offer, and for valuable advice, during the investigation.

Literature.—The memoirs which bear directly on this investigation have been already referred to. A list of papers dealing with the existence of bacteria in living tissues will be found in the *Handbuch der Hygiene der Gewerbekrankheiten*, 1 Theil, 2 Abtheil, 1 Heft. The following papers may be specially mentioned :—

(1) Meissner, *Deutsche Zeitschrift für Chirurgie*, Bd. xiii., 1880, p. 3446.

(2) Rosenbach, *Deutsche Zeitschrift für Chirurgie*, Bd. xvii., 1882, p. 342.

(3) Bonnet, *Ichthyopathologischer Jahresbericht der Münchener Thierarznei Schule*, 1882–83.

PRIVATE BUSINESS.

Mr J. R. Dunstan, Mr Cosmo Innes Burton, and Mr Adolf P. Schulze were balloted for, and declared duly elected Fellows of the Society.

Professor Duns read a letter from Dr R. H. Gunning, intimating his wish to found a prize, or prizes, to be known as the Victoria Jubilee Prizes, to be awarded every three years. The Society agreed to accept the trust, and to record their cordial thanks to Dr Gunning, and remitted to the Council, along with Professor Duns, to arrange details in accordance with Dr Gunning's wishes.

Monday, 20th June 1887.

Sheriff FORBES-IRVINE, Vice-President, in the Chair.

The following Communications were read:—

1. On the Origin of the Great Alpine Lakes. By Professor Federico Sacco, University of Turin.

Among the many and various controversies to which the geological study of the great chain of the Alps has given rise, not the least interesting is that which has reference to the origin of the beautiful lakes which occur most numerously in the lower reaches of the mountain valleys. None of the theories hitherto set forth seems to me to explain the origin of these remarkable basins, and in place of these I now venture to adduce one of my own, which has been suggested by some years' observations on the Tertiary and Quaternary accumulations of the valley of the Po. Of course, it will be understood that I am far from denying that lacustrine basins may owe their origin to many various causes; and for lakes in general I am inclined to adopt some such classification as the following:—

Lake-basins formed by orographic features, as by .	Flexures of strata. Fractures. Elevation. Subsidence. Faults. Superficial inequalities of deposits. Crateral hollows.
Lake-basins formed by dams or barriers, as by . . .	Morainic accumulations. Ice. Alluvial deposits. Dunes. Littoral banks of sand, &c. Landslips and rock-falls. Drift-wood. Lava. Coral reefs. Beaver dams.
Lake-basins formed by erosion, as by	Water, both as a subaërial and subterranean agent, causing local subsidences by removal of materials in solution, &c. Ice. Wind.

It is not, however, with lakes in general that I am now about to deal, but with our Alpine lakes in particular. In commencing the

study of those lakes, we must, in the first place, transport ourselves in imagination to that epoch of powerful earth-movement which, according to most geologists, closed the Miocene period in the Alpine lands, and gave to that mountain-region its last general upheaval. It was during this epoch of powerful movement that, according to common belief, the Alps received their present orographic features, while many geologists were of opinion that the formation of the great Alpine lake-basins ought to be assigned to the same epoch of disturbance. With this latter opinion I cannot agree. On the contrary, I have been led to conclude that the movement of upheaval which brought the succeeding Pliocene period to a close was of much greater extent than that which took place after Miocene times; and therefore, so far as regards the question at present under review, viz., the origin of the Alpine lake-basins, the Post-pliocenic movement is much the most important.

Be this as it may, it is quite certain that with the close of Miocene times marine conditions entirely disappeared on the northern side of the Alps. After that date the only deposits laid down in that region are of fluvio-lacustrine, fluviatile, and glacial origin; and as none of these contains fossils, they do not furnish us with a sufficiently exact basis for the study of the phenomena which have taken place at the foot of the mountain-region since Miocene times. By various geologists these unfossiliferous deposits, which are in general gravelly in character, have been assigned to the Messinian, to the Piacentian, and even to the Astian stage, and in large measure also to the Quaternary. It is probable that during each of these epochs some of the deposits in question were formed, but as the classification of the latter is still far from being established, it is better for our present purpose that we should confine our attention to the post-miocenic accumulations which occur on the south side of the Alps. These, unlike those of Switzerland, are mostly marine and fossiliferous, and therefore afford us a more secure basis for the study of the question at issue.

In the valley of the Po the Messinian is well marked, especially at the foot of the Apennines, where it contains gypsum and marls (with *Dreissena, Melania, Melanopsis, Neritina, Paludina, Cerithium*), arenaceous, and calcareous beds; in other words, the Mes-

sinian consists principally of marshy and lagoon formations, in strong contrast to the underlying Tortonian, which is chiefly of deep-sea origin.

Along the foot of the Alps the Messinian is very poorly developed, and has been little studied. In the Eastern Alps, however, there occurs a certain old alluvial deposit, cemented into hard rock, and containing terrestrial and lacustrine fossils. It has been elevated and much disturbed, but here and there is seen to overlie the Tortonian, while elsewhere it lies abruptly against much older formations. This alluvium M. Taramelli is inclined to include in the Messinian; while M. Rossi * has assigned to the same geological horizon the extensive marshy deposits of the province of Treviso.

But if in Northern Italy so strong a contrast exists between the deposits of the Tortonian and Messinian epochs, while on the northern side of the Alps the Upper Miocene strata are poorly developed, surely we must infer from this that a powerful movement of elevation affected the region of the Alps and Apennines in post-Tortonian times. Is it not evident that this upheaval finally banished the sea from the northern side of the Alps, where it had so long prevailed, while on the south side of the Alps it changed the large and deep Tortonian gulf of the valley of the Po into a region of lagoons, low lands, and marshes? Is it not to this period that the existing orography of the Alps, at all events in its general outlines, ought to be assigned?

To this movement of powerful and wide-spread elevation succeeded another movement, also of great intensity, but in the opposite direction. Thus in the valley of the Po the marshy accumulations of the Messinian are overlaid directly by the deep-sea deposits of the Piacentian. No marine deposits of Pliocene age occur on the north side of the Alps, and Swiss geologists therefore do not admit that the subsidence referred to affected their country. For my part, I think it highly probable that the movement in question did affect the whole mountain-region, but with varying intensity. The absence of marine Pliocene on the north side of the Alps I would attribute to the relatively higher position of that region above the sea-level. In consequence of the upheaval of late

* "Note illustrative alla carta geologica della Provincia di Treviso," *Boll. Soc. Geol. Ital.*, vol. iii. 1884.

Miocene times, the sea retreated from Switzerland, and hence the succeeding deposits consist, not of marine, but terrestrial and lacustrine beds. These I take to be representative of the Tortonian and Messinian of Italy. During the following Piacentian epoch the sea invaded the valley of the Rhone, and reached as far as Lyons, but did not approach nearer to Switzerland.

It is to be noted in this connection that the invasion of the Piacentian Sea was not general, even for the south side of the Alps, for deposits of that age are wanting in Venetia, east of Lake Garda. It would appear, therefore, that the post-Messinian subsidence was not nearly so well marked in this particular region as in that which lay further to the west. Thus the Venetian districts, with their continental deposits of Pliocene age, show phenomena analogous to those met with on the northern side of the Alps.

Towards the middle of the Pliocene period, a movement of elevation was again initiated. This appears to have been somewhat rapid in certain regions, for we find in places blue marls, with a deep-sea fauna, overlaid directly by yellow sands charged with fossils of littoral habitats. In other places the same deep-sea strata are covered by continental accumulations, pointing in like manner to a more or less rapid upheaval. In yet other places, however, we find evidence of a gradual change from deep-sea to shallow-water conditions, showing that the elevation may, after all, have been rather protracted than rapid.

The distribution of the arenaceous marine deposits of the Astian along the base of the Apennines (where they are widely and almost continuously spread), and here and there also at the foot of the Alps (such, for example, as the marly beds of the Piacentian), leaves one in no doubt as to their stratigraphical position. At the foot of the Alps, however, or at a little distance from these mountains, we encounter certain gravelly deposits, generally quite unfossiliferous, and having a prevalent fluviatile character. These gravels, according to some geologists, correspond in age to the yellow marine sands of the Astian; by others they are regarded as Quaternary accumulations. And so in Italy, as in Switzerland, there is the same difficulty as to the precise stratigraphical position of these deposits. But while in Switzerland their horizon has been variously assigned to any stage—from the Messinian to the Quaternary—in

Italy they can only be of Astian or Quaternary age. Indeed, it seems to me probable that just as in Switzerland, those unfossiliferous accumulations may truly belong in part to each of the stages referred to—namely, to the Messinian, the Piacentian, the Astian, and the Quaternary—so the unfossiliferous conglomerates in the valley of the Po may belong in part to the Upper Pliocene, and in part also to the Quaternary. I shall not attempt at present, however, to make this distinction, because it is still matter of doubt, and would lead me into too long a discussion of what, after all, are local details. Nevertheless, I should like to point out some of the more important results obtained from a geological examination of the upper valley of the Po. These may be summarised as follows :—

1. In certain parts of Piedmont, at a distance of more than 50 kilometres from the Maritime Alps, with their important valleys and rivers (as, for example, between Villanuova and Villafranca, Asti), there occur fluviatile and lacustrine deposits, consisting of marls, sand, gravel, and conglomerate, which sometimes attain a thickness of 100 metres, and which from their fossils, studied by me for some years, I judge to be of Pliocene age (Villafranchian of Pareto). These alluvial deposits rest upon the yellow sands of the Astian, which in that district are of inconsiderable thickness.

2. In the valley of the Stura (Cuneo),* and in certain other districts of Piedmont, one may see the yellow marine sands of Astian age thinning off towards the mountains, taking on by degrees the character of true littoral deposits, and then of marshy or lagoon-like accumulations. Followed nearer the mountains, these accumulations are covered and replaced by gravelly, sandy, and argillaceous alluvia, which at first are probably marine, but seem to pass laterally into true continental deposits. The numerous fossils found by me in these beds prove the latter to be of Upper Pliocene age.

3. In other regions of Piedmont, but nearer the mountains, as, for example, between Morozzo and Villanuora, Mondovi, the alluvia in question repose conformably upon the marine blue marls of the Piacentian, presenting in this manner a well-marked parallelism with the yellow marly sands, which at a distance of only two kilometres represent the marine Astian, and overlie the same horizon of

* F. Sacco, "La valle della Stura di Cuneo dal ponte dell' Olla," &c., *Atti Soc. It. Sc. Nat.*, xxix. 1886.

the Piacentian marls. These facts seem to me to demonstrate the synchronism of the alluvial continental deposits nearer the mountains with the marine beds of the Astian.

Now since we find that the yellow marine sands of the Astian are represented along the foot-slopes of the Apennines by more or less extensive gravelly, conglomeratic, torrential accumulations, and since in the higher parts of Piedmont we encounter continental deposits of undoubted Pliocenic age (which attain a thickness of even 100 metres at a distance of more than 50 kilometres from the Maritime Alps, and rest directly upon the marine Astian), it seems only reasonable to expect that similar continental accumulations ought to be met with occupying a like geological position at the foot of the Central Alps. Indeed, when we consider the more extensive drainage area of this latter region, its larger valleys and more imposing water-flow, we can hardly doubt that more or less extensive alluvia, synchronous with the Villafranchian of Piedmont, must have been deposited during the second half of the Pliocene by the great rivers then descending to the Pliocene sea. And these alluvia would form irregular deltas, now and again anastomosing and dovetailing, and spreading out from the Alps towards the Apennines. That great alluvial accumulations do occur along the foot of the Central Alps is of course well known, and the only question therefore that remains for discussion is the classification and correlation of those deposits. Unfortunately, owing to the fact that the cuttings made by the river-courses in the plains of the Po are generally of inconsiderable depth, the whole thickness of the alluvia is not seen, and the determination of the deposits therefore is not an easy matter. As a rule, it is only the superficial Quaternary conglomerates that are exposed in sections. For the same reasons which induce me to believe that along the base of the Alps in Italy very extensive Pliocenic alluvia exist, I am of opinion that a large proportion of the alluvial accumulations, more especially the conglomerates, which occupy a similar position at the northern foot of the Alps, ought to be assigned to the Pliocene rather than the Quaternary.

But the second stage of the Pliocene period was characterised not only by the commencement of the elevation of the Alpine and Apennine regions, and by the accumulation of the marine and continental deposits already referred to, but by the initiation of those

glacial conditions which subsequently attained so great a development. Even at an earlier stage than this, namely, in the Astian epoch, the Alpine snow-fields and glaciers probably reached a notable development, especially in the northern part of the chain, where the geographical and orographical conditions, together with distance from the sea, would necessarily exert an influence favourable to glaciation. For these reasons, I incline to think that the *first glacial epoch* of Swiss geologists coincided generally with the closing stage of the Pliocene. If, as I believe, the first notable extension of glaciers began in Astian times, then we should expect to encounter on the south side of the Alps very considerable alluvia of Pliocene age, extending outwards from the mountains far into the plains. And this is just what I do find.

The actual cause of this former great extension of the Alpine glaciers I would assign to evaporation from a much wider water area than presently exists. Much of what is now dry land in Northern Italy was then submerged—the water being partly that of the sea, partly lacustrine. The vapour rising from these submerged areas, passing north over the Alps (which at that time were being powerfully upheaved), would be precipitated as snow, and so would eventually give rise to glaciers. It must be remembered that the extraordinary glacier-development in question has, in all probability, not been the first to have taken place in the Alps. At various horizons in the Tertiary strata great erratic blocks have been met with. More especially is this the case with the Miocene of the hills near Turin, where, scattered through sandy, marly strata of marine origin, occur enormous blocks, angular in shape, which could only have been carried by ice. It seems most likely that the icebergs or ice-rafts by which they travelled were detached from the front of the glaciers descending from the Alps into the sea of Miocene times.

But if the movement of elevation began to be manifested more or less pronouncedly during the Astian epoch, it was yet gradual enough to allow of the continued accumulation of the deltas, which, step by step, were compelled to recede from the foot of the Alps. At the end of the Pliocene period, however, the movement assumed extraordinary intensity. Thus, the lower Pliocene (Piacentian) of deep-sea origin were uplifted 350 or 400 metres, and even more

than 500 metres in some sub-Alpine regions, whilst the upper yellow sands (Astian) in the vicinity of the Alps were at certain points raised more than 560 metres. In the sub-Apennines, facing the Alps, the same deposits were uplifted 700 metres, and in Southern Italy over 1000 metres. This movement, as I believe (and not that which closed the Miocene period), was the last great elevation of the Alps. It is to this Pliocenic movement that I attribute the general orographic settlement of the Alps. And it is to this last great elevation of the Alps that I chiefly assign the formation of the existing lake-basins of the sub-Alpine regions. These I believe to be due partly to faults—often bifurcating as they pass down the valleys,—and partly to the accentuation or formation of synclinal folds, and to local uplifts and subsidences.

After this period of great elevation the Alpine glaciers, which had already in the second stage of the Pliocene become strongly developed, were now, owing to the changed orographic conditions, compelled to form in cirques differing in shape from those of Pliocene times, and to seek new paths in their descent to the low grounds; but, erelong, making their way through deep valleys newly opened, and preceded by the deposition of diluvial deposits from the waters escaping from them, they reached the plain, and piled up their great end moraines, forming the well-known morainic amphitheatres opposite the mouths of the great Alpine valleys. Underlying these terminal moraines, therefore, we always find a more or less thick accumulation of diluvial conglomerate—the induration of the deposits being due sometimes to infiltrated calcareous matter, and sometimes apparently to the pressure exerted by the glacier-ice which overflowed the gravels.

During the somewhat rapid descent of the glaciers to the low grounds it seems obvious that the terrestrial waters which escaped from them would accumulate in the lake-basins, the bottoms of which would thus tend to be raised; while the glaciers themselves, when they reached those basins, would take some time to fill them up. Before the glaciers could escape from the lacustrine troughs, very considerable masses of gravel and shingle would be swept out by the rivers and torrents, and spread over the low grounds that extend outwards from the mountains. When at last the glaciers

debouched upon the plains, their path therefore lay over a region more or less thickly covered with gravelly deposits, and we need not wonder therefore at the great thickness attained by the conglomerates which we now meet with underneath the great terminal moraines of Piedmont, &c. The occurrence of these conglomerates has long been well known, ever indeed since attention was first directed to them by Martins and Studer some forty years ago.

The "morainic amphitheatres" and the underlying and associated diluvial gravel, &c., are the characteristic accumulations of the glacial period, and correspond, in my opinion, to the similar accumulations which, according to Swiss geologists, belong to what they term the "second glacial epoch." From all the Alpine valleys at this period powerful streams and torrents descended, and their products occur not only opposite the mouths of the greater Alpine valleys which contained large glaciers, but spread out also into the plains and low grounds opposite mountain-valleys in which no glaciers appear to have existed. To these deposits various names have been assigned, such as "Quaternary alluvia;" "fluvio-lacustrine alluvia," "diluvium," "cônes de dejection," "Areneano" = (gravelly sands with remains of *Elephas primigenius*, *Megaceros*, *Cervus euryceros*, &c.); "Ferretto," &c.

It is unnecessary, however, to pause longer over this period, the general conditions of which, so far as they relate to the Alps, are sufficiently well known. The eventual decadence of the great glaciers was, in my opinion, brought about by the gradual and general elevation of the continent, and the consequent disappearance of many wide marine and lacustrine areas. By the gradual disappearance of those water areas evaporation was progressively diminished, until the atmospheric precipitation on the Alps was reduced by one-tenth, consequently the glaciers, for lack of aliment, gradually retreated, and the great troughs in the mountain valleys became lacustrine basins. (Probably, also, the gradual lowering of the temperature of the globe may have had something to do with diminished evaporation and precipitation.) Now, the lakes are being gradually filled up by the sediment washed into them by streams and rivers, so that the geologist can foresee a time when they will become in this way entirely silted up. In Post-glacial times the streams were not of such importance as those of the Glacial period,

which in many respects may be looked upon as a period of torrents and flooded rivers. Since the close of that period the rivers and streams have been engaged in cutting down through the glacial and fluvio-glacial accumulations, so that in some places they have succeeded in reaching the Pliocene, and even the Miocene deposits. This is especially the case in localities where a movement of upheaval had been longest continued, or where it had been most pronounced. The erosive action of Post-glacial times having resulted in the formation of alluvial terraces in the valleys over many wide regions, we term this period the "Terracian."

Having now sketched in outline the phenomena connected with the structure and origin of the great lake-basins of the Alps, I may sum up in a few words my general conclusions. I am of opinion, then, that these basins came into existence during that powerful upheaval which closed the Pliocene—that, in short, they are the direct result of that great movement. They owe their origin partly to fractures and foldings of the strata, partly to subsidences and elevation. They were preserved during the glacial period by the glaciers which occupied them, and were only modified to a slight degree by morainic obstructions, and by fluviatile and glacial erosion.

The form and distribution of the Alpine lakes seem readily explained according to my views as follows:—

1. Along the south-east margin of Lake Garda the strata present distinct folds, the axes of which run generally from west to east. In other words, the undulations and folds of the strata along the eastern side of the lake are approximately parallel to the plain of the Po. Further to the east their direction is mostly from north-west to south-east. The absence of great lake-basins in the Venetian Alps is noteworthy, and may be accounted for in various ways. It is not unlikely, in the first place, that fractures and faults would tend to take place more in the direction of the folds than perpendicular to them; again, during the last great upheaval, when new foldings took place, these would probably be formed parallel to the pre-existing ones, and only rarely perpendicular to them. Finally, the Venetian Alps, according to Taramelli, experienced a less degree of elevation in Post-pliocene times than the regions lying to the west. Such considerations should lessen our surprise, that no great lake-basins occur in the mountain-valleys east of Lake Garda.

2. Upon the south-west margin of that lake the foldings of the strata run usually from south-west to north-east—a direction which coincides generally with that maintained by the anticlinal and synclinal axes of the Western Alps, and explains why the Central Alps advance so much further into the plain of the Po than the Eastern Alps. By this arrangement of the axes in the region under review, it is obvious that the folds of the strata are directed approximately perpendicular to the plain of the Po. Now, since the faults and foldings which followed the trend of the original undulations must have been both numerous and important; and as the Post-pliocene elevatory movement which took place in the Alpine region to the west of Lake Garda was very powerful, it is only natural that great sub-Alpine basins should have been formed in that region, the general trend of these basins being perpendicular to the plain of the Po.

3. In that region where the discordancy between the undulations of the Central Alps and those of the Venetian Alps is most marked, we ought to encounter the largest faults and most pronounced foldings of the strata. Now, it is just in that particular region where Lake Garda occurs—a lake which advances further than any of the other Alpine lakes into the plain of the Po, and which in places exceeds the great depth of 800 metres—so that its bottom is nearly 800 metres below the level of the sea.

4. The larger foldings which occur along the southern area of the Eastern Alps are directed, as I have said, parallel to the plain of the Po, thus differing notably from the Post-miocene and Post-pliocene undulations of the Central Alps. This direction of the folds of the Eastern Alps, taken in connection with the disposition of the Alpine chain in a strong curved line, seems sufficient to account for the absence of deep lacustrine basins at the foot of the Venetian Alps.

5. The northern region of the central area of the chain (including a large portion of the German Alps and the eastern part of Switzerland) having experienced conditions very similar to those which have affected the Alps of Lombardy—for, doubtless the powerful elevation that closed the Pliocene epoch influenced the whole area in question,—we there meet with deep lacustrine basins similar to those of Northern Italy. These basins we see extend in a direction

perpendicular to the axes of the Alpine chain, and are sometimes, as in the case of Lake Constance, bifurcated downwards, like not a few Italian lakes.

6. In the more eastern part of Switzerland the phenomena referred to in the preceding paragraph were complicated by the proximity of the Jura Mountains, a chain which runs parallel to that of the Alps. Hence the deep lake-basins were formed rather parallel than normal to the axes of those chains.

7. It is worthy of note that the orographic axes of the syncline of the valley of the Po lies near the base of the Alps. This explains why the lacustrine troughs, lying for the greater part in solid rocks, shelve off and end near the plain. We must remember also that the Post-pliocene elevation of the Apennines, where the Pliocene strata often reach an elevation of more than 700 metres, must have been more intense than the contemporaneous movement of the Alps, where the Pliocene marine beds do not rise more than 400 metres above the sea. It is highly probable that a similar relation obtained between the Jura and the Swiss Alps. Nor can I doubt that if the Apennines had approached the Alps as closely as the Jura, we should have found the typical direction of the Italian lacustrine basins modified in some cases. Instead of being all perpendicular to the axis of the Alps, some of them would have resembled the Lake of Geneva, the upper reaches of which are normal, while the lower are parallel to the main axis of the mountains.

8. I admit the existence of a relation between the great Alpine basins and the glaciers of Quaternary times; but in my opinion this relation is due neither to excavation by ice nor obstruction by morainic débris, but simply to the conserving action of the glaciers. The troughs, I believe, were occupied by the glaciers, and thus their filling up by the fluvio-glacial detritus of the first half of the Quaternary age was prevented.

In the following table I bring into one view the general conclusions arrived at in the paper :—

Quaternary.	Terracian,*	Retreat of the glaciers. Conversion of the great troughs into lakes. Powerful river erosion, accompanied by the formation of terraces.	Alluvia. Peat.
	Saharian—(2nd Glacial Epoch).	Great development of glaciers: filling of the great troughs by ice. Enormous streams.	Morainic amphitheatres. Drift. Diluvium.

General excessive elevation of the Alps and Apennines, and settlement of the existing Alpine orography. Formation or enlargement of many valleys, and of almost all the lake-basins by faults, folds, elevations, and subsidences.

Tertiary.	Astian—(1st Glacial Epoch.)	Commencement of the general elevation of the Alps and Apennines. Initiation of the great glacial development in the Alps.	Continental fluviatile or lacustrine, or fluvio-glacial deposits. Yellow and grey marine sands.
	Piacentian.	General subsidence of the Alps and Apennines.	Continental fluviatile deposits. Blue marine marls.
	Messinian.	General powerful elevation of the Alps and Apennines. Outlining of the existing Alpine orography.	Continental fluviatile deposits. Marshy deposits.

2. On the Minute Oscillations of a Uniform Flexible Chain hung by one End; and on the Functions arising in the course of the Inquiry. By E. Sang, LL.D. (Plate IX.)

Twenty-seven years ago the writer submitted to the Royal Society of Edinburgh a paper on the minute oscillations of flexible pendulums, in which some new general laws were expounded. He proposes now to consider one case of the phenomenon more in detail.

The investigation of extensive oscillations of a flexible system is far beyond the present powers of the calculus, and we are restricted to the consideration of disturbances not far from the mean position of the system.

In the case of a thread having weights arranged on it at intervals,

* The Terracian embraces the epoch that intervened between the Glacial and the Present, and corresponds to the Post-glacial of other geologists.

the system is capable of as many simple oscillations as there are attached bodies, and all the movements of which it is susceptible are compounds of these simple ones. An imaginary flexible heavy line may be regarded as composed of an infinite number of parts, and thus for it there is an endless series of simple oscillations, each having its own periodic time. The essential feature of our inquiry is as to the manner of one of these.

The character of a simple oscillation may be illustrated thus:—Let HO, fig. 1, represent the direction of the plummet, while the waved line LIHFECBA (supposed, however, to be almost straight) is the form of the chain at some particular instant; then the motions must be such that all the parts of the chain come simultaneously into the position L*i*H*f*E*c*BO.

For this it is requisite that the tendency to redress the position of any element, as P*p*, of the chain must be proportional to the distance PQ and to the mass of the element. Now the tension of the chain at P is (in our restricted case) measured by the weight of the part below,—that is by the length PA, which we hold as equal to QO; and that tension decomposed in the direction QP is proportional to the sine of the inclination at P; so that if we denote OQ by z, QP by x, and use Leibnitz' notation, the tendency of the strain on PA to draw the element outwards is proportional to $z\dfrac{dx}{dz}$. Similarly, the tension of the chain PE, which is proportional to Oq, causes a pull inwardly, indicated by $z'\dfrac{dx'}{dz'}$; and thus the ultimate determination of the element Pp towards the central line is proportional to the difference—

$$z'\frac{dx'}{dz'} - z\frac{dx}{dz} \text{ or to } d\left\{z\frac{dx}{dz}\right\},$$

and thus the general condition of a simple oscillation is expressed by the differential equation of the second order

$$\frac{d\left(z\dfrac{dx}{dz}\right)}{dz} = \frac{dx}{dz} + z\frac{d^2x}{dz^2} = -ax$$

in which a is a coefficient constant all along the chain, but changing from one simple oscillation to another.

The resolution of our physical problem is now converted into the management of a case in the doctrine of functions, and thus it acquires an importance far beyond any that the original question can be supposed to possess. In this equation z stands as the primary variable, x as its function, $\frac{dx}{dz}$ as the first, $\frac{d^2x}{dz^2}$ as the second derivative. Translated into geometry, we have the abscissa z, the ordinate x, the inclination of the curve $\frac{dx}{dz}$, and the curvature $\frac{d^2x}{dz^2}$; or into mechanics, the time t (for z), the position x, the velocity $\frac{dx}{dt}$, and the acceleration $\frac{d^2x}{dt^2}$, all combined in one formula; and the resolution of it may imply that of whole classes of physical problems. It is in this light that the matter is again brought under the notice of the Society.

The problem does not belong to the differential calculus, for in that case we should need to have the relation of the primary z to one of the derivatives explicitly declared; not to the integral calculus, for then the connection between the primary and the derivative would need to be given; nor yet to the third co-ordinate branch, for the relation of the primitive and derivative functions is not prescribed.

For convenience in treating the matter, it is expedient to discard Leibnitz' notation for differentials of higher orders than the first, and altogether to dispense with his notation of integrals.

Such an expression as $\frac{d^5x}{dz^5}$, is intended to represent the result of five successive differentiations in which z is the primary or independent variable and x the function. Here the sign of differentiation is twice, and that of the order also twice, written. Now, the essentials to be indicated are, the idea of differentiation, the primary, the function, and the order. The idea may conveniently be indicated by the position of the marks; Lagrange placed these as accents over the function, thus:— x^{i}, x^{ii}, x^{iii}, x^{iv}, x^{v}. This scheme has two drawbacks; the position of the accents had long been appropriated to the indices of powers, and there is no notice of the primary; Leibnitz' notation clearly shows the distinction between

$\frac{d^5x}{dz^5}$ and $\frac{d^5x}{dy^5}$, whereas the mark x^{v} can show none. The writer, in his "Solution of Equations of all Orders," Edinburgh, 1829, has placed Lagrange's accents as ante-subponents, and has written along with them the primary, so that the symbol ${}_{5z}x$ is used to denote the fifth derivative of x regarded as a function of z, while ${}_{5y}x$ means the corresponding differential coefficient when y is the independent variable. In this way all the essentials are exhibited without redundancy.

The symbols x, ${}_{1z}x$, ${}_{2z}x$, ${}_{3z}x$, thus indicate a series of functions deduced by the repeated operation called differentiation; each one is, as Lagrange says, the derivative of the preceding, and each one is the primitive (integral) of the succeeding. So we may carry the notation backwards by using the sign of reversion and write ${}_{-1z}x$ for the function of which x is the derivative,—that is the $\int xdz$ of Leibnitz, and thus get the progression extending both ways

$$\&c.,\ {}_{-3z}x\,,\ {}_{-2z}x\,,\ {}_{-1z}x\,,\ x\,,\ {}_{1z}x\,,\ {}_{2z}x\,,\ {}_{3z}x\,,\ \&c.$$

Using this notation, the condition of a simple oscillation of the chain is expressed by

$$-ax = {}_{1z}x + z\,.\,{}_{2z}x\,.$$

From this equation we have the second derivative ${}_{2z}x$ in terms of x, of the first derivative ${}_{1z}x$, and of the primary z; and from it also we easily obtain the subsequent derivatives, for on differentiating we find

$$\begin{aligned} -a\,.\,{}_{1z}x &= 2\,.\,{}_{2z}x + z\,.\,{}_{3z}x\,, \\ -a\,.\,{}_{2z}x &= 3\,.\,{}_{3z}x + z\,.\,{}_{4z}x\,, \\ -a\,.\,{}_{3z}x &= 4\,.\,{}_{4z}x + z\,.\,{}_{5z}x\,, \end{aligned}$$

and in general $-a\,.\,{}_{nz}x = (n+1)\,{}_{\overline{n+1}z}x + z\,.\,{}_{(n+2)z}x$; we may also proceed backwards by integration, thus:—

$$\begin{aligned} -a\,.\,{}_{-1z}x &= 0\,.\,x + z\,.\,{}_{1z}x \\ -a\,.\,{}_{-2z}x &= -1\,.\,{}_{-1z}x + z\,.\,x \\ -a\,.\,{}_{-3z}x &= -2\,.\,{}_{-2z}x + z\,.\,{}_{-1z}x \end{aligned}$$

and in general

$$-a\,.\,{}_{-nz}x = -(n-1)\,.\,{}_{-(n-1)z}x + z\,.\,{}_{-(n-2)z}x\,.$$

On writing ϕx to represent some one of these functions, the equation relating to it will take the general form

$$-a\,.\,\phi z = n\,.\,{}_{1z}\phi z + z\,.\,{}_{2z}\phi z$$

in which n may be any integer number, positive or negative; and thus the solution of our problem will virtually contain that of a whole series of allied ones. The particular case, when $n=0$, merits notice, it becomes

$$-a\,.\,\phi z = z\,.\,{}_{2z}\phi z \quad \text{or} \quad {}_{2t}\phi z = -\frac{a}{t} - \phi z\,,$$

and therefore represents the movements of a body actuated by a spring whose stiffness $\frac{a}{t}$ becomes lessened in inverse proportion to the elapsed time.

The coefficient a in these formulæ regulates the scale on which the abscissæ z are measured, and if it be taken as unit, the periodic time of the chain's oscillation will be that of a simple pendulum having the linear unit for its length; so the generality of the results will not be impaired by the assumption $a=1$. Let us then seek to determine the relation of x to the primary z from the equation $-x = {}_{1z}x + z\,.\,{}_{2z}x$.

Naturally we try whether it be possible to represent x by a series of terms involving the powers of the variable z. We shall suppose, then,

$$x = \mathrm{A} + \mathrm{B}z + \mathrm{C}z^2 + \mathrm{D}z^3 + \ldots \mathrm{M}z^{n-1} + \mathrm{N}z^n + \text{ \&c.},$$

which gives, on being differentiated,

$$\begin{aligned} {}_{1z}x &= \mathrm{B} + 2\mathrm{C}z + 3\mathrm{D}z^2 + 4\mathrm{E}z^3 + \ldots\ldots\ldots n\mathrm{N}z^{n-1} +, \text{ \&c.},\\ z.{}_{2z}x &= \quad 1.2\mathrm{C}z + 2.3\mathrm{D}z^2 + 3.4\mathrm{E}z^3 + \ldots (n-1)\,n\mathrm{N}z^{n-1} +, \text{ \&c.}, \end{aligned}$$

wherefore, equating the terms containing the like powers of z,

$$-\mathrm{A} = \mathrm{B}, \quad -\mathrm{B} = 4\mathrm{C}, \quad -\mathrm{C} = 9\mathrm{D}, \quad -\mathrm{D} = 16\mathrm{E}, \text{ and}$$

in general $-\mathrm{M} = n^2\mathrm{N}$, so that

$$\mathrm{B} = -\frac{\mathrm{A}}{1^2}, \quad \mathrm{C} = +\frac{\mathrm{A}}{1^2.2^2}, \quad \mathrm{D} = -\frac{\mathrm{A}}{1^2.2^2.3^2} \text{ and so on.}$$

Whence

$$x = \mathrm{A}\left\{1 - \frac{z}{1^2} + \frac{z^2}{(1.2)^2} - \frac{z^3}{(1.2.3)^2} + \frac{z^4}{(1.2.3.4)^2} - \text{ \&c.}\right\}$$

where the multiplier A depends on the extent of the oscillation and on the particular instant of time. For the present we may assume A also to be unit and confine our attention to the equation

$$x = 1 - \frac{z}{1^2} + \frac{z^2}{\ldots 2^2} - \frac{z^3}{\ldots . 3^2} \pm \cdots\cdots \frac{z^n}{\ldots\ldots n^2} +, \text{ \&c.}$$

Each succeeding term of this progression is formed from its antecedent by means of a factor of the form $\frac{z}{2^2}, \frac{z}{3^2}, \dots \frac{z}{n^2}$, so that, however large z may be taken, the denominator n^2 must eventually come to exceed it; and thus, although the terms may increase at the beginning, they must ultimately come to decrease; and therefore the computation of x to within any prescribed degree of precision is always possible.

The curve lies on the one or on the other side of the plumb-line, according as the sum of the even terms of the progression exceeds or falls short of the sum of the odd terms, and we can discover which way only by the actual calculation. The intersections of the curve with the middle line represent the points of suspension of the oscillating chain, and therefore our attention is first called to the discovery of those values of z which correspond to $x = 0$. From the mere aspect of the progression we could not even predict that any such values are possible, or form any idea of the order of the roots of this transcendental equation. The consideration of the physical problem with which it is connected does indeed throw light on the matter, and leads us to anticipate an endless succession of roots more and more separated as we proceed upwards.

The accurate determination of these roots can only be reached by trial; the computations are very operose, and we look for some means for lessening the labour; this is found in the law of succession of the derivatives. Let us suppose that, corresponding to some value of z, the ordinate x and its derivative ${}_{1z}x$ have been computed, we are then able easily, particularly if z be represented by an integer number, to deduce the subsequent derivatives. Thus—

$${}_{2z}x = \frac{-x - 1 \,.\, {}_{1z}x}{z}$$

$${}_{3z}x = \frac{-{}_{1z}x - 2 \,.\, {}_{2z}x}{z}$$

$${}_{4z}x = \frac{-{}_{2z}x - 3 \,.\, {}_{3z}x}{z}, \text{ and so on;}$$

and these enable us to deduce the values of x corresponding to proximate values of z by the process described in the work above referred to. When z is large these values evidently decrease at the beginning, but as the order advances the multiplier of the last found

derivative must come to exceed z, and it may be that the progression eventually becomes divergent; into this matter we shall inquire hereafter.

The first derivative $\frac{dx}{dz}$ is expressed by the series

$$_{1z}x = -\frac{1}{1} + \frac{z}{1^2.2} - \frac{z^2}{1^2.2^2.3} + \frac{z^3}{1^2.2^2.3^2.4} - , \text{ \&c.}$$

whose terms stand between those of the preceding in such a manner that, with scarcely augmented labour, one computation may be made to give both. Thus if we take a term of the series for x, say $\frac{z^2}{1^2.2^2}$ and divide it by the next exponent 3, we get $\frac{z^2}{1^2.2^2.3}$ a term of the series for $_{1z}x$; and this again multiplied by $\frac{z}{3}$ gives $\frac{z^3}{1^2.2^2.3^2}$, the succeeding term of the series for x.

To make a beginning, let us compute the values for $z=1$; these are

$$x = +.22389, \quad _{1z}x = -.57672.$$

On examining the relations of these numbers to unit and to each other by the method of continued fractions, we get a remarkable result for the ratio of the ordinate to its derivative. Proceeding in the usual way of taking the greater from the less, the remainder from the preceding, and so on, we get the quotients

$$2, 1; 1, 2; 1, 3; 1, 4; 1, 5; \text{\&c.,}$$

whence the successive approximations alternately in defect and in excess.

$$\begin{array}{cccccccccc} & 2 & 1 & 1 & 2 & 1 & 3 & 1 & & \\ \frac{0}{1} & \frac{1}{0} & \frac{2}{1} & \frac{3}{1} & \frac{5}{2} & \frac{13}{5} & \frac{18}{7} & \frac{67}{26} & \frac{85}{33} & \text{\&c.} \end{array}$$

and if we follow the method of excesses, the quotients come out 1, 2, 3, 4, 5, &c., giving the chain of fractions

$$\begin{array}{cccccccccc} & 1 & 2 & 3 & 4 & 5 & 6 & & \\ \frac{-1}{0} & \frac{0}{1} & \frac{1}{1} & \frac{2}{1} & \frac{5}{2} & \frac{18}{7} & \frac{85}{33} & \frac{492}{191} & \text{\&c.,} \end{array}$$

which lie all on one side of the absolute ratio, being indeed the alternates of the preceding.

Now, in forming a list of the successive derivatives of x for $z=1$, according to the law above explained, and writing for clearness' sake, A for ·22389, B for ·57672, we get the progression

$$\begin{array}{rcrr}
x & = & +\ \ \ \ A & \\
{}_{1z}x & = & & -\ \ \ \ B. \\
{}_{2z}x & = & -\ \ \ \ A & +\ \ \ \ B. \\
{}_{3z}x & = & +\ \ 2\ A & -\ \ \ \ B. \\
{}_{4z}x & = & -\ \ 5\ A & +\ \ 2\ B. \\
{}_{5z}x & = & +\ 18\ A & -\ \ 7\ B. \\
{}_{6z}x & = & -\ 85\ A & +\ 33\ B. \\
{}_{7z}x & = & +492\ A & -191\ B. \\
& & \&c., & \&c.
\end{array}$$

But these coefficients of A and B are developed exactly as are the members of the approximating fractions, so that since A : B is nearly as 191 : 492, the difference 492 A – 191 B must be small, and must continue to decrease as we proceed farther. Hence, if we can show that the above progression of quotients 1, 2, 3, 4, &c., necessarily holds good, we shall have demonstrated that the progression of derivatives never becomes divergent.

If we treat the progressions $A=x$, $B=-{}_{1z}x$ by the method for continued fractions, taking the excesses, and dividing each excess by z, we get at once the following results:—

$$A \qquad = 1 - \frac{z}{1^2} + \frac{z^2}{1^2.2^2} - \frac{z^3}{1^2.2^2.3^2} +$$

$$B \qquad = 1 - \frac{z}{1^2.2} + \frac{z^2}{1^2.2^2.3} - \frac{z^3}{\ldots 3^2.4} +$$

$$-A+\ B=zC; \qquad C=\frac{1}{1.2} - \frac{z}{1^2.2.3} + \frac{z^2}{..\,2^2.3.4} - \frac{z^3}{..\,3^2.4.5} +$$

$$-B+2\,C=zD;\ D=\frac{1}{1.2.3} - \frac{z}{1^2.2.3.4} + \frac{zz^2}{..\,2^2.3.4.5} - \&c.,$$

$$-C+3\,D=zE;\ E=\frac{1}{1.2.3.4} - \frac{z}{1^2.2.3.4.5} + \frac{z^2}{..\,2^2.3.4.5.6} - \&c.,$$

and so on; wherefore, in general, the formation of the fractions

approximating to the ratio of A to B, is identical with that of the coefficients of A and B in the expressions for the successive derivatives; in fact, the excesses above found are the very derivatives themselves, with the alternate signs changed; and thus it appears that in no case can the progression of derivatives become divergent.

Proceeding now one step forward, the computations for $z=2$ are found to give $x=-\cdot 19655$; ${}_{1z}x=-\cdot 28928$, so that the curve must cross the axis between $z=1$ and $z=2$. The exact place of crossing may conveniently be reached from either side; it is at $z_1=1\cdot 44580$. Thus it appears that a chain having the length 1·44580 will perform a simple oscillation in the same time as will a pendulum whose length is 1·00000. Or, conversely, that a chain of the length unit will perform its slowest simple oscillation along with a pendulum having ·69166 for its length. These proportions are shown in the figure, OB being the length of the chain, OQ that of the pendulum oscillating along with it.

Proceeding onwards in search of the second crossing, we find it to lie between $z=7$ and $z=8$, from either of which an easy approximation gives us $z_2=7\cdot 61782$, rather more than five times the preceding; this is the OE of the figure, the curved line EDCBA representing in a most exaggerated way the character of the oscillation. The second simple oscillation of the chain is thus isochronous with that of a pendulum ·13127 long, the length of the chain being unit.

In continuing the search for the remote crossings, the labour of the trial calculations increases greatly, and we seek to lessen the toil by watching the progress of the distances; and, to our considerable relief, find that the second differences are almost, though not quite, constant, as is seen in the subjoined table for six crossings.

$z_1=$ 1·44579 64903		
	6·17201 90958	
$z_2=$ 7·61781 55861		4·93191 70158
	11·10393 61116	
$z_3=$ 18·72175 16977		4·93438 33025
	16·03831 94141	
$z_4=$ 34·76007 11118		4·93468 53785
	20·97300 47926	
$z_5=$ 55·73307 59044		4·93475 75309
	25·90776 23235	
$z_6=$ 81·64083 82279		

Hence, after having computed the fourth crossing it was easy for us to see that the fifth must be between 55 and 56; and now that the sixth crossing has been accurately determined we readily infer that the seventh must be at 112·48, the eighth at 148·26, and the ninth at 188·97 nearly.

It is also worthy of remark that the second difference approaches closely to the value of $\frac{1}{2}\pi^2$, namely to 4·93480, and we are tempted to conclude that this well-known number is the asymptote to which the second difference tends. The mere arithmetical coincidence is a weak argument in favour of this notion; yet it is all that the algebraic formula seems capable of supplying: we shall find a much stronger argument in the character of the physical phenomenon under review.

Lemma.

The vibration of the portion LH, comprised between two crossings, is that of a musical string fixed at L and H, and stretched as by a weight HO at its lower end; and the preceding investigation takes into account the change of strain due to the weight of the cord. In the case of the musical string the tension is many times greater than the weight of the cord, which weight, therefore, may be neglected even when the string is upright.

Using as the linear unit the length of a pendulum oscillating in the same time as the string, and as the unit of tension the weight of one unit's length of the string, and writing w for the tension so measured, the differential equation of the curve is

$$-x = w \cdot {}_{2z}x$$

of which the solution in its most general form is

$$x = p \,.\, sin\frac{z}{\sqrt{w}} + q \,.\, cos\frac{z}{\sqrt{w}}$$

where p and q are coefficients depending on the initial motion and on the elapsed time. In our present example q is zero, and the equation of the curve at some particular instant becomes

$$x = p \,.\, sin\frac{z}{\sqrt{w}},$$

which applies strictly to the case when the two ends are on one level.

From this we see that x is zero when the arc represented by $\frac{z}{\sqrt{w}}$ is zero, or is any multiple of the half circumference π; that is, when $z=0$, $z=\pi\sqrt{w}$, $z=2\pi\sqrt{w}$, &c., so that the curve must cross its axis at points separated by the uniform distance $\pi\sqrt{w}$.

If now we imagine a second string having its tension greater than that of the former by the weight of this length, or altogether $w+\pi\sqrt{w}$; the distance between its cusps, so as to keep the same time of oscillation, must be

$$\pi\sqrt{(w+\pi\sqrt{w})},$$

and the increase, analogous to our second difference, becomes

$$\begin{aligned}\pi\{\sqrt{(w+\pi\sqrt{w})}-\sqrt{w}\} &= \pi\frac{\pi\sqrt{w}}{\sqrt{w}+\sqrt{(w+\pi\sqrt{w})}}\\ &= \pi^2\frac{1}{1+\sqrt{\left(1+\frac{\pi}{\sqrt{w}}\right)}}.\end{aligned}$$

Now when w increases, the fraction $\frac{\pi}{\sqrt{w}}$ decreases, and the denominator of this fraction becomes more and more nearly equal to 2, so this analogue of our second difference approaches to $\frac{1}{2}\pi^2$.

Having thus determined the points of crossing, we proceed to consider the extreme distances to which the curve reaches on either side, as at the points c, F, I of the figure. Thereat the curve is parallel to the axis and the derivative ${}_{1z}x$ is zero; we have no other way of discovering these points than by the solution of the transcendental equation

$$0=-1+\frac{z}{1}-\frac{z^2}{1^2.2}+\frac{z^3}{1^2.2^2.3}-\text{ \&c.}$$

which we manage exactly as before—that is by calculations arranged according to the fundamental law known as Taylor's theorem. The results, with the corresponding values of x, are

z			x
3·67049			− ·40276
	8·63412		
12·30461		4·93613	+ ·30012
	13·57025		
25·87486		4·93508	− ·24700
	18·50533		
44·38019		4·93489	+ ·21836
	23·44022		
67·82041			− ·19647

Here the second differences of z are seen to be in excess of $\frac{1}{2}\pi^2$, and to tend towards it. These maximum points are below the middles of their respective arcs by the distances

·86131
·86517
·86605
·86638
·86654

which evidently approximate to some definite limit. The exact determination of this asymptote would be a matter of great difficulty.

In passing from side to side of its axis the chain must change the direction of its curvature, the concavity being in general toward the axis; but the points of reflexure are not necessarily at the crossings. At these points the second derivative must be zero; now the very genesis of the curve is contained in the equation

$$-x = {}_{1z}x + z\,{}_{2z}x \quad \text{or} \quad {}_{2z}x = \frac{-x - {}_{1z}x}{z},$$

wherefore for the points of reflexure D, G, K, the ordinate x and its derivative ${}_{1z}x$ must be equal to each other with opposite signs. The subtangent of the curve is given by the formula $-\frac{x}{{}_{1z}x}$ or $-x\frac{dx}{dz}$ in Leibnitz' notation; wherefore the tangents applied at these points must meet the axis at the distance of unit (that is the length of the corresponding pendulum) above the points d, g, h, k of the figure, as also is the case for the tangent applied at the lowest point A.

Thus it seems that the portions AB, DE, GH, KL are convex toward the axis.

The values of z corresponding to these points of reflexure are the roots of the transcendental equation ${}_{2z}x=0$, and are obtained in the manner already described; they, along with the corresponding values of x, are

z	x
6·59365	− ·13228
17·71250	+ ·06448
33·75518	− ·04001
54·73005	+ ·02792
80·63878	− ·02090

while the distances of the points of reflexure below the respective crossings are

1·02416
1·00925
1·00489
1·00303
1·00206

The details connected with these singular points, namely, the crossings, the maxima, and the reflexures are contained in the subjoined table:—

Singular Points in the Curve.

z	x	${}_{1z}x$	${}_{2z}x$
0·00000 00000	+ 1·00000 00000	− 1·00000 00000	+ ·50000 00000
1·44579 64903	·00000 00000	− ·43175 48070	+ ·29862 83407
3·67049 26605	− ·40275 93957	·00000 00000	+ ·10972 89745
6·59365 41007	− ·13227 94874	+ ·13227 94874	·00000 00000
7·61781 55861	·00000 00000	+ ·12328 26057	− ·01618 34589
12·30461 40804	+ ·30011 57525	·00000 00000	− ·02439 05051
17·71249 97297	+ ·06448 25277	− ·06448 25277	·00000 00000
18·72175 16977	·00000 00000	− ·06273 64998	+ ·00335 09952
25·87486 34727	− ·24700 48771	·00000 00000	+ ·00965 04810
33·75517 72165	− ·04000 79701	+ ·04000 79701	·00000 00000
34·76007 11118	·00000 00000	+ ·03942 82580	− ·00113 42974
44·38019 17035	+ ·21835 94072	·00000 00000	− ·00492 01997
54·73004 72864	+ ·02791 85486	− ·02791 85486	·00000 00000
55·73307 59044	·00000 00000	− ·02767 63754	+ ·00049 65880
67·82041 35683	− ·19646 53715	·00000 00000	+ ·00289 68471
80·63877 90738	− ·02090 51560	+ ·02090 51561	·00000 00000
81·64083 82279	·00000 00000	+ ·02077 67294	− ·00025 44894

We have seen that the area of the curve represented by $\int xdz$ or by $-{}_{1z}x$, is the product of the abscissa z by ${}_{1z}x$, the derivative of the ordinate; hence the area of the portion AOB is

$$1{\cdot}44580 \times {\cdot}43175 = {\cdot}62423\,.$$

But the derivative at C is zero, wherefore the area BcC on the subtractive side must balance AOB on the additive side; its value must also be ·62423. The derivative again becomes zero at F, and consequently the areas CcE, EfF balance each other, each of them being given by the product of the abscissa OE into the derivative at E, —that is by $7{\cdot}61782 \times {\cdot}12328$ or ·93914.

The same law continues all along, the areas increasing, but more and more slowly as we proceed upwards, as is seen from the subjoined list—

·62423		
	·31491	
·93914		– ·07951
	·23540	
1·17454		– ·03941
	·19599	
1·37053		– ·02403
	·17196	
1·54249		– ·01822
	·15374	
1·69623		

—from which, however, we can form no idea as to whether the increase be or be not confined to within some definite limit.

Hitherto we have been considering the form of an indefinitely long chain, whose oscillations are performed in a fixed time, namely, that of a pendulum whose length is unit. We shall now proceed to investigate the forms and times of oscillation of a chain having a determinate length.

The simple oscillations of any chain, PO, are easily got from the preceding investigations: thus the slowest oscillation, that in which the whole chain swings from one side of its mean position to the other side, is represented by the part BO of the first figure, the ordinates of the curve being, in imagination, reduced so as to be scarcely perceptible. If L be the total length of the chain, $\frac{L}{1{\cdot}4458} = L \times {\cdot}69166$ is the length of the pendulum oscillating synchronously with it, and its oscillations are more frequent than that of a

pendulum having the whole length L in the ratio of $\sqrt{1{\cdot}44580}:1$, that is, as $1{\cdot}20241:1$. The chain makes almost exactly 101 oscillations, while the simple pendulum makes 44.

The second oscillation, that in which the chain has one node, is represented by the part EBO of the first figure; and on dividing PO of the second figure in the ratio of EB to BO, we get B′, the node of the actual chain; on making OQ′ also in proportion, we have the length of the pendulum swinging in the same time as the chain. This length is $\frac{L}{7{\cdot}61782}$ or $L \times {\cdot}13127$; and consequently these second oscillations are more frequent than those of a pendulum whose length is L in the ratio of $\sqrt{7{\cdot}61782}$ to 1, that is, of 2·76004 to 1.

When two oscillations, represented by the (A) and the (B) of figure 2, are coexistent, the character of the compound motion results from the ratio of their periodic times, that is, of 1·20241 to 2·76004. On examining this ratio by the method of continued fractions, we find the successive quotients, 2, 3, 2, 1, 1, 2, 13, 2, 7, &c., which give the approximating fractions—

$$\frac{1}{2},\ \frac{3}{7},\ \frac{7}{16},\ \frac{10}{23},\ \frac{17}{39},\ \frac{44}{101},\ \&c.$$

Taking the second of these for the sake of illustration, while (A) has made three complete oscillations, (B) has made seven, and the chain is (nearly) in the same position as at first, so that the same phases would be repeated. But the periodic times are incommensurate, and so the same phase can never be accurately reproduced.

The two sets of oscillations may or may not be in one plane; when they are in planes inclined to each other, the path of a point in the chain is analogous to the curve produced by the vibration of a straight wire whose periodic times are in the same ratio; only in the present case the figure is not necessarily circumscribed by a rectangle.

In order to form some idea of the compound movements, let us draw AB, figure 3, to represent the extent of the oscillation (A), and BC, inclined to it, to show that of the simple oscillation (B). Then, having described a semicircle on each of these, we divide the

one into some multiple of 7, the other into the corresponding multiple of 3 equal parts (actually into 21 and 9). Perpendiculars drawn from the points of section divide the diameters into graduated parts, representing the distances passed over by the end of the chain in equal portions of time during each of the separate simple oscillations. Having completed the rhomboid ABCD, and divided it into a multitude of small rhomboids by parallels drawn through the divisions of its sides, we begin at the corner of any one of these, draw a line to the opposite corner, thence into the next, and so on, passing from side to side of the entire rhomboid, until we return to the first point. In this way we get an approximate representation of the path of the lower end of the chain when a plane oscillation (B) is imposed on a plane oscillation (A).

But the chain may perform two simple oscillations (A) in different planes, the result being an elliptic movement; and so also of the oscillation (B); and then the compound of the two (or rather four) must be got by carrying the centre of the one ellipse along the circumference of the other, in the manner used for the epicycloid.

These curves present an endless diversity of form, according to the dimensions and relative positions of the ellipses. Adopting the ratio 7 : 3 for that of the periodic times, some of these are depicted in figure 4, *a*, *b*, *c*, *d*, *e*. In *a* and *b* the ellipses have been placed conformably and the curves are symmetric; for *a* the motions were made both in one direction, and, as in the analogous case of the epicycloid, there are *four*, that is $7-3$, lobes; for *b* one of the motions has been reversed, and we find *ten*, that is $7+3$, lobes; *c* and *d* are corresponding examples with the axes of the ellipses set obliquely; while for *e* the ellipse (B) is compressed into a straight line. These examples may give some faint idea of diversity of character among the curves.

While the lower end of the chain is describing some one of these curves, the points higher up are performing each its own peculiar evolution. As we ascend, the dimensions of the ellipse (B) decrease more rapidly than do those of (A), and consequently, along with its extent, the curve changes also its configuration; and when we arrive at the height of the node B′, the quicker ellipse has collapsed into a point, and the chain there describes simply the ellipse due to the oscillation (A). Above this height the ellipse (B) reappears,

Fig 1.

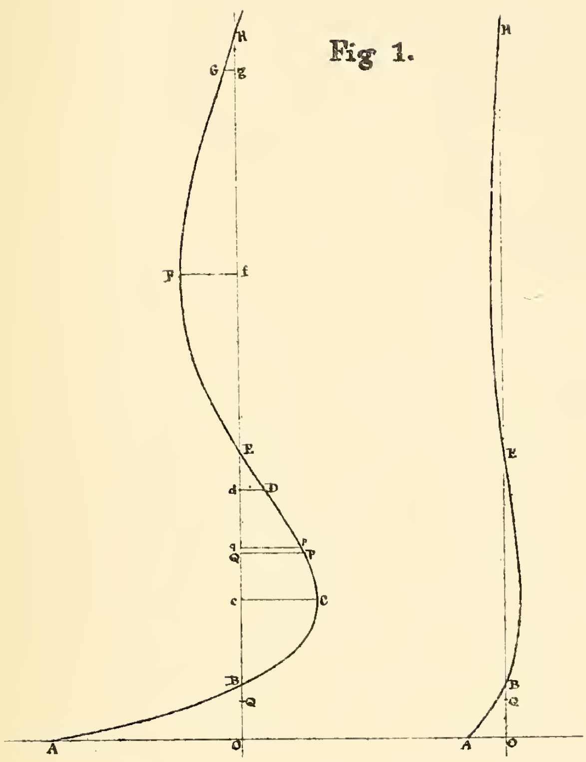


Fig 2.

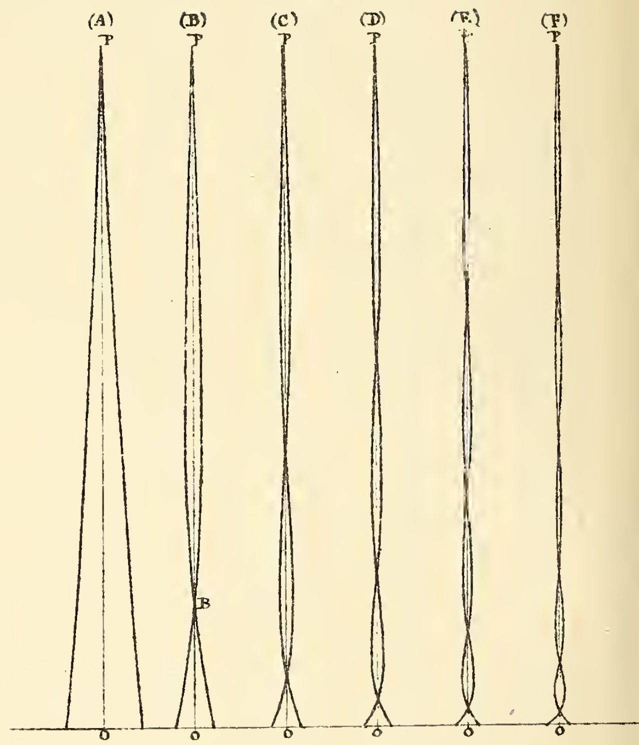


Fig 4 *b*.

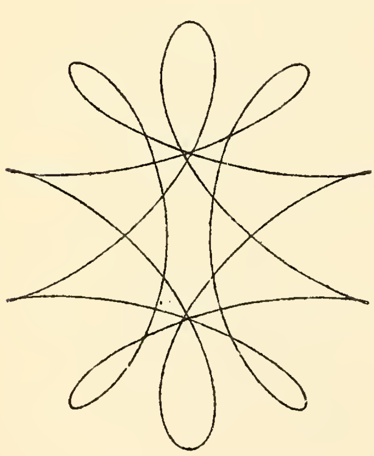


Fig 4 *c*.

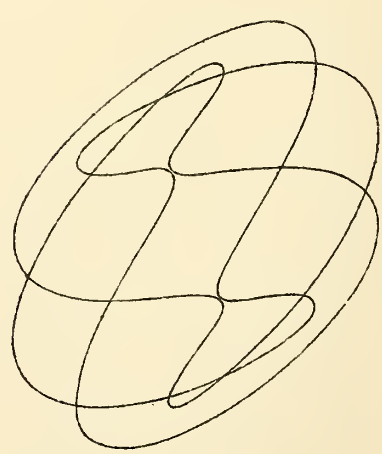


Fig 3.

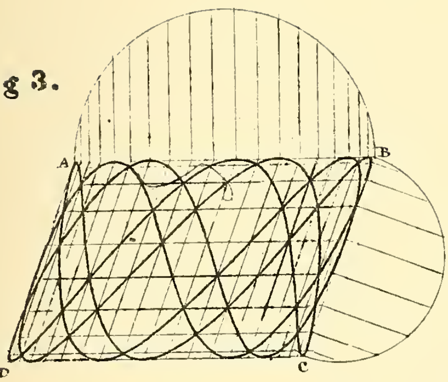


Fig 4 *a*.

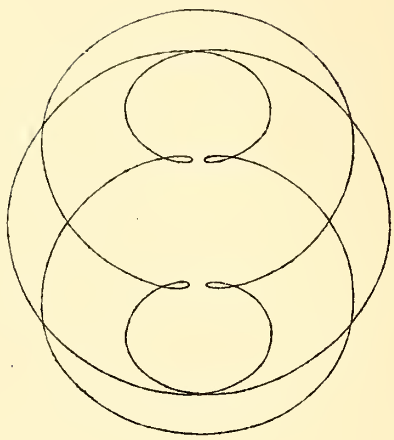


Fig 4 *d*.

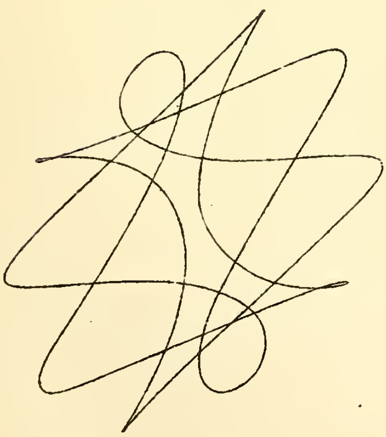
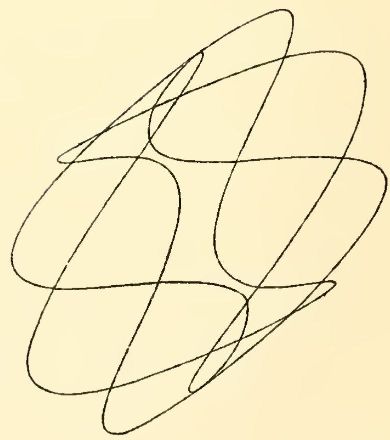


Fig 4 *e*.



with its radii opposed to their former directions, and thus we have a new series of configurations; at the same time, owing to the incommensurability of the motions, the whole of these configurations undergo a gradual change.

When the chain divides in three, its simple oscillations take the form shown in (C) of figure 4; while (D) and (E) show the forms when the chain is divided into *four* and *five* oscillating parts. The respective lengths of the corresponding pendulum, the periodic times, and the frequencies of oscillation are given in the following table, in which the whole length of the chain is taken as the linear unit, and the periodic time of a pendulum of that length as the unit of time.

No.	Pendulum.	Periodic Time.	Frequency.	
1	·691 6603	·831 6512	1·202 4128	
				1·557 6263
2	·131 2712	·362 3137	2·760 0391	
				1·566 8249
3	·053 4138	·231 1143	4·326 8640	
				1·568 9032
4	·028 7686	·169 6132	5·895 7672	
				1·569 6917
5	·017 9427	·133 9102	7·465 4589	
				1·570 0731
6	·012 2488	·110 6742	9·035 5320	

Here the almost uniform increase of the frequencies is remarkable, the difference approximates to $\frac{1}{2}\pi$, and this is in accordance with what has been said of the successive roots of the equation $x=0$. On comparing the frequencies themselves with π, we find

$$\begin{aligned}
&\text{for } 1, \ \tfrac{1}{8}\pi \times 3{\cdot}0619\,;\\
&\text{for } 2, \ \tfrac{1}{8}\pi \times 7{\cdot}0284\,;\\
&\text{for } 3, \ \tfrac{1}{8}\pi \times 11{\cdot}0183\,;\\
&\text{for } 4, \ \tfrac{1}{8}\pi \times 15{\cdot}0136\,;\\
&\text{for } 5, \ \tfrac{1}{8}\pi \times 19{\cdot}0106\,;\\
&\text{for } 6, \ \tfrac{1}{8}\pi \times 23{\cdot}0088\,;
\end{aligned}$$

and we are tempted to conclude that, when the chain vibrates in a great number (N) of parts, the frequency of its oscillation may be denoted by $\frac{1}{8}\pi \times (4\text{N} - 1)$.

The closeness of these results to a simple arithmetical progression

is a warning to us against too hasty conclusions from experimental data. Let us suppose for a moment that the periodic oscillations of a chain had been of great importance; while as yet we had only observation to guide us, means had been found for counting the numbers per minute, and it had been found that, within all the attainable accuracy these are as the alternate odd numbers, 3, 7, 11, 15, 19,—this with the greater precision the farther we prosecute our trials. Here then we have discovered a periodic law! Nature has a partiality for small numbers,—vibrating bodies divide in harmonic ratio, substances combine in easy proportions. So, in seeking for the law of light's refraction, the constancy of the ratio of the sines was accepted as absolute, while, as yet, the inaccuracy of the observations was so great as to cover the whole range of dispersion. Nay, while we firmly hold to the simple laws of pressure and motion as revealed to us by our experiments, may it not be that these only exhibit close coincidences, resulting from more deeply-seated principles? We know of the law of velocity only from experiment, but the velocities observed by us are only relative variations, mere *zerrë's* in comparison with the speed with which we and our apparatus are hurried along.

The function which has been under review, and its derivatives and primitives, are all deducible from the generic property

$$-a.\phi z = n.{}_{1z}\phi z + z.{}_{2z}\phi z$$

in which, however, n is restricted to be an integer positive number. It may be interesting to inquire into the phases when this restriction is removed.

On making the same supposition as before, namely,

$$\phi z = \mathrm{A} + \mathrm{B}z + \mathrm{C}z^2 + \mathrm{D}z^3 +, \&c.,$$

we find

$$-a\mathrm{A} = 1.n\mathrm{B}$$
$$-a\mathrm{B} = 2.(n+1)\mathrm{C}$$
$$-a\mathrm{C} = 3.(n+2)\mathrm{D}, \&c.,$$

whence

$$\phi z = \mathrm{A}\left\{1 - \frac{a}{n}\frac{z}{1} + \frac{a^2}{n(n+1)}\frac{z^2}{1.2} - \frac{a^3}{n(n+1)(n+2)}\frac{z^3}{1.2.3} +, \&c.\right\}$$

which formula is applicable to all cases except those in which n is a negative integer, for then the denominator of a^n would come to be zero.

Let us first consider the case when n is $\frac{1}{2}$. We get

$$\phi z = \mathrm{A}\left\{1 - \frac{2a}{1}\frac{z}{1} + \frac{4a^2}{1.3}\frac{z^2}{1.2} - \frac{8a^3}{1.3.5}\frac{z^3}{1.2.3} + \frac{16a^4}{1.3.5.7}\frac{z^4}{1.2.3.4} +, \text{ \&c.}\right\}.$$

Since the powers of a accompany those of z, its value serves merely to fix the scale on which the z's are to be measured, and does not at all affect the generality of the formula. Let us then assume the generic condition

$$-\tfrac{1}{4}\phi z = \tfrac{1}{2} \cdot {}_{1z}\phi z + z \cdot {}_{2z}\phi z,$$

and we at once get

$$\phi z = \mathrm{A}\left\{1 - \frac{z}{1.2} + \frac{z^2}{1.2.3.4} - \frac{z^3}{1.....6} + \frac{z^4}{1......8} -, \text{ \&c.}\right\}.$$

If, having only this formula to guide us, we inquire as to the shape of the curve represented by it, our course is to compute the values of the ordinate corresponding to various values of the abscissa. The series converges much more rapidly than that for the oscillating chain, and the points of crossing are much more remote; thus the first would be found at $z = 2{\cdot}46740$, the second at $z = 22{\cdot}20661$, and the third at $z = 61{\cdot}68503$. These numbers are exactly in the ratios of 1, 9, and 25, and the first of them is just $\frac{1}{4}\pi^2$. Moreover, in the course of this arithmetical quest, we should find that the ordinate varies within the limits $+1$ and -1, unlike the preceding, where the limits decrease.

In the present instance, however, we readily perceive that on writing v^2 for z, our series becomes

$$1 - \frac{v^2}{1.2} + \frac{v^4}{1..4} - \frac{v^6}{1...6} + \text{ \&c.}$$

the well-known representative of $\cos v$; and that thus our function may be written

$$\phi z = \mathrm{A} \,.\, \cos \sqrt{z}\,.$$

But this formula defines only one part of the curve, namely, that on one side of the origin of co-ordinates; for the other part we must have recourse to catenarian functions, and write

$$\phi z = \mathrm{A} \,.\, rod \sqrt{-z}.$$

This convenience, however, is a rare one; for the case when $n = -\frac{1}{2}$, that is when the generating condition is

$$-\tfrac{1}{4}\theta z = -\tfrac{1}{2} \cdot {}_{1z}\theta z + z \cdot {}_{2z}\theta z$$

the expression becomes

$$\theta z = \mathrm{A}\left\{ 1 + \frac{1z}{2} - \frac{3z^2}{1.2.3.4} + \frac{5z^3}{1....6} - \frac{7z^4}{1.....8} + \&c. \right\}$$

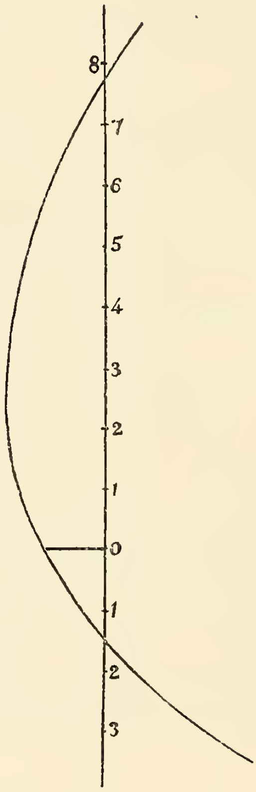
for which no such facility presents itself.

For the purposes of calculation this may be written

$$1 + \frac{z}{2} - (\;)\frac{z}{1.4} + (\;)\frac{z}{3.6} - (\;)\frac{z}{5.8} + \&c.$$

where the mark ( ) stands for the preceding term.

z	θz
+8	− ·080 0038
+7	+ ·379 2051
+6	+ ·773 2548
+5	+1·141 9518
+4	+1·402 4478
+3	+1·549 0238
+2	+1·552 8556
+1	+1·381 7733
0	+1·000 0000
−1	+ ·367 8795
−2	− ·558 4152
−3	−1·827 1822



The first or lowest intersection is between $z = -2$ and $z = -1$, the second between $z = +7$ and $z = +8$; but the position of the third has not been examined.

When n is a rational fraction such as $\frac{r}{s}$, if we make $a = \frac{1}{s^2}$, the function takes the form

$$\phi z = 1 - \frac{1}{r}\frac{z}{s} + \frac{1}{r}\frac{1}{r+s}\frac{z^2}{1s.2s} - \frac{1}{r}\frac{1}{r+s}\frac{1}{r+2s}\frac{z^2}{1s.2s.3s} + \&c.$$

$$= 1 - (\;)\frac{1}{r}\frac{z}{s} + (\;)\frac{1}{r+s}\frac{z}{2s} - (\;)\frac{1}{r+2s}\frac{z}{3s} + \&c.$$

in which the denominators are the continued products of the terms of two arithmetical progressions whose common difference is s.

The cases of n being zero, or being a negative integer, demand special consideration. When n is zero the above general formula would give $\phi z = \text{A}\ (1-\infty)$; but this infinity does not exist in the nature of the problem—it has been introduced by our mode of procedure. The original condition $-a\phi z = z._{2z}\phi z$ may belong to a variety of physical problems, as to this one $_{2t}x = a\dfrac{x}{t}$, in which the incitement to motion is proportional to the distance directly and to the time inversely. In this case we get

$$-a\text{A} = 1\,.\,0\,.\,\text{B} \quad , \qquad \text{A} = 0,\ \text{B} = \text{B},$$

$$-a\text{B} = 2\,.\,1\,.\,\text{C} \quad , \qquad \text{C} = \frac{-a}{1.2}\,\text{B} \quad ,$$

$$-a\text{C} = 3\,.\,2\,.\,\text{D} \quad , \qquad \text{D} = \frac{-a}{2.3}\,\text{C} \quad ,$$

$$-a\text{D} = 4\,.\,3\,.\,\text{E} \quad , \qquad \text{E} = \frac{-a}{3.4}\,\text{D} \quad , \ \&\text{c.}$$

$$\phi z = \text{B}\left\{\frac{z}{1} - \frac{a}{1}\,\frac{z^2}{1.2} + \frac{a^2}{1.2}\,\frac{z^3}{1.2.3} - \frac{a^3}{1.2.3}\,\frac{z^4}{1.2.3.4} + \&\text{c.}\right\}$$

When $n = \ 1$ we have

$$-a\text{A} = 1\,.\,(-1)\,.\,\text{B} \quad , \qquad \text{A} = 0$$

$$-a\text{B} = 2\,.\quad 0\ .\,\text{C} \quad , \qquad \text{B} = 0\,,\ \text{C} = \text{C}$$

$$-a\text{C} = 3\,.\quad 1\ .\,\text{D} \quad , \qquad \text{D} = \frac{-a}{1.3}\,\text{C},$$

$$-a\text{D} = 4\,.\quad 2\ .\,\text{E} \quad , \qquad \text{E} = \frac{-a}{2.4}\,\text{D}\,,\ \&\text{c.}$$

$$\phi z = 1.2\text{C}\left\{\frac{z^2}{1.2} - \frac{a}{1}\,\frac{z^3}{1.2.3} + \frac{a^2}{1.2}\,\frac{z^4}{1.2.3.4} - \frac{a^3}{1.2.3}\,\frac{z^5}{1.2.3.4.5} + \&\text{c.}\right\}$$

And similarly for $n = -2$

$$1.2.3\ \text{D}\left\{\frac{z^3}{1.2.3} - \frac{a}{1}\,\frac{z^4}{1\ldots4} + \frac{a^2}{1.2}\,\frac{z^5}{1\ldots.5} - \&\text{c.}\right\}$$

where we at once observe that each series is the primitive (integral) of the preceding.

In our original inquiry the tension at any point was measured by the length of the chain below it, but if we suppose a load w to be appended, that tension will be represented by $w+z$, and the condition necessary for a simple oscillation will be expressed by

$$-ax = {}_{1z}\{(w+z)_{1z}x\} = {}_{1z}x + (w+z)_{2z}x ,$$

and then the investigation becomes much more complex. On using the same method as before we find

$$\begin{array}{lll} -a\mathrm{A} = 1^2.\mathrm{B} & + \quad 1.2w.\mathrm{C}\,; & \mathrm{C} = -\dfrac{a\mathrm{A}+1^2.\mathrm{B}}{1.2w}\,; \\[2ex] -a\mathrm{B} = 2^2.\mathrm{C} & + \quad 2.3w.\mathrm{D}\,; & \mathrm{D} = -\dfrac{a\mathrm{B}+2^2.\mathrm{C}}{2.3w}\,; \\[2ex] -a\mathrm{C} = 3^2.\mathrm{D} & + \quad 3.4w.\mathrm{E}\,; & \mathrm{E} = -\dfrac{a\mathrm{C}+3^2.\mathrm{D}}{3.4w}\,; \end{array}$$

in which each new coefficient is deduced from the two preceding ones. In this case the computation of the coefficients is very tedious, while that of the function and of its derivative is more so; but these being found for any given z, the successive derivatives are found as before, for

$$\begin{array}{ll} -a.x - {}_{1z}x & = (w+z)_{2z}x\,, \\ -a.{}_{1z}x - 2.{}_{2z}x & = (w+z)_{3z}x\,, \\ -a.{}_{2z}x - 3.{}_{3z}x & = (w+z)_{4z}x\,, \end{array}$$

and thus it is easy to compute the value for any proximate value of z.

The complexity in the preceding case arises from the circumstance that the terms of the series for $w.{}_{2z}x$ are displaced from those of $z.{}_{2x}$; and that, therefore, each new coefficient involves the two preceding ones. Now the terms of ${}_{1z}x$, $z.x$, $z^2.{}_{2z}x$, &c., are all coincident, and thus we are tempted to go one step farther and to propose for inquiry the condition

$$ax = n.{}_{1z}x + pz.{}_{2z}x + z^2.\ {}_{2}x\ .$$

Proceeding in the same way as before, we have

$$\begin{array}{rclclclclcl} x & = & \mathrm{A} & + & \mathrm{B}z & + & \mathrm{C}z^2 & + & \mathrm{D}z^3 & + & \mathrm{E}z^4 + \\ {}_{1z}x & = & \mathrm{B} & + & 2\mathrm{C}z & + & 3\mathrm{D}z^2 & + & 4\mathrm{E}z^3 & + & 5\mathrm{F}z^4 + \\ z.{}_{2z}x & = & & & 1.2\mathrm{C}z & + & 2.3\mathrm{D}z^2 & + & 3.4\mathrm{E}z^3 & + & 4.5\mathrm{F}z^4 + \\ {}^2.\ {}_{z}x & = & & & & & 1.2.3\mathrm{D}z^2 & + & 2.3.4\mathrm{E}z^3 & + & 3.4.5\mathrm{F}z^4 + \end{array}$$

and thence the equations of condition,

$$\begin{aligned}
a\mathrm{A} &= n\mathrm{B} \\
a\mathrm{B} &= (2n + 1.2p)\mathrm{C} \\
a\mathrm{C} &= (3n + 2.3p + 1.2.3)\mathrm{D} \\
a\mathrm{D} &= (4n + 3.4p + 2.3.4)\mathrm{E} \\
a\mathrm{E} &= (5n + 4.5p + 3.4.5)\mathrm{F}\ , \ \&c.,
\end{aligned}$$

which may be written

$$\begin{aligned}
a\mathrm{A} &= n.1\mathrm{B} \\
a\mathrm{B} &= (n + 1p)2\mathrm{C} \\
a\mathrm{C} &= (n + 2p + 1.2)3\mathrm{D} \\
a\mathrm{D} &= (n + 3p + 2.3)4\mathrm{D}\ ,
\end{aligned}$$

and in general

$$a\mathrm{Q} = \{n + (r-1)p + (r-2)(r-1)\}r\mathrm{P}\ ,$$

and thus the denominator in the term containing $a^r z^r$ is the continued product of the natural numbers 1.2.3. up to r, multiplied by the continued product of all the terms of the progression

$$(n - p + 2) + r(p - 3) + r^2$$

for all values of r up to the same limit.

The terms of this progression can be resolved into products whenever n and p are such that $(p-1)^2 - 4n$ is a square number, and then the denominators are the continued products of the terms of three arithmetical progressions.

Thus when $p = 3$ and $n = 1$, that is when the condition $ax = {}_{1z}x + 3z.{}_{2z}x + z^2.{}_{3z}x$ is proposed, the solution becomes

$$x = \mathrm{A}\left\{ 1 + \frac{az}{1^3} + \frac{a^2z^2}{1^3.2^3} + \frac{a^3z^3}{1^3.2^3.3^3} + \ \&c. \right\},$$

in which the three progressions coincide.

On introducing fractional coefficients we get arithmetical progressions with differences other than unit; thus, on making $a = \dfrac{1}{27}$, $n = \dfrac{2}{9}$ $p = 2$ the three progressions become

1, 4, 7, 10, &c.; 2, 5, 8, 11, &c., and 3, 6, 9, 12, &c.,

whose terms just fill up the progression of natural numbers, so that for the condition

$$-\frac{1}{27}x = \frac{2}{9}\cdot {}_{1z}x + 2z \cdot {}_{2z}x + z^2 \cdot {}_{3z}x$$

we have

$$x = A\left\{1 - \frac{z}{1.2.3} + \frac{z^2}{1.2.3.4.5.6} - \frac{z^3}{1\ldots\ldots 9} + \&c.\right\},$$

which is related to recurring functions of the third order.

There is thus opened up a wide field for research, rendered, however, unprofitable by the absence of application to physical phenomena.

3. Note on the Biological Tests employed in determining the Purity of Water. By A. W. Hare, M.B. Edin. (Plates X., XI.)

(From the Public Health Laboratories, Edinburgh University.)

Part I.

There are two experimental paths by which the facts relating to organic impurity in water may be approached, the chemical and the biological; and there are two aspects in which these facts may be regarded, the catalytic and the fermentative. The series of observations made in following the one path is the necessary complement of that met with in the other. The path of chemical investigation has been well cleared, and is easily traversed; that of biological inquiry is still beset by many impediments, and is as yet by no means a safe one. It is the object of many recent researches, and of this paper, to lessen these difficulties. The pollution of water with organic substances is inevitably associated with the presence of those organisms whose function it is in the economy of nature to disintegrate such materials. These organisms in their turn are dependent upon organic matter for their continued existence; the Bacteriaceæ to which they belong being distinguished from allied forms by an entire absence of chlorophyll, which obliges them to feed only on organic substances. The relation of these two factors of pollution, each to each, is therefore a necessary one; and due regard must in all cases be paid to each in determining the degree of pollution that their coexistence implies. It is further of

importance to inquire if their relation to one another can be more closely analysed, and whether specific features of the one factor are necessarily related to a certain constitution of the other. A well-established case of such a relationship may be here cited in illustration, and as indicating the direction in which the present inquiry is to proceed. The *Beggiatoa alba*, a well-known filamentous organism, is an inhabitant of warm sulphur springs, in which it carries on a definite analytical function, decomposing sulphur compounds in solution in the water, and giving off sulphuretted hydrogen. That such facts are of frequent occurrence is shown by the work of Pasteur in the case of the Torula group; while the elaborate researches of Duclaux exhibit functional specialism in this respect carried to an astonishing degree of complexity. It is not then beside the mark to press the analysis of aquatic microbes one stage further, and to inquire whether, in addition to the fact of organic contamination predicable from their presence, there is not a possibility of recognising definite species, associated with special forms of organic material, particularly such as are derived from sewage matter. In making this attempt an initial difficulty is met with in the fact that a complete classification of Bacteria according to their function is not yet made; their provisional division into zymogenic, pathogenic, and chromogenic forms, though of great dialectic convenience, is of no value in practical questions, for each division is in part overlapped by portions of the other two. A new classification must therefore be attempted to suit the conditions of this inquiry; and that which specially commends itself as at once practical and well adapted to our purpose is based on observing the nature of the pabulum on which various species subsist; thus dividing them into groups according to the complexity of their assimilative processes, as higher groups of organisms may be classified according to their carnivorous or herbivorous habits of life. In the case of the Bacteria this classification can rest on no such obvious diversity of function, for they are distinctly omnivorous; but there are important differences in the composition of the organic matter in water, corresponding to the different sources from which it is derived, and it is not unreasonable to expect that analogous differences will be found to obtain amongst the species of microbes associated with it. Two chief sources of organic material in rivers

are to be recognised: the natural processes of vegetable growth and decay contribute a large amount of soluble material which is carried down by land drainage; the other source, which supplies both soluble and insoluble organic matter, is the drainage of towns where a flushing system of sewage disposal obtains, and where its products are poured into rivers. There is an important difference between the organic material derived from these two sources. This difference does not depend on an original diversity of character between animal and vegetable waste products; for though such a diversity is doubtless recognisable by delicate tests, it is too fine a point to found a generalisation upon for any practical purpose. But the difference that does obtain is due to the disparate stage of waste product decomposition in which organic matter from the two sources reaches a river. In both cases the disintegration is carried on by the analytical action of micro-organisms, and finally results in the production of such simpler substances as binary compounds of C, H, N, and O. In the vegetable waste products from land drainage this process is far advanced, and the resulting materials are in a soluble and much simplified form by the time they reach the river water; whereas in the animal and vegetable waste products present in sewage the process is only commencing, and in places where the drains are flushed into a river, additional insoluble matter may be introduced, and may pass far down the stream before even the first stage of its disintegration takes place, rendering it soluble, and thus in a position for the completion of the process. It remains then to inquire, in regard to the disintegration of organic matter, whether the same microbes are present throughout the whole process, or one group of ferments is replaced by another in correspondence with the constantly changing chemical equations that express the several stages by which it advances. That the latter is the case, analogy strongly suggests, and experimental results go far to prove. As an analogous case, that of the fermentation of sugar offers a good example, where the first stage of the process is due to the action of the *Torula cerevisecæ*, and concludes with the formation of alcohol; the further stage of acetous fermentation being produced by a distinct species, the *Mycoderma aceti*. A similar case is that of the lactic fermentation of milk, where *Bacterium lactis* initiates the process, changing lactose into lactic acid, at which point the

Bacillus butyris makes its appearance, and from the lactic produces butyric acid. It is difficult to prove distinctly that the same law holds true in the disintegration of so complex a compound as sewage matter; but the direct evidence obtained from the investigation of decomposing animal solutions points to that conclusion. In such a substance *Bacterium termo* and its congeners appear, and carry on their special functions of decomposition in a definite sequence, one group commencing it labours where another has completed its special share in the process. In the sequel, it will appear, from a series of observations recently made, that different species of microbes preponderate in the different areas of a sewage-laden river, and it will be attempted to show how these distinctions probably depend on the advance of successive fermentative processes from stage to stage, *pari passu* with the flow of the river from point to point, from its initial area of sewage contamination till it is restored to a state of relative purity. In the meantime, however, the status of microbes in water of different qualities requires attention. For the purpose of description it is convenient to differentiate four qualities of water, viz., distilled water, spring water, river water, and dilute sewage.

1. *Distilled Water.*—The absence of organic matter prevents any great development of microbes in this medium. Yet marked diversities are found in the behaviour of different species in this respect. Whilst it has been shown by Crookes, Tidy, and Odling that *Bacillus anthracis* does not long survive its introduction into distilled water, and by P. Frankland that the same is true of Koch's *Comma bacillus* and of Finkler's and Prior's *bacillus*, yet it has been shown that the hardy *Bacillus pyocyaneus* is capable of surviving for a considerable time in such conditions, and that, in the first place, it even increases in numbers (P. Frankland). Many of the ordinary species present in atmospheric dust are also capable of living in distilled water; hence the necessity known to all bacteriologists of keeping distilled water used for microscopic purposes rigidly free from direct contact with the air, and of frequently obtaining supplies freshly prepared. But these contaminations, though very serious where exactitude of microscopic observation is at stake, have no immediate hygienic importance, and we may consider distilled water at least as an absolutely safe substance for human consumption.

2. *Spring Water.*—Water from deep wells is always, in the first instance, nearly free from the presence of microbes; that from shallow wells may be seriously contaminated with sewage matter, and may be loaded with organisms. Deep wells in the chalk at times supply water rich in organic matter, but which only yields evidence of microbic activity after it has stood for a time in contact with the air. In such cases it is not unfrequently more crowded with organisms even than river water,—a condition probably due to the smaller number of species present in such cases; for thus the struggle of opposing vital requirements is avoided, and the total sum of possible individual life thereby increased.

3. *River Water.*—Glacier water is free from microbes, but in all other cases river water contains a larger or smaller number of micro-organisms, depending on the relative amount of organic matter that it holds in solution. The nature of the land drained by a river, the presence or absence of direct sewage infection, and the speed of its flow, are the chief conditions affecting the purity of its water. As will be shown in the second part of this paper, the popular sentiment in favour of rapid and tumultuous rivers as a source of domestic supply is probably based on a misconception, and is completely at variance with sound deductions from the facts of the case.

4. *Sewage.*—The rich supplies of organic matter here present permit of an enormous development of microbes, but the fermentative activity thus established is of a duration inversely proportional to its intensity. After a maximum development occurring on the third or fourth day, a rapid decrease is observed in the number of microbes, so that in the course of a week to ten days there may be a smaller number in a sample of sewage than in a stored specimen of river water (Bischof). In this case the large amount of food material is rapidly exhausted by the disproportionate production of microbe life; and when the supply comes to an end, the death-rate amongst the microbes is for a time excessive.

We must now turn to the tests employed in determining the number and varieties of microbes present in water, and inquire into their accuracy and reliability. Various methods have been suggested, some giving quantitative, others qualitative results. It is obvious that a method giving reliable information on both points is desirable, since the total number of microbes present in a specimen of water

gives an indication of the quantity of organic matter necessarily present for the support of so much life; and the special forms present and their proportion to one another, is no less important, from the indication thus given, as the sequel will show, of the state of decomposition at which such organic material has arrived.

(*a*) Of the purely quantitative biological tests that by "dilution" may be mentioned, employed by Fol and Dunant. It consists in enormously diluting the sample to be tested with germless water in a known proportion, and in inoculating a large number of culture glasses with equal quantities of this mixture. The proportion of the culture glasses remaining sterile to those showing microbe life affords a basis for calculating the number of germs in the original specimen. This method is inexact; it labours under the twofold disadvantage of depending on perfect mechanical mixing under great difficulties, and of the certainty that the culture fluid used could not suit the requirements of *every* microbe present; some, therefore, would not grow in it, and would be omitted from the calculation. When the number thus obtained is multiplied by the number of dilutions previously carried out, the omissions thus made will be multiplied, and the resulting error in the calculated total most serious.

(*b*) Of the purely qualitative methods may be mentioned that by "fractional cultivation," successfully employed by Lister in separating species from one another. It is, however, so laborious as to be inapplicable for practical purposes, although it was primarily instrumental in establishing the important scientific principle of specificity amongst microbes.

(*c*) Another method is that proposed by Dupré, in which the nature of organic impurities present is determined by observing changes in the aeration of water, the gases absorbed and given off giving an index of the amount of vital action occurring in the specimen investigated. It must be seriously doubted whether this can ever be developed into an accurate method of observation: the factors of sewage contamination are so variable under varied conditions, that a uniformity of results is scarcely to be hoped for; while there is room for so many fallacies in this method, that it would require confirmation from others before its results could be accepted.

(*d*) By far the best method of determining the number and varieties of microbes in water is that introduced by Koch. It con-

sists in mixing a sample of water of definite bulk with a quantity of liquefied nutrient jelly previously sterilised. This mixture is poured on a sterile glass plate with aseptic precautions. When it solidifies, the microbes in it are fixed and develop, each becoming the centre of a colony growing in the jelly, and each colony representing a "pure cultivation" of its parent germ. The number of such colonies bears a definite relation to the total number of microbes present in the original sample of water, and their varied appearances show with what diversity of species the sample was inhabited. Species which cannot be recognised in this way at once may be identified by removing a portion of one such colony to a separate quantity of culture material, in which its characteristic growth may be separately observed. A convenient form of apparatus is shown in Plate X. It consists of a deep glass bell of 8 inches inside diameter, standing in a glass dish that closely fits its mouth. Within are alternate square glass plates and india-rubber washers, fitting closely to the inside of the bell-jar. When the apparatus is closed it will travel safely without any movement of the plate, and can thus be conveyed to the near vicinity of the water which it is desired to test. The plates are sterilised by heating at 170° C for one hour, and the rings by steeping them in 1 per cent. aqueous solution of perchloride of mercury for twenty-four hours. A piece of thick filter paper soaked in the same solution is placed in the floor of the apparatus. A series of ten or twelve plates is contained in the apparatus. In making observations the upper two or three of these should be used as "control" plates, since they run the greatest risk of aerial contamination from their longer exposure to the air in manipulating the apparatus. They are charged with a layer of the nutrient jelly alone without the addition of the water to be tested. If they give negative results, *i.e.* if the layer of nutrient jelly upon them shows no foci of microbic growth, the manipulative procedure in the experiment may be considered reliable. In examining a river for microbes at different parts of its course, one such set of twelve plates may advantageously be used at each point, a measured quantity of the water being used in each case, so that the experiments may have uniformity of scale throughout. Two such experimental plates are shown in Plate XI., in which the results of the test are shown in the case of a river examined

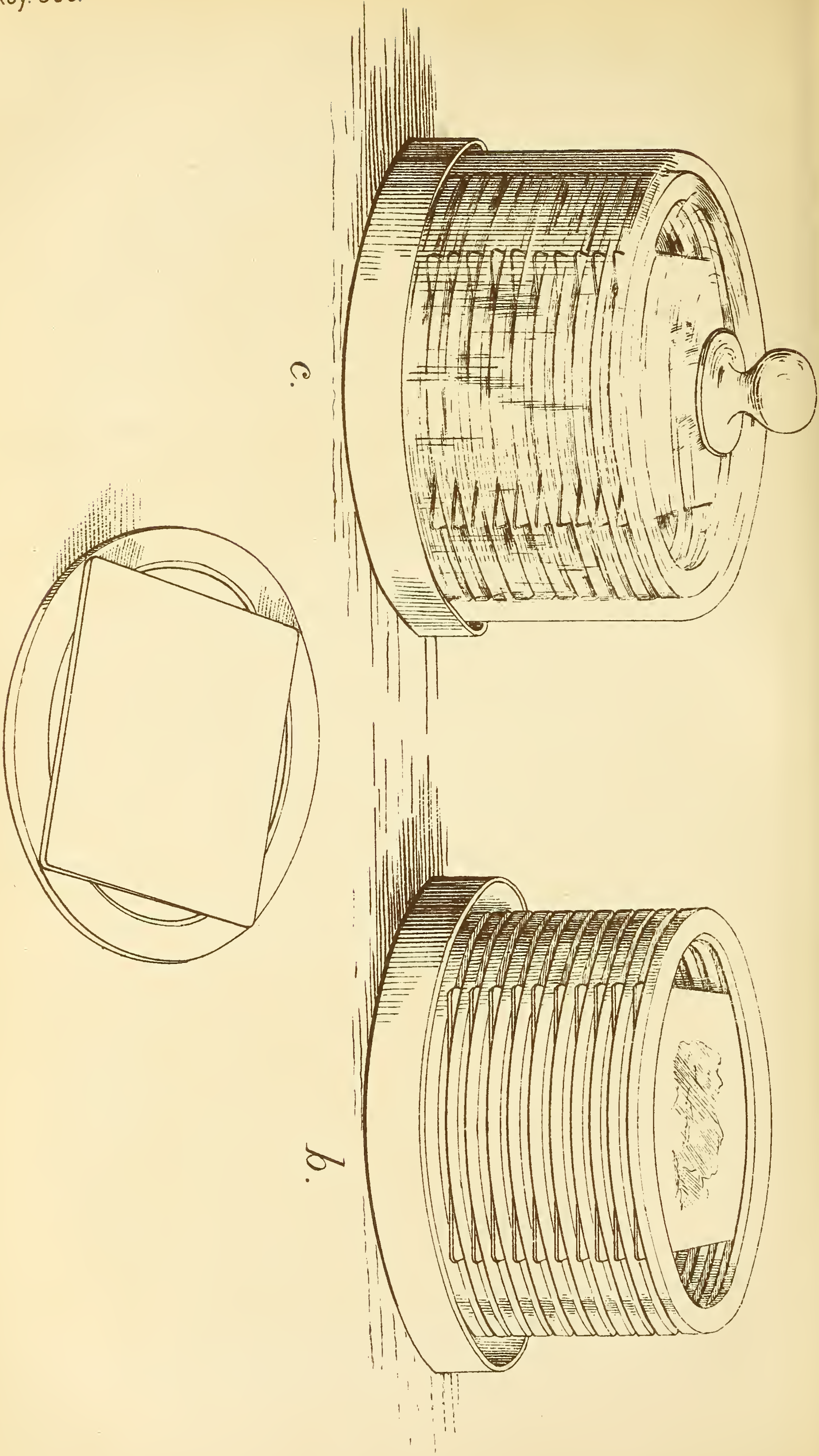

c.
a.
b.

FIG. 1. (PLATE II. TABLE I.)

PLATE CULTIVATION made with five drops of River Water above the point where the Sewage enters.

FIG. 2. (PLATE I. TABLE II.)

PLATE CULTIVATION made with five drops of River Water below the point where the Sewage enters.

immediately above, and again immediately below a source of sewage contamination. The results obtained by this method in the case of a rapid river with gross sewage contamination will be detailed in the second part of this paper, where special attention will be drawn to the way in which this test is of value in associating special forms of microbes with special conditions of organic decomposition, thus acting as a qualitative test of some degree of definite value in determining the purity of water.

(*e*) Another biological method is an extension of the preceding. Having found by the preceding method what organisms are usual inhabitants of a river, the introduction of a foreign organism at a certain point, and its recovery from the stream at another by plate cultivations, may give valuable evidence as to the condition of the river-water between these points. The organism so employed must be perfectly distinctive in its mode of growth, and its relation to other organisms well known, as also its powers of survival and multiplication in a variety of conditions. Given these data, much may be learned from its behaviour in various areas of the river examined. In the second part of the paper such an experiment will be described.

In the present state of our knowledge of water testing, it would be unwise to discard the methods of chemical examination for any one, or a combination, of the above biological tests. But some of them, and particularly that of Koch, are capable of affording strong corroboration of the results obtained by chemical tests; and since it is its vital rather than its purely chemical contaminations that render water a source of danger to the health of the human subject, it may safely be predicted that, when extended and rendered yet more exact, these biological tests will become an essential element in the experimental determination of the purity of water.

4. Alternants which are Constant Multiples of the Difference-Product of the Variables. By A. H. Anglin, Esq., M.A.

5. Glories, Halos, and Coronæ seen from Ben Nevis Observatory. Extracts from Log Book. By R. T. Omond. Communicated by Professor Tait. (Plate XII.)

Red [2]
Red and Blue
Red [1]
☽

May 23, 1886.—Solar corona seen at 12^h. Colours as in margin. Inner and outer reds distinct, but space between very mixed in colours.

Radius of inner red, .	3° 25′	3° 25′	3° 18′	3° 25′
„ outer red, .	6° 41′	6° 35′	6° 41′	6° 41′

May 27, 1886.—At 13^h bright solar halo seen; red inside, then yellow, and blue outside.

	Radius of Red.	Yellow.	Blue.
I.	21° 24′	23° 44′	24° 43′
II.	22° 0′	22° 49′	24° 43′
III.	22° 12′	23° 30′	24° 13′
IV.	22° 0′	23° 3′	24° 43′

The double-headed arrows show the diameters along which the measurements (I., II., III., and IV.) were taken.

June 4, 1886.—At 5^h 10^m halo and mock suns seen. Halo red inside and blue outside. Mock suns at each side, so bright as to be dazzling; right hand the brightest. Radius from centre of sun to centre of mock suns = 23° 17′. Vertical white beam below sun, and horizontal segment passing through mock sun; this horizontal arc was 12° 32′ above the level-topped haze that hid the horizon. The mock sun on the right was white and outside the red of the halo; the mock sun on the left side was coloured red, yellow, and blue in same order as the halo. The following measurements of the halo were made:—

Radius of red, .	22° 36′	22° 36′
„ yellow, .	23° 3′	23° 30′
„ blue, .	23° 44′	23° 44′

At 8^h the halo was seen again; rather faint.

Radius of inside of red,	22° 0′	Measured on lower segment of halo.
„ outside of red,	23° 17′	
„ outside of blue,	24° 43′	

August 14, 1886.—At 13^h solar halo observed; no colours visible. Radius = 23° 30, 23° 3′, and 22° 36′.

October 7, 1886.—At 12^h a solar fog-bow was observed. No colours, only a broad white band.

Radius to inside of bow,	. .	36° 20′
" outside of bow,	. .	43 36′

October 22, 1886.—Fog-bow seen at 11^h 25^m. Colours as in fig. 1; the order of colours in the glory was not determined. No measurements were got. The *pink* was a badly-defined space, not a true band.

October 25, 1886.—At 16^h glory seen on fog to N.E.; rather misty and badly defined. Four rings, inmost a mere blotch; second—the brightest of the four—yellow and red; third, green and red; fourth, only red seen clearly. The third and fourth were only seen occasionally.

Radius of first red,	. .	1° 10′ (bad observation)
" second red,	. .	3° 46′
" third red,	. .	6° 18′
" fourth red,	. .	7° 22′
" yellow in second ring,		2° 55′
" green in third ring,		4° 31′

November 5, 1886.—At 18^h lunar corona seen; colours as on margin (outer blue probably a *margin*). Radius of red = 2° 17′ and of blue = 4° 43′.

Blue
Red
White
☽

Well-defined halo seen all evening. Two measurements gave as radius 22° 36′ and 22° 0′.

November 12, 1886.—Double fog-bow seen at 13^h; outer bow white, inner bow red and blue, red being inside.

Triple corona seen at 19^h (lunar), colours as in margin.

Radius of inside red,	.	1° $2\frac{1}{2}$′
" middle red,	.	2° 29′
" middle green,	.	2° 3′

Size apparently varying. Measurements were stopped by the ice crystals deposited by passing mist clogging the stephanome.

Red
Green
Blue
Red
Yellow
Green
Blue
Red
White
☽

Portion of halo seen at 21^h. Radius = 21° 13′.

Red 3 Green Blue Red 2 Green Blue Red 1 White ☽			

November 14, 1886.—Triple lunar corona seen at 5^h. Colours as noted on margin.

Radius of inner red,	.	1° 23½′
,, middle red,	.	2° 52′
,, outer red,	.	3° 36′
,, inner blue,	.	1° 34½′

November 16, 1886.—Fog-bow seen at 11^h; red outside and white inside. A fainter bow was seen inside this one at times.

December 16, 1886.—Glory and fog-bow seen at 15^h, too fleeting to measure. The glory was double, with reds outside; the fog-bow a broad whitish band, with occasionally another bow inside it more sharply defined and coloured, but the order of its colours was not observed.

December 20, 1886.—At 12^h upper half of halo seen; red inside, white outside.

Radius of middle of red,	. .	21° 54′
,, junction of red and white,		22° 12′
,, middle of white,	. .	23° 58′

December 26, 1886.—At 12^h 30^m misty glory and double fog-bow seen; outer bow had red outside, and inner bow red inside. No measurements got.

December 30, 1886.—Double fog-bow seen at 11^h. Red outside outer bow and inside inner bow. The following rough measurements were got:—

Radius of outside of outer bow,	. .	41° 22′
,, middle ,,	. .	39° 20′
,, inside ,,	. .	36° 36′
,, outside of inner bow,	. .	34° 44′
,, inside ,,	. .	32° 20′

The first and last measurements give the radii of the outer and inner red respectively.

Misty glory seen at 15^h, colours as in fig. 2; the central spot a confused mass of colour.

Radius of red, . . 1° 53′.

January 2, 1887.—At 18^h triple corona seen, red outside in all three rings; outermost ring faint and evanescent. At times a fourth ring was seen inside these, surrounding white space near moon, but it was too small to measure (less than 50′). This corona was formed on scud, size varied.

Radius of innermost red, . .	1° 22′.
„ middle red, . . .	2° 5′
„ outermost red, . .	4° 23′.

When no scud was passing a (faint) blue corona was seen on the clear sky. Radius = 3° 2′. Lunar fog-bow was also seen at 18^h. Radius about 38° 40′.

January 3, 1887.—Triple corona (lunar) seen at 19^h; red outside in all three rings.

Radius of first red, . .	0° 54′	0° 56′	1° 0′
„ second red, . .	1° 48′		
„ third red, . .	3° 36′	3° 15′	

January 5, 1887.—Solar corona seen at 13^h; colours as on margin.

		Red [2]
		Green
Radius of red[1], ...	3° 7′	Red [1]
„ red[2], ...	6° 12′	White
„ extreme outside of red[1], .	4° 13′	⊙

Lunar corona seen at 22^h, colours as on margin; yellow bands narrow, but green broad and badly defined.

		Red [2]
Radius of inner red, . . .	3° 20′	Yellow
„ outer red, . . .	6° 29′	Green
„ inner yellow, . . .	2° 36′	Red [1]
„ outer yellow, . . .	5° 22′	Yellow
		White
		☽

January 8, 1887.—Lunar halo seen at 1^h. Three measurements of radius to inside edge of halo gave 21° 36′, 22° 0′, 20° 51′. Double solar corona seen at 11^h.

Radius of inner red, ...	3° 31′
„ outer red, ...	6° 7′

At same hour glory seen with four rings; larger than usual, and the colours broad and soft-looking. The innermost red was only seen occasionally.

Radius of second red, ...	4° 46′
„ third red, ...	8° 43′
„ fourth red, ...	12° 6′

While measuring this glory, a cloud passed to southward of Ben Nevis, and its shadow blotted out part of these three rings, but did not fall on the observer.

At 12^h portion of glory seen on clouds to northward, though at the time the shadow of the observer fell inside the edge of the cliff (see fig. 3).

February 6, 1887.—Lunar fog bow seen at 2^h.

Inside radius, . . 33° 56′
Outside „ . . 40° 20′ } (∴ bow 6° 24′ broad).

February 8, 1887.—Double lunar corona at 23^h and midnight. Rather indistinct, apparently formed on cirrus clouds.

Radius of inner red, ...	2° 1′
„ outer red, ...	4° 40′

February 9, 1887.—At 6^h double lunar corona seen; colours as on margin.

Red[2]
Green
Blue
Violet
Red[1]
White
☽

Radius of inner red, . .	1° 45′
„ violet, . . .	2° 19′
„ blue, . . .	2° 50′
„ green, . . .	3° 15′
„ outer red, . .	4° 0′

February 12, 1887.—Double lunar corona seen at 7^h; colours as on margin.

Red[2]
Yellow
Green
Purple
Red[1]
White
☽

Radius of inner red, . .	1° 14′
„ purple, . .	1° 34′
„ green, . . .	1° 49′
„ outer red, . .	2° 28′

February 13, 1887.—Double fog-bow seen at 12^h. Trace of red outside outer and inside inner bow. No measurements got.

February 15, 1887.—A few passing glories seen at 14^h 10^m. Solar corona observed on scud at 12^h, always double, sometimes

triple; best seen when the scud was uniform in thickness—not filmy—and thin enough to see the blue sky through. The following measurements were got; those bracketed together were taken at as nearly as possible the same time.

Radius of inner red,	3° 35′	3° 31′	{ 3° 52′	{ 4° 13′	3° 21′
,, outer red,	4° 28′	5° 43′	{ 6° 55′	{ 7° 38	

March 1, 1887.—Solar corona seen at $13^h\ 10^m$ on passing scud. Three rings, red outside in each. The following measurements were got:—

	I.	II.	III.	IV.	V.
Radius of first red, .	1° 46′	...	...	1° 41′	...
,, second red,	2° 26′	2° 34′	2° 39′	2° 41′	...
,, third red, .	4° 8′	...	...	...	4° 40′

At the same hour a faint red corona was seen on cirrus clouds when the scud cleared off. Radius about 0° 56′.

At 17^h misty red colour under sun, and solar corona formed on scud. Three rings, red outside in each.

	I.	II.	III.
Radius of first red, . .	1° 41′	1° 25′	1° 19′
,, second red, .	2° 23′	2° 26′	2° 30′
,, third red, .. .	3° 54′	...	...

March 4, 1887.—Glories seen on passing fog all day. At 11^h 15^m one seen from roof of Observatory, the shadow of the observer falling on the snow about 10 yards away. Colours in following order:—Shadow, white, red^1, blue, green, red^2, blue, red^3.

Radius of inside edge of red^1, . . .	2° 15′
,, outside edge of red^1, . . .	3° 12′

Another glory seen from edge of cliff at $11^h\ 30^m$; no fog-bow with it. It had four rings of colours arranged in the following order:—

(The radii are given with the colours; measurements taken to *outside* of colour in each case.)

Colours.	Radii.
Faint red[4], ...	9° 28′
Faint green,	...
red[3], ...	6° 12′ and 6° 18′
yellow, ...	5° 14′
green, ...	4° 40′
Dark blue, ...	4° 5½′
red[2], ...	3° 25′ and 3° 35′
yellow,	...
white,	...
Bad blue,	...
Yellowish red[1], ...	1° 1½′ and 1° 4½′
Centre of shadow.	...

The radii appeared to vary slightly as the surface of the fog on which the glory was formed rose and fell.

Another glory seen at 14^h 5^m from edge of cliff. Four rings with red outside in each.

Radius of second red, . .	3° 46′
„ seond yellow, . .	2° 40′
„ second blue, . .	1° 43′
„ third red, . . .	10° 27′
„ third yellow, . .	6° 12′
„ third green, . . .	4° 25′

Bright glories, too fleeting to measure, were seen all afternoon on the loose fog drifting across the hill top.

Solar corona seen at 11^h 10^m. Colours and radii as under :—

Colours.	Rad. to Centre of Colour.	To Outer Edge of Colour.	To Inner Edge.
⊙			
White,	...	...	...
Red[1],	2° 9′	2° 27′ and 2° 30′	1° 40′
Dark blue,	2° 30′	...	...
Green,	...	...	...
Red[2],	3° 54′	4° 43′ and 4° 37′	3° 31′
Dark blue,	...	...	...
Green,	...	...	...
Red[3],	6° 48′	...	...
Dark blue,	...	...	...
Green,	...	...	...
Red[4],	7° 59½′	...	...

Another solar corona seen at 12^h 10^m. Colours and radii (outer edge of colour in each case) as on margin.

☉	Radii.
White	...
Yellow	...
Red[1] . .	2° 39′ and 2′ 39′
Dark blue .	...
Green . .	...
Yellow . .	...
Red[2] . .	4° 46′
Dark blue .	...
Green . .	...
Red[3] . .	7° 55′
Blue . .	...
Red[4] . .	9° 41′

Solar halo seen between 9 and 10 hours, as sketched in fig. 4; colours as marked. The mock suns were red inside and blue outside; they lay distinctly on the outer edge of the halo (A). The following measurements of them were got:—

	Left Mock Sun.	Right Mock Sun.
Radius of red,	22° 49′ and 23° 17′	23° 3′
,, white,	23° 44′ and 24° 13′	24° 13′
,, blue,	24° 28′	24° 58′

Faint traces of green and yellow were seen in the mock suns at 10^h 10^m. The mock suns and horizontal white bar were about 24° above the horizon at 9^h. As the sun rose higher the bar curved upwards, and at noon was as sketched in fig. 5. The bar then extended inside the halo A almost to the sun, which it had not done in the morning.

The following measurements were got at various times during the forenoon of the different parts by Mr Rankin:—

Sun to western mock sun,	23° 46′
,, eastern ,,	23° 42′
,, white circle E,	79° 56′ and 81° 23′
,, green at junction of C and D,	50° 26′
Red of C to red of A,	25° 28′ and 24° 48′
Blue of C to blue of A,	25° 13′ and 24° 32′
Green of C to green of A,	24° 20′

The arc B distinctly overlapped the halo A, the reds coinciding above the sun; but the arc D only touched the halo C, its red combining apparently with the blue of C.

At 12^h 20^m measurements were made of the wings between halo

A and arc B. Two points, P and Q (see fig. 6), were taken, and their distances measured from the sun (S) and the mock sun (Z).

S to Z	23° 42′
P to Q	13° 22′
S to Q	24° 8′
S to P	29° 52′
Z to Q	28° 56′
Z to P	19° 34′

The wings were not arcs of one circle; judging by the use of *ring* of stephanome, their centres of curvature lay about midway between the sun and the mock suns on either side.

One mock sun was seen again at 17^h.

Radius of red, 22° 0′; of yellow, 22° 36′; of blue, 23° 17′.

March 16, 1887.—At 11^h two mock suns, with faint trace of white, horizontal circle outside of them, seen on apparently perfectly clear sky. No trace of 22° halo. Radius of eastern mock sun = 25° 13′.

April 5, 1887.—At 3^h, double lunar corona seen; colours and radii as on margin.

Fog-bow (faint) seen at the same time.

Colour.	Radii.
Red² . . .	4° 13′
Yellow² . . .	3° 58′
Blue . . .	...
Red¹ . . .	2° 10′
Yellow¹ . . .	1° 38′
Bluish White . .	...

☽

May 13, 1887.—At 11^h pink-coloured cloud seen with upper part under sun, coloured blue, green, and red. Length of cloud = 103°. Breadth = 6° 30′.

Radius of green (only distinct line of colour) = 12° 36′.

This cloud vanished suddenly, showing a halo on cirrus much higher up. Shortly afterwards the halo got more distinct; it had inside it another ring, as in fig. 7. The outer ring was a distinct halo, with red inside and blue outside; radius of red = 22° 12′. The inner ring had only a faint tinge of red inside; radius of this red = 17° 54′. By $11^h\,15^m$ all had disappeared.

Coloured clouds were seen again several times during the day, and at 17^h a solar halo was again observed. The following measurements were got:—

Radius of red, ...	21° 54′ and	22° 12′
,, yellowish green, . .	23° 17′ ,,	23° 17′
,, blue, ...	24° 28′ ,,	24° 13′.

No trace of an inner ring was visible.

June 3, 1887.—Solar halo seen at 11^h and 12^h.

At 11^h radius of red, ...	22° 18′
,, ,, blue, ...	22° 43′

At 12^h the halo had a segment projecting from its S.E. side (as shown in fig. 8) that appeared to cut into the halo, but was not visible inside it. The halo was brightly coloured—red inside and blue inside; the segment was tinged with red inside.

Radius of red of halo, . . .	22° 18′ and 22° 0′	
,, red of segment at farthest point from sun, . .	25° 28′.	...

June 10, 1887.—Faint glory seen at 4^h; no measurements got.

June 20, 1887.—Solar halo seen at 4^h and 6^h; no measurements got.

June 28, 1887.—Fog lying over the hills round all day, and occasionally covering Ben Nevis also. At 20^h glory seen on this fog; three rings, badly defined, and too fleeting to measure, but the middle ring was distinctly the brightest. Red outside in all the rings.

June 29, 1887.—Fog-bow, occasionally double, observed at 5^h and 6^h.

July 2, 1887.—Solar halo, seen at 7^h, red inside; rather broad and faint. Seen again at 9^h and at 13^h.

July 21, 1887.—At 10^h 50^m, a double glory was observed from edge of cliff. The following measurements were taken:—

Radius of inner red, .	1° 57′ 36″ :	2° 1′ 12″ :	1° 59′ 14″
,, outer red, .	3° 18′ 0″ :	3° 19′ 30″.	...

A double fog-bow was also seen at times. Red inside inner bow and outside outer bow; the rest of the bows were white.

A solar halo was seen at 8^h; no measurements got.

July 31, 1887.—Double rainbow seen at 18^h 45^m, primary bow the brightest. Red outside primary, and inside secondary. The following measurements were made from the centre of shadow of observer's head:—

Radius of red of primary, . .	42° 48′ :	44° 17′
,, secondary, . .	53° 8′	...

August 17, 1887.—Glory seen at 5^h 7^m. Single ring, badly defined, with occasional traces of second outer ring. Radius of red of first ring (two measurements), 3° 42′ and 3° 52′.

August 19, 1887.—Glories seen about 9^h 40^m. Two rings distinct, and two others outside these indistinct. Red outside in all. The colour next the shadow was yellow; between its red (*i.e.*, red^1) and red^2 was violet, and between red^2 and red^3 green. No measurements got.

A fog-bow was seen at 9^h 45^m, with glory inside it round shadow.

At 10^h 22^m another glory seen at cliff; red outside, radius = 2° 36′. The violet was a broad band reaching nearly, if not quite, to the shadow.

Two other measurements of different glories about the same time gave for radius of red, 3° 4′ and 3° 13′.

Misty glories were seen at various times during the day.

A rainbow was seen at 19^h.

August 21, 1887.—Triple glory seen at about 9^h 30^m; colours as in fig. 9.

$Yellow^1$ and red^1 faint.

$Yellow^2$ and red^2 very distinct.

In 2 there was no green, and in 3 no blue [or only faint traces in both cases].

In 3 the green and red were the most distinct colours; yellow barely visible.

When clouds or fog blew up the corrie where the glory was seen the colours got blurred and indistinct.

The following measurements were taken by Mr Herbertson :—

Radius of $yellow^2$ (inside edge of colour),	.	2° 36′ and	2° 32′
,, junction of $yellow^2$ and red^2,	.	3° 15′ ,,	3° 29′
,, red^2 (outside edge of colour),	.	4° 25′ ,,	4° 18′
,, red^3, ,,	.	7° 30′	...
,, $green^3$, ,,	.	6° 48′	...

August 23, 1887.—At 5^h 20^m glory seen from window in tower door. Two and three rings seen; red outside in each. No measurements got.

August 28, 1887.—At 14^h a rainbow was seen.

September 1, 1887.—At 22^h an ill-defined white lunar halo was observed.

September 4, 1887.—At 4^h a lunar corona was observed; order of colours as shown on margin. Too fleeting to measure.

Pinkish red
Blue
Yellow
White
☽

September 18, 1887.—Glories seen from edge of cliff in afternoon. At 13^h 30^m measurements were made as under :—

	Radius.	
Outside of red², ...	6° 24′ and	6° 29′
Middle of red², ...	5° 34′	...
Inside of red², ...	4° 56′ and	5° 3′
Green, ...	4° 43′	...
Blue, ...	4° 23′	...
Outside of red¹, ...	4° 5½′ and	3° 46′
Middle of red¹, ...	3° 44′ ,,	3° 35′
Inside of red¹, ...	2° 56′	...
Red glow round shadow, . . .	1° 12½′	...
Shadow of observer, . . .	...	...

A fog-bow was observed at the same time, measuring 35° 16′. [To inside of bow?]

September 21, 1887.—Fog lying over and hiding all the lower hills most of the day. On this glories were seen frequently; the following measurements were got at 7^h 10^m. Order of colours as on margin, with occasional traces of third red :—

Radius of red², .	4° 18′	3° 15′	4° 18′	3° 54′
,, yellow²,	2° 59′	3° 2′	3° 4′	2° 39′
,, blue², .	2° 17′	2° 21′	2° 24′	2° 12′

Red²
Yellow²
Blue²
Red¹
Yellow¹
Shadow

After 8^h the following measurements were got—some from top of tower, and some from edge of cliff. The glory seemed to vary in size as different parts of the fog drifted past :—

	Tower.			Cliff.	
Radius of red², .	3° 15′	3° 44′	3° 50′	4° 6′	3° 54′
,, yellow²	2° 37′	2° 47′	2° 58′	3° 2′	2° 47′
,, blue², .	2° 2′	2° 0′	2° 13′	2° 19′	1° 58′
,, red³, .	...	...	...	6° 55′	...

October 4, 1887.—When fog was clearing off in early morning, a double lunar corona was observed. The following were the order of colours and radii measured at 5^h :—

☽ White, yellow, orange, red,
2° 12′

Violet, blue, green, yellow, orange, red,
4° 34′.

Glories were also seen during the day on the fog in the valley to northward.

The following measurements of one with five rings were taken between 11^h and 12^h :—

Radius of red⁵, . . .	Too faint to measure.
,, red⁴, . . .	6° 18′
,, red³, . . .	4° 37′
,, red², . . .	2° 40′
,, red¹, . . .	0° 52 (about).

A lunar fog-bow was seen at 23^h. Radius to inner edge (about) 38° 5′.

October 5, 1887.—At 2^h fog was beginning to blow across the hill top, and on it a distinct lunar fog-bow was seen, with traces of faint second bow outside it. The following measurements of the inner bow were got :—

Radius to inside edge, ...	35° 4′
,, outside edge, ...	41° 0′

A similar lunar fog-bow was seen at 3^h; there appeared to be a faint trace of red about the outer edge of the inner bow. On both these occasions a faint white patch of light was seen round the shadow of the observer's head, which was probably a lunar glory.

The fog-bow was seen again at 4^h and 5^h, but without any glory. The following measurements were made at 4^h :—

Radius to inside of inner bow, . . .	36° 3′
,, outside of inner bow, . . .	41° 0′

October 15, 1887.—At 14^h double fog-bow and glories observed; no measurements got. At 14^h 10^m, and again at 16^h 10^m, a solar corona was seen, triple each time. The following measurements were got :—

Fig 1.

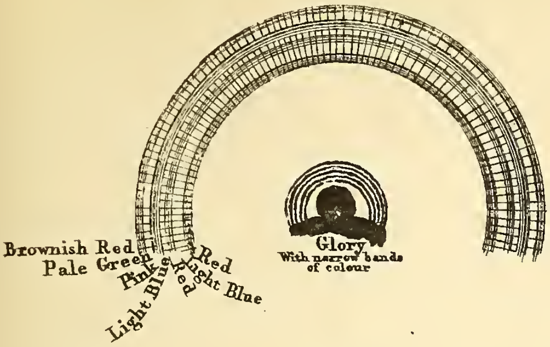


Fig 2.

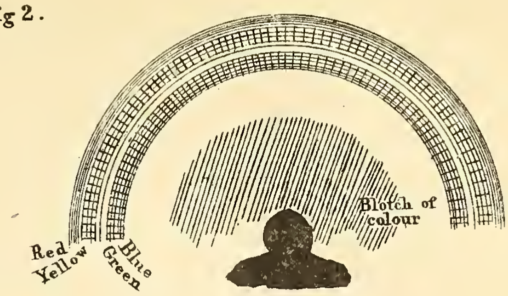


Fig 3.

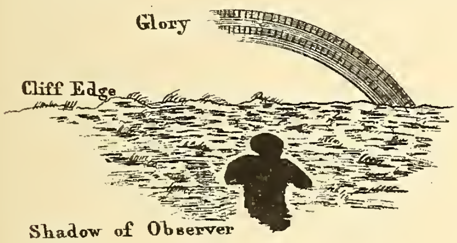


Fig 4.

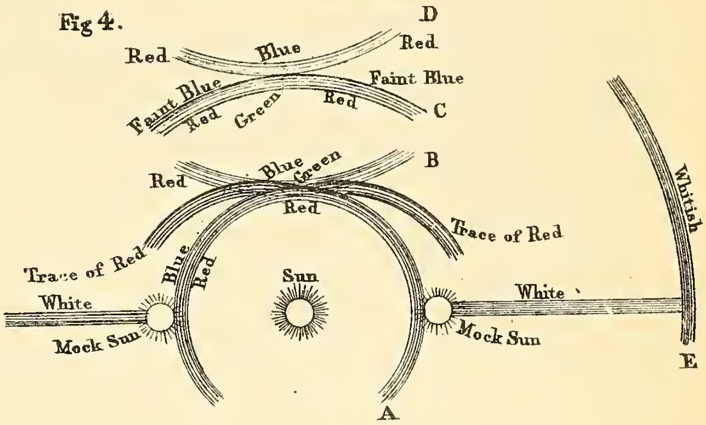


Fig 5.

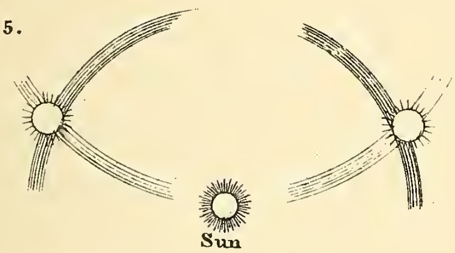


Fig 6.

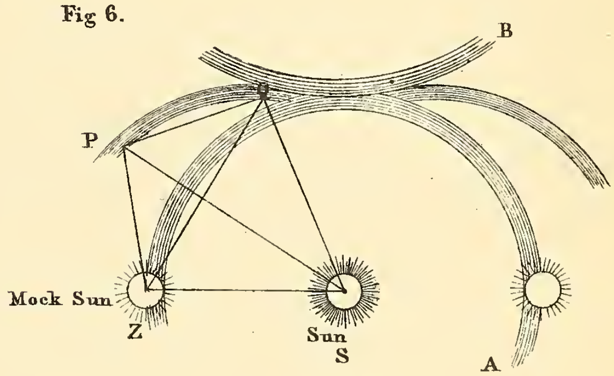


Fig 7.

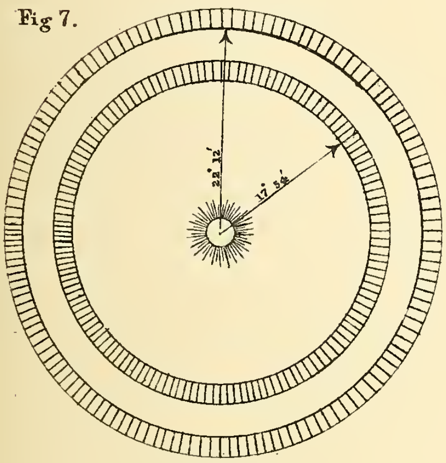


Fig 8.

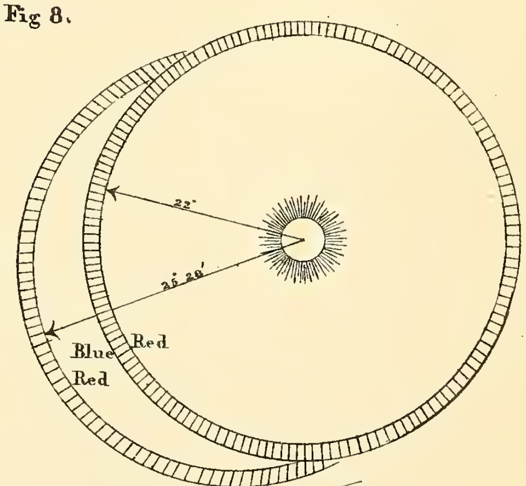


Fig 9.

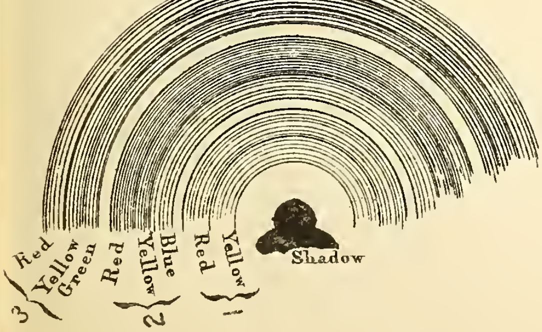
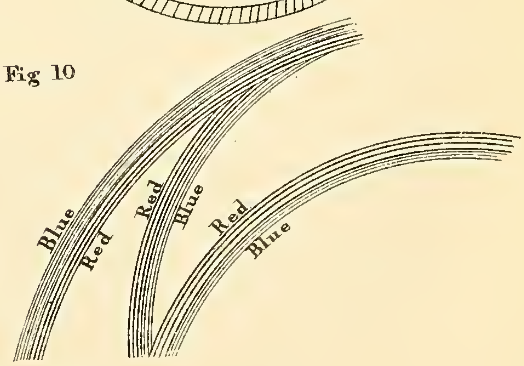


Fig 10



	At 14^h 10^m.	At 16^h 10^m.
Radius of red¹, . .	...	2° 6′
„ red², . .	2° 20½′	3° 35′
„ red³, . .	...	...

Note.—Fig. 10 is a sketch of rainbow seen at Fort-William on 16th August 1887, at about 17^h, by A. Rankin, J. Miller, W. Stewart, and D. M'Kenzie.

Monday, 4th July 1887.

Dr JOHN MURRAY, Vice-President, in the Chair.

The following Communications were read:—

1. Thermal Conductivity of Iron, Copper, and German Silver. By A. C. Mitchell, Esq. Communicated by Professor Tait.

2. On the Probability that a Marriage entered into by a Man of any Age, will be Fruitful. By T. B. Sprague, M.A.

In a paper which I read before the Society in 1879, I gave the results of an investigation I had made with the object of determining the probability, that a man marrying at any age over 40, will, or will not, have children; and I have now extended the same enquiry to men of all ages. The statistics upon which my former conclusions rested, related to 339 marriages entered into by peers and their near relations, above the age of 40, and were extracted from Lodge's Peerage for the year 1871. For all statistical enquiries this work is, in my opinion, greatly preferable to any of the other works that give records of the British Peerage. The principal ground for this opinion is, that Lodge usually gives the dates of birth of the daughters of each family, as well as those of the sons; whereas other Peerages generally omit those dates, and place the names of the daughters (without any dates) after the names of the sons, so that it is impossible to tell whether

they are older or younger than their brothers. In the case of a few families this practice is also adopted in Lodge, presumably by special desire of the head of the family; and it is a matter of regret that this, together with other circumstances to be presently mentioned, diminishes the value of the book for statistical purposes.

I could have greatly increast the above mentioned number of facts, if I had not thought it desirable altogether to exclude the marriages of the persons given under the heading, "Collateral Branches". But a very slight examination of the book, was sufficient to satisfy me that the information as to the collateral branches of each family, was very much less trustworthy than that relating to the immediate family. There appears to be no very precise rule according to which persons are transferred from the portion of the work relating to the immediate family, which is printed in larger type, to the "Collateral Branches", printed in smaller type. When the title has descended to the present holder from his father and grandfather, the name of the peer is given in the first instance, with his date of birth, his date of succession to the title, and full particulars of his marriages. Then follow the names and dates of birth of his children, the dates of their marriages, if any, and the dates of death of any who have died. In the case of the married sons, similar information is given as to their children; but this is very rarely done in the case of the married daughters. After all the usual information has thus been given as to the peer and his descendants, we have the name of the peer's father, with similar information as to himself, his marriages, his children (other than the peer), and their marriages. As in the case of the peer, no information is given as to the children of the married daughters, the sisters of the peer; but full information is given as to the children of the married sons, the brothers of the peer; in other words, we have information as to the nephews and nieces of the peer who trace their descent through the male line.

In some cases we next have the peer's grandfather, with information as to his children, who are the uncles and aunts of the peer; and, as in former cases, we have information as to the children of the uncles, but not as to the children of the aunts; and we thus get particulars as to the first cousins of the peer who trace their descent

through the male line. In other cases, the name of the grandfather is not given, but under the Collateral Branches we have the names of the uncles and aunts of the peer, with particulars as to their children and grandchildren through the male line. In still other cases, when all the uncles and aunts of the peer are dead, the particulars as to them are no longer given; but the children of the uncles (who are cousins of the peer) are placed among the Collateral Branches, and information is given as to the marriages of the males and their children, and as to the marriages and children of more distant male relatives, whose number is sometimes very great.

An examination of a single copy of Lodge's Peerage was sufficient to show that the information as to the collateral branches, lacks the completeness that is necessary for the purposes I had in view. We constantly find it stated there that a particular man is dead, but no date of death is given; or that he is married and has issue, but the date of marriage and the names of his children are not given. The book claims to be corrected by the nobility; and, although this may tend to secure accuracy as regards the immediate family, the information as to the collateral branches is, in many cases, just such as might be given by the head of the family, in correcting the proofs from memory. He has not kept up intimate relations with the numerous younger branches of the family; but, on looking through the proofs, and coming upon the name of a cousin or other more remote relative, he remembers having heard that he is dead, or that he is married and has children; and having no record at hand that will give him the exact date of marriage or death, he contents himself with stating the mere fact of marriage or death, without the date. A comparison of the editions of Lodge for different years, subsequently proved that the information as to the collateral branches is in other respects deficient, and that the marriages and births of children among them are not regularly recorded in the work, from year to year, as they take place; in fact, sometimes a marriage is not recorded for many years after it has taken place; and when it is first mentioned, a long list of children of the marriage is given in addition. The fact, therefore, that a man whose name appears among the collateral branches in Lodge's Peerage for any year, is not stated to have been married,

cannot be accepted as evidence that he is not at that time married; and the fact that a married man is not mentioned as having children, cannot be accepted as evidence that he has none. Another circumstance, which diminishes the value of Lodge's Peerage for statistical investigation, is the practice adopted in some families of giving, among the collateral branches, the names, &c., only of the "last surviving" uncles, aunts, &c., omitting all mention of those who are dead. It is clear that we may fall into very serious errors if we draw conclusions from incomplete information of this kind, and that very careful consideration is necessary to determine what use may safely be made of it.

Such were the reasons which led me, on the former occasion, to reject all the facts relating to the collateral branches; but further consideration showed me that, although the information as to the collateral branches is, on the whole, much less trustworthy than that as to the immediate family, yet we cannot safely lay down the rule, to take the latter and reject the former; for, as we have seen, no strict line can be drawn between the two; the uncles and aunts and cousins being sometimes included in the immediate family, and sometimes among the collateral branches. In fact, we find, on comparing the editions for different years, that uncles and aunts and cousins, who are given in one year's Peerage as members of the immediate family, will, after the lapse of some years, when the peer has died, be transferred to the collateral branches in the new volume. The distinction, therefore, between the collateral branches and the immediate family, is one that cannot be acted upon in practice; and we must seek for some other distinction.

Even among the immediate family, the information given is not always full and trustworthy; and the facts given in Lodge's Peerage for one year, sometimes differ from those given in the edition for another year, or in Foster's work to be presently mentioned. Careful examination soon showed me that the cases where the information is most defective and least trustworthy, are those of recent titles. When a man is created a peer who has been married many years, the information as to his children is often less full, and apparently less accurate, than in the case of peers who inherited their titles; and I therefore came to the conclusion that,

in the present enquiry, it would be desirable to reject every marriage entered into by a commoner who was subsequently created a peer. For the same reason, if a son was married before his father was created a peer, I rejected the son's marriage. Similar considerations apply to the cases where a peer did not succeed to the title in the ordinary way, but establisht his claim to a title that had been dormant for some time, or succeeded a distant relative; and to the cases where a peer was placed upon the roll of peers in consequence of the reversal of an attainder against an ancestor. When a man's name has stood for a long series of years upon the roll of peers, each fact as to his marriage, and as to the births and deaths or marriages of his children, is usually recorded as it takes place, and there is little risk of error or omission; but when a man is created a peer, or succeeds under the exceptional circumstances above mentioned, the facts as to his family are not on record in the same way, and have to be supplied by himself. No doubt in some cases the new peer will give this information with all the desired accuracy, but in a good many cases he will not; and, as it is not possible to say with certainty in which cases the information is complete and exact, and in which it is defective or incorrect, the only safe course is to reject the whole of the cases as clearly liable to error.

In the present investigation I have, as before, taken Lodge's Peerage for 1871 as my starting point; but I have made extensive use also of the new Peerage by Foster, publisht for the first time in 1880, and I think it right to mention that this has supplied a good many dates and other facts which are not given in Lodge. Not only are a great number of additional dates of birth, marriage, and death given—principally among the collateral branches—but in many cases the names and dates of birth of children which are not mentioned at all in Lodge. Foster seems not to have relied on the somewhat questionable assistance of the peers themselves in revising the proof sheets, but to have obtained in many cases documentary evidence in addition to that which the editor of Lodge has used. Foster's Peerage also gives information, omitted by Lodge, as to the children of the married daughters of the peers and of their relatives. It cannot, however, be safely used by itself for ordinary statistical purposes; for it not only omits the dates

of birth of the females of the family, and places the names of the sisters in a family after the names of all the brothers, but it also systematically omits all mention of children who died young; and when children have grown up to maturity and died unmarried, they also are often omitted.

A careful examination of various Peerages has left upon my mind the general impression that too much reliance should not be placed upon individual facts contained in them. There are many sources of error, which are in practice not sufficiently guarded against. Sometimes an obvious misprint is made in the edition for one year, and is repeated without correction in the editions for several successive years. In a few cases there seems good ground for believing Foster's statement, that the information furnisht by members of the peerage, has been intentionally incorrect. Occasionally, though very rarely, the marriage of a peer or his son is mentioned in one Peerage and not in another; and the same is the case with regard to the issue of some marriages. These inaccuracies, however, are not sufficiently numerous to produce any appreciable effect upon the general results of an enquiry of the present kind; and I think that, if proper precautions are adopted, the vital statistics furnisht by the records of the British Peerage, are more complete and trustworthy than we can hope to get in almost any other way. Perhaps better statistics might be got from the records of some of the Widows' Funds which grant benefits to the children, as well as to the widows, of members; but, in the absence of these, I think the Peerage statistics the best we have.

Taking now a general survey of the facts supplied by the Peerage, we see that they relate to different classes of persons, and that all the facts so supplied are not equally trustworthy. We may, I think, assume that, in general, the facts relating to each peer will be the most complete and trustworthy; that those relating to his children during his life will be almost (or quite) as trustworthy; and that the information will become less trustworthy in proportion as the relationship to the peer of the day, is more distant. We have, in the case of every peer included in our list, the necessary information as to his sons, and their sons (if any), also as to his father, his brothers, and their sons (who are the peer's nephews). In

some cases we have also particulars as to the grandfather (being the father's father), the father's brothers, and their sons, who are respectively uncles and cousins to the peer. The existing peer at any time may thus be regarded as the central figure of a group, around whom are arranged his different relations, at a greater or less distance, according as their degree of relationship is more or less remote.

Assuming now that the precautions we have taken have secured that all the facts we extract from the Peerages are equally trustworthy, we have next to consider whether they are all equally suitable for our purpose. Our object is to obtain particulars of a large number of marriages, which may be considered as a fair sample of the whole; and then to ascertain how many of these resulted in the birth of issue, and how many were childless. If, then, we extract all the marriages of the peer and of his above-mentioned relations, will these give us a fair average of cases suitable for the solution of our problem? Or is there anything in the principle on which our selection of facts is made, that renders the marriages we select unsuitable representatives of the general body? One such circumstance becomes obvious at first sight. The peer may be single, or married; and if the latter, he may either have children, or have none. The same is the case with regard to his sons, his grandsons, his brothers, his nephews, his uncles, and his cousins; but we see that his father and his grandfather had at least one son each; each of them, in fact, being included in our list for the very reason that he had a son. It follows that, if we include in our enquiry the information regarding the fathers and the grandfathers of the peers, we shall not obtain trustworthy results; for we shall have an undue proportion of fruitful marriages. If we include in our list the marriages of the peers' fathers, we must, in order to get the proper proportion of childless marriages, include also the marriages of all their contemporaries who were married and had no children; but to attempt this would, I believe, be an impracticable task. If we took only those fathers of peers who were themselves peers, we might perhaps obtain a suitable body of statistics, by taking the marriages of all contemporary peers who married but had no children. In some cases these were succeeded by their brothers, or nephews, or other relatives; and all cases of this kind could probably be traced without any great difficulty; but, in a number of other cases,

such peers have had no male relations to inherit the title, which has therefore become extinct. In order to trace out cases of this kind, it would be necessary to have a complete set of old volumes of the Peerage, and ascertain by an examination of them which titles became extinct each year, in consequence of the peer having died without issue, although he was married. From several points of view this would be a very interesting enquiry, but it was one which I was not in a position to undertake: and if I had desired to do so, I cannot see how it would have been possible to decide how far back the enquiry should extend, so as to include the proper number of holders of extinct titles, but not too many of them. In a certain number of cases, moreover, the peers' fathers were not themselves peers, but were untitled men belonging to a younger branch of the family; and I imagine it would be quite impossible to lay down any principle upon which to determine all the persons who may fairly, for the present purpose, be considered their contemporaries. The difficulty, however, is completely got over by excluding from consideration the marriages of the fathers and grandfathers of our peers, or, speaking more correctly, the marriages from which our peers were descended. I have, therefore, if a peer's father or grandfather was married twice, or three, or four times, excluded from consideration the marriage from which the peer was descended, but have made use of the remaining marriages,—subject to a correction that will be hereafter explained. I have also, in a few cases, rejected all the facts relating to certain peers, when, from very exceptional circumstances, such as residence in a foreign country, the information was obviously incomplete.

Another circumstance may be noted:—The peers themselves are all alive at the time at which our observations begin, and their fathers and grandfathers are all dead; but their sons, their brothers, and their uncles, may be either living or dead. There seems, however, no reason for thinking that the fact that all the peers are living, will affect to any appreciable extent, if at all, the probability of their marriages being fruitful.

As already mentioned, my investigation commenced with Lodge's Peerage for 1871. The principal object was to ascertain what proportion of marriages were fruitful: and as many marriages which are not fruitful at once, become so in later years, it would be com-

paratively useless to include in the observations any existing marriages that had subsisted for less than, say, ten years. I have therefore taken no account of any marriage entered into after the year 1870, these being the latest recorded in the Peerage for 1871; and by examination of Lodge's and Burke's Peerages for 1884, I ascertained in regard to each marriage whether it had been fruitful up to the year 1883 inclusive. Thus every marriage included in my observations, has either been dissolved by death (or divorce), or has subsisted for at least thirteen years.

Proceeding, then, on the principles explained, I obtained information as to the marriages of the peers of 1870 and certain of their male relations; and although, as far as I could see, the facts stated in the Peerage with regard to all of these, were equally trustworthy and suitable for my purpose, I decided, as a matter of precaution, to extract and tabulate separately, the facts relating to the following classes of men:—(1) the peers whose names are given in the Peerage for 1871; (2) their fathers; (3) their grandfathers; (4) their sons; (5) their brothers; (6) their uncles, being the fathers' brothers; and I did not consider it desirable to extend the enquiry to the peers' nephews, or cousins, or more remote relations.

To each marriage was assigned a distinctive number, and the particulars were then written in a book in the following form:—

No.______	Born ______________
Title ____________________________	Married ____________ Age __________
Name ____________________________	Widower ______________
Wife born ______________ Age ___	Died ____________

Children.				
S. or D.	No.	*Born.*	*Died.*	*Married.*
	1	———	———	———
	2	———	———	———
	3	———	———	———
	⋮	⋮	⋮	⋮

2nd Wife born ________ Age ___	2nd Married ________ Age ___ 2nd Widower ________ Died ________

Children.				
S. or D.	No.	*Born.*	*Died.*	*Married.*
	1	———	———	———
	2	———	———	———
	3	———	———	———
	⋮	⋮	⋮	⋮

It is clear that from materials of this kind we can obtain much information, not only as to the probability of a marriage being fruitful, but as to the number of children to a marriage, the probability that they will be sons or daughters, the length of time that elapses between marriage and the birth of the first child, and the intervals between the births of successive children, &c. If the age of the wife could also have been ascertained in every case, much interesting information could have been obtained as to the probability of a wife of any age having issue; but it is only in exceptional cases that the age of the wife can be determined—in fact, only when she is a daughter belonging to one of the peerage families. On the present occasion I confine myself to the consideration of the one point as to the probability of the marriage being fruitful. If any child has been born of a marriage, I have considered it as a fruitful marriage, although all the children may have died young in the lifetime of the father.

The number of marriages which are included in my observations are, of peers 427; of their sons, 199; of their brothers, 511; of their uncles, 384. On grouping these according to age at marriage, I obtained the figures shown in the following table (A):—

A comparison of these figures shows us that the percentages for the brothers and uncles agree very well together, and that the proportion of childless marriages is very much greater among these two classes taken together, than among the peers and their sons. This appears more clearly when the figures are combined as shown in table (B):—

TABLE A.—*Marriages of the Peers of* 1870, *and of their Sons, Brothers, and Uncles.*

Age at Marriage.	Peers of 1870.			Sons.			Brothers.			Uncles.		
	Marriages.	Of which were Childless.		Marriages.	Of which were Childless.		Marriages.	Of which were Childless.		Marriages.	Of which were Childless.	
		Number.	%		Number.	%		Number.	%		Number.	%
16 to 29	248	30	12·1	134	23	17·2	278	64	23·0	193	44	22·8
30 ,, 39	108	16	14·8	63	11	17·5	170	47	27·6	121	29	24·0
40 ,, 49	39	10	25·6	2	1	50·0	43	17	39·5	47	13	27·7
50 ,, 59	19	10	52·6	...	...	...	15	11	73·3	17	9	52·9
60 and upwards	13	9	69·2	...	...	...	5	4	80·0	6	6	100·0
Total	427	75	17·6	199	35	17·6	511	143	28·0	384	101	26·3

TABLE B, *containing the facts of Table* A, *arranged in two classes.*

Age at Marriage.	Peers and Sons.			Brothers and Uncles.		
	Marriages.	Of which were Childless.		Marriages.	Of which were Childless.	
		Number.	Per-centage.		Number.	Per-centage.
16 to 29	382	53	13·9	471	108	22·9
30 ,, 39	171	27	15·8	291	76	26·1
40 ,, 49	41	11	26·8	90	30	33·3
50 ,, 59	19	10	52·6	32	20	62·5
60 and upwards	13	9	69·2	11	10	90·9
Total	626	110	17·6	895	244	27·3

The percentages here run so regularly, and the differences between those relating to the two sets of observations are so great, that we are forced to the conclusion that the differences cannot be accidental, but that there must be something in the manner of compiling our statistics, that necessarily causes the percentage of childless marriages to be greater at all ages among the brothers and uncles, than among the peers and their sons. It was not long before I discovered a cause that accounted for a great deal of the difference;—in fact, I found that my class of peers' brothers, in-

cluded a certain number of their elder brothers who had died without male issue. In some cases, these were themselves peers, and were succeeded by their younger brothers. In other cases, they were the eldest sons or nephews of peers, and would have succeeded to the title if they had lived; but they died before the succession to the title opened to them, so that the next brother succeeded. If these men had left sons who succeeded to the title, they would have appeared in my classification as peers' fathers; but in consequence of their having no sons, they are placed in my class of peers' brothers: and this circumstance causes that class to include an undue proportion of men who died without issue. The same remark applies to the class of peers' uncles, which includes a number of elder brothers of the peers' fathers, who died without leaving issue. The obvious way of eliminating this source of error is to exclude from observation all such elder brothers of the peers, and all uncles who were elder brothers of the fathers; and to consider only the younger brothers of the peers, and the younger brothers of the fathers, who, for brevity, may be called "younger uncles". When this was done, I got the figures shown in the following table:—

TABLE C.—*Marriages of the Younger Brothers of the Peers, and of their Fathers' Younger Brothers.*

Age at Marriage.	Y'nger Brothers.			Younger Uncles.			Total.		
	Marriages.	Of which were Childless. Number.	Of which were Childless. °/ₒ	Marriages.	Of which were Childless. Number.	Of which were Childless. °/ₒ	Marriages.	Of which were Childless. Number.	Of which were Childless. °/ₒ
16 to 29	253	47	18·6	168	28	16·7	421	75	17·8
30 ,, 39	161	40	24·8	115	26	22·6	276	66	23·9
40 ,, 49	43	17	39·5	46	13	28·3	89	30	33·7
50 ,, 59	14	10	71·4	11	5	45·5	25	15	60·0
60 and upwards	5	4	80·0	5	5	100·0	10	9	90·0
Total	476	118	24·8	345	77	22·3	821	195	23·8

Comparing the figures for the younger brothers and the younger uncles, we see that, excluding the very few marriages at 60 and

upwards, the proportion of childless marriages is at all ages greater among the brothers than among the uncles. Similarly, referring back to Table A, we see that the proportion of childless marriages among the sons of peers, is at all ages greater than among the peers themselves. In both cases, therefore, the proportion of childless marriages is greater in the younger generation. This is quite consistent with the proposition laid down by many writers, that there is a constant tendency in the families of the peerage, and of ruling classes generally, to die out; and it suggests a tempting field of enquiry. No doubt many interesting and valuable results would be obtained, if the experience of several successive generations of the peerage families, were investigated with regard to both their mortality and their fecundity; but, although the figures above given, as far as they go, certainly support the idea that a gradual deterioration is taking place in the peerage families, the figures involved are too small to be accepted as giving any conclusive evidence on the point.

It is now time to compare the results thus far obtained, with those given in my former paper, which related only to men over 40 at marriage. For this purpose, my former figures are entered in the following table, alongside of those now obtained for (1) peers and their sons, (2) their younger brothers and their fathers' younger brothers.

TABLE D.

Age at Marriage.	Peers and their Sons.			Y'nger Brothers and Younger Uncles.			Total.			Former Observations.		
	Marriages.	Of which were Childless. Number.	Of which were Childless. %	Marriages.	Of which were Childless. Number.	Of which were Childless. %	Marriages.	Of which were Childless. Number.	Of which were Childless. %	Marriages.	Of which were Childless. Number.	Of which were Childless. %
16 to 29	382	53	13·9	421	75	17·8	803	128	15·9	...	...	...
30 ,, 39	171	27	15·8	276	66	23·9	447	93	20·8	...	...	...
40 ,, 49	41	11	26·8	89	30	33·7	130	41	31·5	196	56	28·6
50 ,, 59	19	10	52·6	25	15	60·0	44	25	56·8	92	42	45·7
60 and upwards	13	9	69·2	10	9	90·0	23	18	78·3	51	39	76·5
All Ages	626	110	17·6	821	195	23·8	1447	305	21·1	...	...	...
40 and upwards	73	30	41·1	124	54	43·5	197	84	42·6	339	137	40·4

The statistics I formerly made use of, differed from the present ones in two respects :—

1. They included a number of fathers of peers, all of whom, of course (as pointed out in the early part of this paper), left sons to inherit the title. The exclusion of these fathers from the present observations, has a tendency to increase the proportion of childless marriages.

2. They included a number of elder brothers of the peers and elder brothers of their fathers, these being men whose male issue failed, that is to say, men who had either no children at all, or only daughters, or if they had a son or sons, their male issue had all died, and the title had therefore descended to a younger branch of the family. The exclusion of these men has a tendency to reduce the proportion of childless marriages.

These two tendencies are therefore in opposite directions ; and, as it happens, they to a great extent neutralize each other, the aggregate result being that my present observations show 197 marriages of men over 40, of which 84, or 42·6 per cent, were childless ; against 339 marriages formerly considered, of which 137, or 40·4 per cent, were childless.*

Comparing now the figures shown in Table D with those in Table B, we see that the exclusion of the elder brothers, and of the fathers' elder brothers, has had the effect of reducing the proportion of childless marriages at all ages, with a trifling exception at the ages 40–49 ; the reduction being particularly noticeable at the ages under 30, at which more than half the total marriages took place. The new percentages given in Table D for the younger brothers and the younger uncles, are much nearer to those for the peers and their sons, than are our original figures in Table B, which related to all the brothers and all the uncles ; but we see that at all ages, the percentages for the younger brothers and the younger uncles, are still greater than the corresponding ones for the peers and their sons ;—in other words, that at all ages there is a less proportion of childless marriages among the peers and their sons, than there is among the younger brothers of the peers, and their fathers' younger

* Including, as hereafter explained, certain marriages of the fathers and grandfathers of peers, the total number of marriages of men over 40 included in the present observations, is 259, of which 118, or 45·5 per cent, were childless.

brothers. An explanation of this fact soon suggests itself. Comparing the position of an unmarried peer with that of his brothers, it seems likely that the former, being in possession of the estates which go along with the title, will feel more free to follow his personal inclinations in the selection of a wife, than will be the case with his younger brothers; and the same remark applies to the peer's eldest son, and to the eldest son of this eldest son, as compared with their younger brothers. In other words, they may be expected as a rule to marry wives who are personally attractive, being young and of healthy constitutions; while the younger brothers may more frequently marry for money, the wife being in many cases an heiress who, from her age, or from being herself an only child, is less likely to have children. For the purpose of testing this supposition, I next examined all the 626 marriages of peers and their sons, and noted which of them were entered into by a man who was at the time either a peer, or the heir-apparent of a peer, or the eldest son of an heir-apparent; and the results are shown in the following table:—

TABLE E.—*Marriages of Peers and of their Sons, distinguishing those which were entered into by a Man who was either a Peer or an Heir-Apparent.*

Age at Marriage.	Peers.						Sons of Peers.					
	Married as Peer or Heir-Apparent.			Remainder.			Married as Heir-Apparent.			Remainder.		
	Marriages.	Of which were Childless.		Marriages.	Of which were Childless.		Marriages.	Of which were Childless.		Marriages.	Of which were Childless.	
		Number.	%		Number.	%		Number.	%		Number.	%
19 to 29	202	25	12·4	46	5	10·9	70	10	14·3	64	13	20·3
30 ,, 39	89	13	14·6	19	3	15·8	30	4	13·3	33	7	21·2
40 ,, 49	32	8	25·0	7	2	28·6	1	1	100·0	1	0	0·0
50 ,, 59	17	9	52·9	2	1	50·0	..	..	..	..	..	..
60 and upwards	12	8	66·7	1	1	100·0	..	..	..	..	..	..
Total	352	63	17·9	75	12	16·0	101	15	14·9	98	20	20·4

As the numbers we are now dealing with are smaller than before, the results do not proceed with so much regularity as those we formerly obtained. The figures relating to the sons of peers, however, strongly support the above views; as a much larger proportion of the younger sons are childless, than is the case with those sons who married as heir-apparent. The same is not the case, with the peers; in fact, if we take those peers who married under 40, the figures in the two classes are:—

	Marriages.	Of which were Childless.	Percentage.
Married as Heir-apparent,	291	38	13·1
Did not so marry,	65	8	12·3

the results being thus practically identical. One reason at once suggests itself why the difference between the two classes should be much less among the peers than among the sons; namely, that among the peers who did not marry as peer or heir-apparent, a large proportion may at the time of their marriage have had, from special circumstances, a practical certainty of succeeding to the title; for instance, through being heir-presumptive to an elderly unmarried peer; but I have not at present attempted to follow up this idea.

Combining the peers and sons of peers, we get the following figures:—

TABLE F.—*Marriages of Peers and their Sons (Combined), distinguishing those which were entered into by a Man who was Peer or Heir-Apparent.*

Age at Marriage.	Peers and Sons of Peers.					
	Married as Peer or Heir-Apparent.			Did not so marry.		
	Marriages.	Of which were Childless.		Marriages.	Of which were Childless.	
		Number.	Per-centage.		Number.	Per-centage.
19 to 29	272	35	12·9	110	18	16·4
30 ,, 39	119	17	14·3	52	10	19·2
40 ,, 49	33	9	27·3	8	2	25·0
50 ,, 59	17	9	52·9	2	1	50·0
60 and upwards	12	8	66·7	1	1	100·0
Total	453	78	17·2	173	32	18·5

A study of the results thus far obtained, satisfied me that the most satisfactory course would be to arrange my facts into two classes, the first relating to those men who married as peer or heir-apparent, and the second to those who did not. For business purposes, we of course prefer to err, if at all, on the side of safety; for instance, in calculating the risk attaching to an insurance against the birth of issue, to take the probability of issue rather above than below the truth, and thus to make use of that body of facts which gives the smallest probability of failure of issue.

In order to increase the number of the facts, especially at the higher ages, I have thought it desirable to take into account, as far as practicable, those marriages of the fathers and grandfathers of peers, from which the peers were not descended. There were, in all, 110 fathers and grandfathers who were more than once married. In the case of 61, the peer was descended from the first marriage, and in the remaining 49 from the second marriage. As already mentioned, these 61 first marriages, of course, have to be excluded. We must also exclude the 49 first marriages, corresponding to the 49 second marriages from which the peers were descended; for it is obvious that these 49 marriages will include an unduly large number of cases where there was either no issue or no male issue. We have thus to reject all the first marriages; and, of the second marriages, we can only make use of the 61 that correspond to the 61 first marriages from which the peers were descended. We have also 13 third marriages, and one fourth marriage, making in all 75 marriages of fathers and grandfathers, to be taken account of. In 62 of these, the man married when he was peer or heir-apparent; and in 13, he did not so marry.

Collecting our results, we now obtain the figures in the following tables G and H, where it will be observed that, at each decennial group of ages, the percentage of childless marriages is considerably greater among those who did not marry as peer or heir-apparent, than among those who did.

In order that our results may be practically useful for professional purposes, as, for instance, in the calculation of the values of the interests of the various heirs in disentail proceedings, it is necessary to graduate the probabilities. This I have done by the graphic

TABLE G.—*Marriages of Men who married as Peer or Heir-Apparent.*

Age at Marriage.	Peers.		Sons.		Fathers and Grandfathers.		Total.		
	Marriages.	Of which were Childless.	Marriages.	Of which were Childless.	Marriages.	Of which were Childless.	Marriages.	Of which were Childless. Number.	Of which were Childless. °/o
19 to 29	202	25	70	10	2	0	274	35	12·8
30 ,, 39	89	13	30	4	8	1	127	18	14·2
40 ,, 49	32	8	1	1	15	6	48	15	31·3
50 ,, 59	17	9	..	..	17	5	34	14	41·2
60 and upwards	12	8	..	..	20	18	32	26	81·3
Total	352	63	101	15	62	30	515	108	21·0

TABLE H.—*Marriages of Men who did not marry as Peer or Heir-Apparent.*

Age at Marriage.	Peers.		Younger Sons.		Younger Brothers.		Uncles, (Fathers' Younger Brothers).		Fathers and Grand-fathers.		Total.		
	Marriages.	Childless.	Marriages.	Childless.	Marriages.	Childless.	Marriages.	Childless.	Marriages.	Childless.	Marriages.	Of which were Childless. Number.	Of which were Childless. °/o
16 to 29	46	5	64	13	253	47	168	28	1	0	532	93	17·5
30 ,, 39	19	3	33	7	161	40	115	26	2	0	330	76	23·0
40 ,, 49	7	2	1	0	43	17	46	13	6	3	103	35	34·0
50 ,, 59	2	1	..	..	14	10	11	5	4	2	31	18	58·1
60 &c.	1	1	..	..	5	4	5	5	..	..	11	10	90·9
Total	75	12	98	20	476	118	345	77	13	5	1007	232	23·0

method, and the following tables show, for each of the two classes of men mentioned above—(1) the number of marriages at each age, contained in our observations, and the number of them which were childless; and (2) the graduated probability that a marriage entered into at any age, will be childless.

TABLE I.—*Men married as Peer or Heir-Apparent. Adjusted probability that the Marriage will be childless.*

Age at Marriage.	Number of Marriages.	Of which were Childless	Probability that the marriage will be Childless.	Age at Marriage.	Number of Marriages.	Of which were Childless	Probability that the marriage will be Childless.	Age at Marriage.	Number of Marriages.	Of which were Childless.	Probability that the marriage will be Childless.
19	1	...	·121	41	3	1	·230	63	1	...1	·725
20	9	2	·122	42	6	1	·245	64	1	4	·761
21	39	2	·123	43	5	2	·260	65	5	2	·794
22	31	4	·124	44	1	...	·275	66	2	1	·822
23	35	6	·125	45	4	1	·291	67	1		·846
24	25	4	·126	46	4	...	·307	68	...	...	·866
25	41	4	·127	47	3	2	·323	69	1	1	·883
26	31	4	·128	48	5	1	·339	70	1	1	·898
27	20	3	·129	49	4	2	·355	71	1	1	·912
28	26	4	·130	50	2	...	·372	72	1	1	·925
29	16	2	·131	51	4	1	·389	73	1	1	·937
30	24	3	·133	52	3	...	·407	74	1	1	·948
31	14	...	·135	53	3	1	·426	75	...	...	·958
32	15	2	·138	54	4	2	·447	76	1	1	·967
33	12	...	·142	55	1	1	·470	77	...	...	·975
34	17	3	·148	56	6	3	·495	78	1	1	·982
35	9	1	·156	57	5	2	·522	79	1	1	·988
36	10	2	·166	58	6	4	·551	80	...	...	·993
37	12	3	·177	59	...	...	·582	81	...	...	·997
38	7	2	·189	60	9	6	·615	82	...	...	1·000
39	7	2	·202	61	1	...	·650				
40	13	5	·216	62	3	3	·687		515	108	

TABLE J.—*Men who did not marry as Peer or Heir-Apparent. Adjusted probability that the Marriage will be childless.*

Age at Marriage.	Number of Marriages.	Of which were Childless.	Probability that the marriage will be Childless.	Age at Marriage.	Number of Marriages.	Of which were Childless	Probability that the marriage will be Childless.	Age at Marriage.	Number of Marriages.	Of which were Childless.	Probability that the marriage will be Childless.
16	1	...	·138	38	16	3	·279	60	5	4	·852
17	...	...	·139	39	10	4	·288	61	1	1	·874
18	3	1	·140	40	11	4	·297	62	...	...	·892
19	3	...	·142	41	15	1	·307	63	1	1	·907
20	4	...	·144	42	13	7	·317	64	1	1	·920
21	39	6	·147	43	11	2	·327	65	1	1	·931
22	36	4	·151	44	12	6	·337	66	1	1	·941
23	45	4	·156	45	9	3	·347	67	1	1	·950
24	68	11	·162	46	9	3	·358	68	...	...	·958
25	59	11	·169	47	10	5	·370	69	...	...	·965
26	64	11	·177	48	9	2	·383	70	...	...	·971
27	82	19	·185	49	4	2	·398	71	...	...	·976
28	75	13	·193	50	5	2	·415	72	...	...	·980
29	53	13	·201	51	2	1	·433	73	...	...	·984
30	52	8	·209	52	4	1	·466	74	...	...	·987
31	46	12	·217	53	3	3	·514	75	...	...	·990
32	50	7	·225	54	5	1	·574	76	...	...	·993
33	42	12	·234	55	5	3	·644	77	...	...	·995
34	28	6	·243	56	3	3	·703	78	...	...	·997
35	28	6	·252	57	1	1	·752	79	...	...	·999
36	30	13	·261	58	2	2	·792	80	...	...	1·000
37	28	5	·270	59	1	1	·825				
									1007	232	

Simple inspection of the graduated probabilities shows that they proceed with sufficient regularity. They will also satisfy the other criterion of a good graduation, if the number of childless marriages, as calculated from them, does not differ much from the actual number. A comparison of this kind is made in the following table:—

TABLE K.—*Comparison of the actual and the calculated Numbers of Childless Marriages in Quinquennial Groups of Ages.*

Ages at Marriage.	Men who married as Peer or Heir-Apparent.			Men who did not so marry.		
	Number of Marriages.	Childless Marriages.		Number of Marriages.	Childless Marriages.	
		Actual.	Calculated.		Actual.	Calculated.
16–24	140	18	17·5	199	26	30·6
25–29	134	17	17·1	333	67	61·7
30–34	82	8	11·4	218	45	48·8
35–39	45	10	7·9	112	31	29·9
40–44	28	9	6·6	62	20	19·6
45–49	20	6	6·5	41	15	15·0
50–54	16	4	6·5	19	8	9·3
55–59	18	10	9·4	12	10	8·5
60–64	15	10	10·5	8	7	7·0
65–69	9	8	7·3	3	3	2·8
70–74	5	5	4·5	...	...	...
75–79	3	3	3·0	...	...	...
	515	108	108·2	1007	232	233·2

In conclusion it may be useful to remind my readers that I have considered a marriage to be fruitful, if any child has been born, even although all the children born of the marriage may have died young.

3. On the Nephridia of *Hirudo medicinalis*. By Dr A. B. Griffiths, F.R.S.E., F.C.S. (London and Paris); Principal, and Lecturer on Chemistry and Biology, School of Science, Lincoln.

The nephridia of *Hirudo medicinalis,* as is well known to biologists, are in pairs extending from the second to the eighteenth segments (somites). Each nephridium consists of a much convoluted cellular tube. The cells of the tube are perforated by small

ducts. The nephridia ("segmental organs") open externally on the ventral side of the body.

In *Lumbricus* the nephridium communicates internally by a wide funnel-shaped aperture (which is ciliated) with the perivisceral cavity, but in *Hirudo* it opens internally by *a* "cauliflower-headed" portion (the analogue of the funnel-shaped aperture of *Lumbricus*) into the perinephrostomial sinus. Each nephridium consists of five principal parts—(1) posterior lobe, (2) anterior lobe, (3) apical lobe, (4) the testis lobe, (5) the vesicle, with its duct, which opens externally.

The nephridia of *Hirudo* are covered by a pigmented connective tissue. These pigments are no doubt the histohæmatin of Dr C. A. MacMunn,* for he says in another paper—"I have found that throughout the whole animal kingdom, in each tissue and organ, there are present colouring matters" (*Proc. Birmingham Philosophical Soc.*, vol. v. part 1, p. 211). I have shown in my paper † the presence of uric acid in the nephridia of the Oligochæta.

In the present paper details are given of the extraction of uric acid from the nephridia or "segment organs" of the Hirudinea. The species taken for investigation was *Hirudo medicinalis.* The secretions of the nephridia were obtained from a considerable number of freshly killed leeches, and examined by similar chemical and microscopical reactions as I have employed in my paper already cited, and in one "On the Nephridia and Liver of *Patella vulgata*,"‡ read before the Royal Society of London, June 16, 1887. It may be useful to give the details of the processes. The secretions were examined by two separate methods—

(*a*) The clear liquid from the nephridia was treated with a hot dilute solution of sodium hydrate, and then, on the addition of hydrochloric acid, a slight flaky precipitate is obtained, after some hours' standing. These flakes were seen, under the microscope, to consist of various crystalline forms. On treating these crystals with nitric acid, and then gently heating with ammonia, the reddish-purple murexide is produced, which crystallises in four-sided prisms. The secretion alone, when treated with alcohol, deposits rhombic

* *Proc. Roy. Soc.*, No. 240, 1886, and *Philos. Trans.*, 1886.

† "Researches on the Problematical Organs of the Invertebrata, especially those of the Cephalopoda, Gasteropoda, Lamellibranchiata, Crustacea, Insecta, and Oligochæta," read before the Royal Society, Edinburgh, May 16, 1887.

‡ *Proc. Roy. Soc.*, vol. xlii. (1887), p. 392.

crystals. According to the test of Dr Schiff (*Ann. Chem. Pharm.*, vol. cix. p. 67) for uric acid, these crystals were dissolved in a drop or two of sodium carbonate solution, and then poured upon a piece of filter-paper moistened with a solution of silver nitrate: a dark brown stain of metallic silver was obtained.

(*b*) Another process was used as follows:—To the liquid secreted by the nephridia of *Hirudo* boiling water (distilled) was added, and then evaporated carefully to dryness. The residue so obtained was treated with absolute alcohol, and filtered. Boiling water was poured upon the residue, and an excess of acetic acid added to the filtrate (aqueous). After standing several hours, crystals of uric acid made their appearance, and were easily recognised by the chemical and microscopical tests mentioned above.

Further than this, the presence of sodium was found in the secretions of the nephridia of *Hirudo.* It may be that the uric acid is in combination with sodium as a sodium urate.

From this investigation the nephridium of the Hirudinea functions as a true kidney.

Renal Organs of the Hirudinea aud Oligochæta and their Constituents.

	Hirudinea.	Oligochæta.
Uric acid, ...	present.	present.
Sodium, ...	,,	absent.
Urea, ...	absent.	,,
Guanin, ...	,,	,,
Calcium phosphate, . . .	,,	,,

The minute structure of the excretory organs in general of the Oligochæta, especially those of *Lumbricus terrestris,* have been worked out by Dr E. Claparede, and detailed in his "Histologische Untersuchungen über den Regenwurm" (*Zeitschrift für Wissenschaftliche Zoologie*, vol. xix.), and also by Professor Gegenbaur, "Ueber die sogenannten Respirationsorgane des Regenwurms" (*Zeitsch. W. Zool.*, vol. iv.).

4. On Degenerated Specimens of *Tulipa sylvestris.* By Mrs A. B. Griffiths. Communicated by Dr A. B. Griffiths, F.R.S.E., &c.

This note describes a peculiar yet interesting form of degenerated *Tulipa sylvestris.* Last June (1886) the tulip bulbs were removed (after growing and flowering in very rich soil), and set in the December of the same year in a soil of poor quality. The plants did not flower until the present June.

These degenerated forms have the usual erect scapes found in the genus *Tulipa;* but the inflorescence has the form of well-marked *umbels.* The flowers were small (fig. 1), and consisted of the usual parts of *Tulipa sylvestris*, namely, a yellow polyphyllous and inferior perianth (with six perianth leaves). The stamens were hexandrous and hypogynous; the pistil syncarpous and superior; the placentation was axile, and the ovary divided into three cells. The unusual *inflorescence* and the peculiar shape of the petaloid segments of the perianth were so unlike *Tulipa* that the present investigation was undertaken as a point of some interest to botanists.

Fig. 1.—Flower of degenerated Tulip (natural size).

The bulbs, leaves, and peduncle had developed the strong odour of the oil of onions or garlic. The essential oil was extracted by distilling the peduncle, leaves, and bulbs separately with water. The essential oil was then obtained from this, distillated by repeated fractionation and rectification over potassium.

This purified essential oil had a boiling point of 141° C., and yielded upon analysis a percentage composition similar to that of *allyl sulphide*, as is shown by the following figures :—

Theory.	Found. I.	Found. II.	Found. III.
$C_6 = 72 = 63 \cdot 15$ %	63·24 %	63·17 %	63·21 %
$H_{10} = 10 = 8 \cdot 77$,,	8·69 ,,	8·72 ,,	8·73 ,,
$S = 32 = 28 \cdot 07$,,	...	...	28·06 ,,
114			

(I wish, here, to thank my husband [Dr Griffiths] for the above determinations.)

Then, again, on the addition of an alcoholic solution of silver nitrate to the purified oil, a white precipitate is thrown down. This precipitate, when allowed to crystallise from hot alcohol, separates in white needle-shaped crystals (fig. 2), as observed by low power under the microscope. This crystalline precipitate is no doubt the silver nitrate compound of allyl sulphide,

$$(C_3H_5)_2S(AgNO_3)_2$$

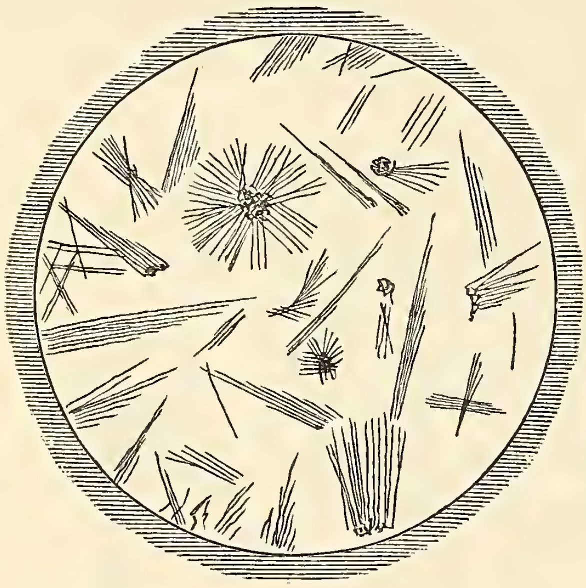


FIG. 2.—Needle-shaped crystals of $(C_3H_5)_2S(AgNO_3)_2$.

The bulbs, peduncles, and leaves of this degenerated form of *Tulipa sylvestris* all yielded the same essential oil as above. No *oil of mustard* was detected in the above distillate as is usual (according to Dr Pless, *Ann. Chem. Pharm.*, vol. lviii. p. 36) during the distillation of some plants containing either of the two isomeric oils of garlic and onion.

It is well known that many species of *Allium* yield, on distillation, allyl sulphide; also several genera of the Cruciferæ yield the same chemical compound under similar treatment. Amongst these, *Iberis amara, Alliaria officinalis, Thlaspi arvense* (Wertheim, *Ann. Chem. Pharm.*, vol. li. p. 289, and vol. liv. p. 297). But the essential oil, which is capable of yielding allyl sulphide, has not been found in the genus *Tulipa*, although it, like *Allium*, belongs to the Liliaceæ.

It has been shown by Professor Alphonse de Candolle (*Origin of Cultivated Plants*, p. 66) that the *onion* (*Allium cepa*) is a very old form of the vegetable kingdom. He says:—"Its [onion] cultivation in Southern Asia and eastern region of the Mediterranean dates from a very early epoch." Therefore, if the *cultivated* onion is a very ancient variety, what must be the age of its wild ancestor? From the facts detailed in this paper, is it not likely that the wild

tulip (*Tulipa sylvestris*) is a descendent from the genus *Allium*, and that by a change in the surroundings and other causes these particular plants have retrograded in certain points (namely, the production of an oil identical with oil of onions, and the inflorescence similar to the onion family) to the original ancestral type. To conclude, in the words of Darwin, "As we have no written pedigrees, we are forced to trace community of descent by resemblances of any kind" (*Origin of Species*).

5. The Luminous Organs of *Nyctiphanes norvegica*, Sars. By J. T. Cunningham, B.A., and Rupert Vallentin.

The fact that light was emitted in the dark by a Schizopodous shrimp was first noticed by J. Vaughan Thomson, who, in his *Zoological Researches*, published about 1820, describes a species which he observed to be luminous under the name *Noctiluca*. He mentions the presence of scattered spots of red pigment in the animal, but was quite unaware that the production of light was confined to certain definite organs enveloped by this pigment—was indeed unaware of the existence of the organs which form the subject of this paper. Later on, when the family of the Euphausiidæ was defined, various accounts were given of certain complicated organs of spherical shape in the animals belonging to the family. These organs were generally considered to be organs of vision, and were called accessory eyes. The most detailed account of the structure of these supposed eyes was given by Claus,* in 1863. The fact that the Euphausiidæ were luminous, was however known to the naturalists on board the "Challenger," and a paragraph is devoted to the subject in the Narrative of the Cruise of that ship (vol. i. p. 743). It is there stated that the phosphorescent light emitted by the species of the Euphausiidæ was frequently under observation during the voyage. It was found that when one of the animals newly caught was taken up by a pair of forceps, a pair of bright phosphorescent spots was observed immediately behind the eyes, other two pairs on the trunk, and four other spots along the median ventral line of the

* "Ueber einige Schizopoden und niedere Malakostraken Messina's," *Zeits. f. Wiss. Zool.*, Bd. xiii.

tail: that these could all be seen easily by the unaided eye: that the pair close to the eyes was first and most brilliantly illuminated, and then the light, which was bluish white, spread to the other organs in the trunk and tail: that after a brilliant flash had been emitted, the organs glowed for some time with a dull light: that the light was given out at will by the animal, and usually but not always on irritation: that subsequent flashes became less and less bright till the animal appeared to lose the power of emitting light: that if the organs were removed by the forceps, the points of the latter glowed brightly for some time, and when the animal was dying the whole body was frequently illuminated by a diffused light: that the phosphorescent organs appeared under the microscope as pale red spots with a central clear lenticular body, and the light came from the red pigment. It is further mentioned that in August 1880 Mr John Murray observed at night, on the surface of the sea in the Færöe Channel, large patches and long streaks of apparently milky-white water, and the tow-nets caught in these places immense numbers of *Nyctiphanes norvegica*, M. Sars, about half the size of the adult, and the peculiar appearance of the water seemed to be due to the diffused light emitted from the phosphorescent organs of this species.

That the organs, erroneously called accessory eyes, were in reality luminous or "phosphorescent" organs was definitely asserted by Prof. G. C. Sars, in his Report on the Schizopoda of the "Challenger." He gives an account of their structure, but does not discuss very fully the questions concerning the method and mechanism of the production of light. Mr W. Patten* only last year has again attempted to maintain that these organs are eyes and not luminous organs; but in view of the evidence of Sars and others and of our own experiments, Mr Patten's arguments need no special refutation; they are indeed contradicted sufficiently by the postscript which is added to his paper by Drs Giesbrecht and Paul Meyer, who personally observed the luminosity of Euphausia.

We have studied these organs and their function in *Nyctiphanes norvegica*, Sars, which is abundant in certain deep places in the Clyde sea-area. We obtained it in 95 fathoms off Brodick Bay, by

* "Eyes of Molluscs and Arthropods," *Mitt. aus der Zool. Stat. zu Neapel*, 1886.

means of the shrimp trawl worked a little above the bottom. We kept the animals alive in the "Ark" at Millport, and there made observations and experiments on the luminous organs in the fresh state. Their structure was investigated by Mr Vallentin, by means of the preserved material, at the Granton Laboratory of the Scottish Marine Station.

The distribution of the organs in the body has been completely described by Sars. There are three pairs and four median single. The first pair are in the eye peduncles immediately behind and dorsal to the eyes. The 2nd pair are in the basal joints of the 2nd pair of thoracic appendages, and each of these is on the internal side looking towards the median line of the body. The 3rd pair are in the basal joints of the 7th pair of thoracic appendages; each is on the external side, and looks backwards and outwards. Of the four unpaired organs, one is in the middle of the ventral surface of each of the first four abdominal somites.

All the organs except the pair in the eye peduncles are perfectly similar in structure. The organ forms a spherical body lying immediately beneath the epidermis, and almost entirely independent of the surrounding tissues. The envelope of the posterior half of the organ is formed by a hemispherical cup of considerable thickness, of laminated or stratified structure, appearing fibrous in section, and non-cellular. Internal to this is a cellular layer, consisting of large cubical cells to the exterior, and smaller cells at the internal surface. The hollow of the hemisphere within this layer is entirely filled with a non-nucleated fibrous mass, the fibres or rods being externally perpendicular to the surface of the cellular layer, but in the centre crossing one another at right angles. In front of this fibrous mass is a lens of perfectly homogeneous and highly refracting substance. This is surrounded and clasped by a ring of a structure similar to that of the stratified layer. Outside the fibrous ring and the lens is a cellular layer, whose nucleated cells are smaller in size than those of the posterior cellular layer. Outside the posterior half of the organ, forming a thin mosaic-like epithelium over the stratified layer, is a coating of flat polygonal red pigment cells, which are a specialised set of the red mesoblastic chromatophores which occur beneath the epidermis in various parts of the body. Cellular strands may be occasionally detected passing from

the surrounding tissues in between the anterior and posterior halves of the organ, but we have not satisfied ourselves that these strands are either muscular or nervous. The relations of the various layers are for the most part correctly described by Claus and Sars; but the former described a cuticle enveloping the whole organ, which does not exist, and the latter did not correctly describe the relation of the external pigmented epithelium to the stratified layer.

With regard to the emission of light, our experiments confirm the evidence of previous observations, that the luminosity is intermittent, and, as far as can be judged, closely dependent on stimulation. The following experiments were made :—

1. *Mechanical Stimulation.*—In total darkness the hand was inserted into a vessel containing sea-water in which some of the animals were swimming, and moved about. When an animal was touched it instantly emitted light. When an animal was taken between the fingers and removed from the water, all the organs shone brilliantly for 5 to 10 secs., while the animal was flapping its abdomen and trying to escape; then followed a series of separate flashes, and after 10 secs. more the emission of light ceased altogether, until fresh stimulation was applied by means of a squeeze between the finger and thumb, when all the organs immediately flashed.

When a fresh animal was crushed between the two hands, certain particles of the tissue became luminous, and remained so till they were dried up.

When one of the organs was crushed on the stage of the microscope, the field became immediately illuminated, and remained so for some time.

2. *Chemical Stimulation.*—When a living animal was placed into saturated solution of bichloride of mercury, all the organs shone most brilliantly during the energetic struggles preceding death: the luminosity lasted usually 5 to 7 secs., but in one case for 30 secs.

When a specimen was placed in nitric acid $\frac{1}{20}$ per cent., the same result occurred.

In both cases the posterior organs ceased to shine first, and the pair in the eye peduncles were the last to cease shining. One of us spent a whole morning at Millport in examination of the organs in the fresh state, by means of the microscope, with the object of ascertaining

from which particular portion of the organ the light proceeded. The results of this examination were afterwards verified by both of us together. In this investigation, no evidence was obtained in support of Sars' opinion that the light principally emanated from the central mass of fibres behind the lens. When the organ was crushed beneath a cover glass, and examined in daylight, it was not difficult to separate the different layers from one another. The exterior pigment cells were in this process dispersed, and every other part of the organ, with a single exception, was found to be perfectly colourless and transparent. The exception was the inner superficial portion of the stratified layer, which may for the time be named the argentea. This portion, when viewed by transmitted light, was seen to have a beautiful luminous purple colour, like a sunset tint. The purple was reddish at first, but gradually became more blue as time went on, till after about half an hour the colour was a deep blue or violet. When the transmitted light was shut off, and the preparation was viewed by reflected light without a condenser, the colour of the same region was the complementary tint of that seen by transmitted light. The peculiarity of this colour was, that it appeared to be luminous; that is, no part of the preparation could be seen at all in the field but this colour, which shone with a greenish-yellow light. When the light was entirely excluded, the whole preparation was invisible. It follows from this, that the inner surface of the argentea possesses in a marked degree the property of fluorescence. It was afterwards found that such a preparation, when viewed in the dark with the naked eye, contained a luminous spot, and this spot was always found to be the inner portion of the argentea. The phosphorescence was not visible through the microscope, simply because the light was absorbed by the lenses; but when light fell on the preparation, although it was not sufficient to render visible other parts of the organs, the inner surface of the argentea, by its fluorescent action on the most refrangible rays of the spectrum, became visible, just as in the case of uranium glass. Another fact is of great interest. When a living specimen of the animal is crushed between the hands and rubbed with the fingers, certain pieces of the mangled tissue are seen to be luminous in the dark. Such pieces can be picked off with the forceps and examined with the microscope, and are always found to be morsels

of the argentea. One of us was tempted to conclude from these facts that the luminosity of the organs in the living animal was entirely and exclusively due to the purely physical property of fluorescence in the internal portion of the argentea. But this conclusion is quite inconsistent with the intermittent emission of the light and its dependence on stimulation. Moreover, in other luminous organs, *e.g.*, *Lamypris splendidula*, the light has been shown to come from a thick mass of cells, and no layer resembling the argentea of *Nyctiphanes* is present. At present we conclude that probably the posterior cell layer in *Nyctiphanes* is the active agent in producing light when acted on by a nervous impulse, and that the light is much intensified by the fluorescent property of the surface of the argentea. Of the function of the central mass of rod or fibres we have ascertained nothing at all. The lens is obviously there in order to concentrate the light, while the anterior cellular cap is merely a transparent cornea. The fibrous iris-like ring round the lens perhaps acts as a diaphragm, though it undoubtedly is not pigmented and is transparent. We hope shortly to make renewed attempts to elucidate the mechanism of the organs.

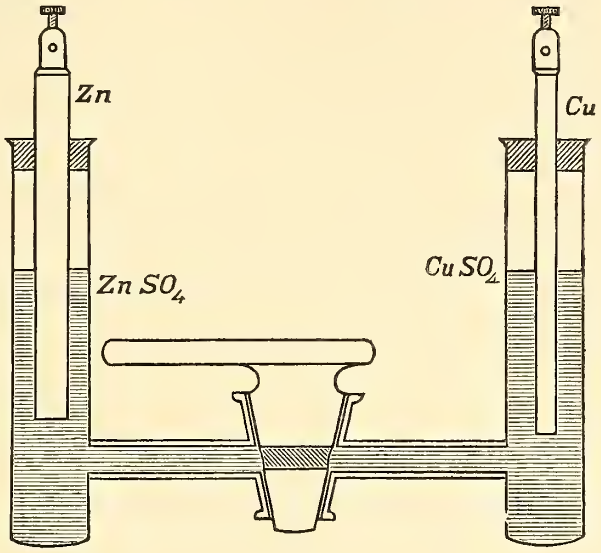
6. On a Constant Daniell Cell, for use as a Standard of Electromotive Force. By Cosmo I. Burton, B.Sc., F.C.S.

This cell consists of two tubes about three inches long and half an inch in diameter, sealed at one end, and connected together near the closed end by a glass tube about four inches long, having a glass tap in the middle.

The hole through the plug of the tap is filled with plaster of Paris, made as nearly as possible flush with the glass surfaces. This plaster plug serves as a porous septum between the two tubes, which represent respectively the two compartments of a Daniell cell—the one tube containing a copper rod immersed in a saturated solution of copper sulphate, the other a zinc rod in a solution of zinc sulphate, of as nearly as may be the same density as the copper solution. The cell is designed for use only with the quadrant electrometer, and must never be short-circuited.

When the cell is used the tap is turned "on," *i.e.*, so that the

ends of the plaster plug are opposite the two tubes containing solutions. Immediately that the observations are completed, the tap is turned off, and so diffusion of the solutions completely prevented. The tap is made of the peculiar shape shown in the



diagram, in order that it may only be turned in one direction, and so the possible transfer of a minute quantity of the one solution into the opposite tube is avoided.

Several years ago Mr A. P. Laurie and I used this cell constantly for four months, and during that time it showed no change of E.M.F., as compared with a standard Latimer Clark, readings being made to three figures only. More recently Professor Ayrton, of the City and Guilds Technical Schools, London, very kindly undertook to compare this cell with a standard Latimer Clark of his own construction, and also with Fleming's standard Daniell. The results of the comparison are given in the following table. E.M.F. of standard Latimer Clark is taken as unity.

Date.	Standard Fleming, E.M.F.	Experimental Cell, E.M.F.	Temperature of	
			Room.	Standard Latimer Clark.
1886. May 13.	0·7466	0·7476	18°·2 C.	16°·9 C.
	0·7462	0·7476	,,	,,
,, 20.	0·7505	0·7485	18°·6	16°·1
	0·7500	0·7489	19°·6	18°·1
,, 31.	...	0·7509	17°·9	16°·8
Oct. 7.	0·7456	0·7468	...	16°·8

On looking at the cell again, about eight months later, the copper wire was found corroded through, and contact broken. In order to avoid this accident, it is well to use copper rod not less than one-eighth of an inch thick, as copper is somewhat soluble in solution of sulphate of copper (see Gray, *Phil. Mag.*, 1886, p. 389).

The corks used to support the copper and zinc rods should be carefully paraffined, and every precaution taken to prevent evaporation of the solutions.

The observation in the above table, dated May 31, was taken rather hurriedly, and Professor Ayrton considers it untrustworthy. Omitting this result, it is seen that the experimental cell has a very constant E.M.F., and that the change, after about five months, was very small.

7. On Glories. By Professor Tait.

(Abstract.)

When Mr Omond was appointed to the Ben-Nevis Observatory I requested him to take every opportunity of observing what are called Glories—specially noting, when possible, their angular diameters and the order of their colours, so that it might be possible to decide upon the exact mode in which they are produced.

Young, while attributing to their true cause the spurious (or supernumerary) rainbows, proceeds to say:—"The circles, sometimes seen encompassing the observer's shadow in a mist, are perhaps more nearly related to the common colours of thin plates as seen by reflection."—[*Lectures*, II. p. 645].

Now from Mr Omond's observations it appears that the mists to which the glories are due produce coronæ of, say, 2° or 3° radius;—from which it follows that the diameter of the particles is somewhere of the order $\frac{1}{1000}$ inch. It is thence shown that, were Young's explanation correct, the radii of the rings would vary with great rapidity in passing from one kind of homogeneous light to another. This is altogether irreconcilable with Mr Omond's observations.

That the glories are not of the nature of spurious rainbows is shown very simply by the fact that they are more intense as their radii are smaller.

Hence, the only possible explanation is diffraction depending on the *form* of the vertex of the reflected wave. The form of an originally plane wave, once reflected inside a drop of water is, roughly, when the central ray has just emerged, a portion of an hyperboloid of revolution, doubled back cusp-wise round its border. An approximate calculation is given, based on this assumption.

A simple first approximation to the theory of glories is given by the behaviour of a plane wave incident normally on a screen pierced with a great number of very small circular apertures of nearly equal size. They are thus, to a certain extent, analogous to coronæ.

8. Report on the Pennatulida, dredged by H.M.S. "Porcupine." By A. Milnes Marshall, M.D., D.Sc., M.A., F.R.S., Beyer Professor of Zoology in the Owens College, and by G. H. Fowler, B.A., Ph.D., Berkeley Fellow of the Owens College, Manchester. Communicated by John Murray, Esq., Ph.D.

Friday, 15th July 1887.

JOHN MURRAY, Ph.D., Vice-President, in the Chair.

The following Communications were read :—

1. Stability of Fluid Motion.—Rectilineal Motion of Viscous Fluid between two Parallel Planes. By Sir W. Thomson, LL.D., F.R.S.

27. Since the communication of the first of this series of articles to the Royal Society of Edinburgh in April, and its publication in the *Philosophical Magazine* in May and June, the stability or instability of the steady motion of a viscous fluid has been proposed as subject for the Adams Prize of the University of Cambridge for 1888.* The present communication (§§ 27–40) solves the simpler

* See *Phil. Mag.*, July 1887.

of the two cases specially referred to by the examiners in their announcement, and prepares the way for the investigation of the less simple by a preliminary laying down, in §§ 27–29, and equations (7) to (12) below, of the fundamental equations of motion of a viscous fluid kept moving by gravity between two infinite plane boundaries inclined to the horizon at any angle I, and given with any motion deviating infinitely little from the determinate steady motion which would be the unique and essentially stable solution if the viscosity were sufficiently large. It seems probable, almost certain, indeed, that analysis similar to that of §§ 38 and 39 will demonstrate that the steady motion is stable for any viscosity, however small; and that the practical unsteadiness pointed out by Stokes forty years ago, and so admirably investigated experimentally five or six years ago by Osborne Reynolds, is to be explained by limits of stability becoming narrower and narrower the smaller is the viscosity.

Let OX be chosen in one of the bounding planes, parallel to the direction of the rectilineal motion; and OY perpendicular to the two planes. Let the x-, y-, z- component velocities, and the pressure at (x, y, z, t) be denoted by $U+u$, v, and p respectively; U denoting a function of (y, t). Then, calling the density of the fluid unity, and the viscosity μ, we have, as the equations of motion,*

$$\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1);$$

$$\left.\begin{aligned}
&\frac{d}{dt}(U+u)+(U+u)\frac{du}{dx}+v\frac{d}{dy}(U+u)+w\frac{dw}{dx}=\mu\nabla^2(U+u)-\frac{dp}{dx}+g\sin I,\\
&\frac{dv}{dt}+(U+u)\frac{dv}{dx}+v\frac{dv}{}+w\frac{dw}{dz}=\mu\nabla^2 v-\frac{dp}{dy}-g\cos I,\\
&\frac{dw}{dt}+(U+u)\frac{dw}{dx}+v\frac{dw}{dy}+w\frac{dw}{dz}=\mu\nabla^2 w-\frac{dp}{dz},
\end{aligned}\right\}\ (2);$$

where ∇^2 denotes the "Laplacian" $\frac{d^2}{dx^2}+\frac{d^2}{dy^2}+\frac{d^2}{dz^2}\cdot$

28. If we have $u=0$, $v=0$, $w=0$; $p=C-g\cos Iy+gx\sin I$; these four equations are satisfied identically; except the first of (2), which becomes

* Stokes's *Collected Papers*, vol. i. p. 93.

$$\frac{dU}{dt} = \mu\frac{d^2U}{d^2y} + g \sin I \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3).$$

This is reduced to

$$\frac{dv}{dt} = \mu\frac{d^2v}{dy^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4),$$

if we put

$$U = v + \tfrac{1}{2}g \sin I/\mu . (b^2 - y^2) \quad . \quad . \quad . \quad . \quad . \quad (5).$$

For terminal conditions (the bounding planes supposed to be $y=0$ and $y=b$, we may have

$$\left.\begin{array}{ll} v = F(t) \text{ when } y = 0 \\ v = \mathfrak{F}(t) \quad ,, \quad\;\; y = b \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6),$$

where F and $\mathfrak{F}$ denote arbitrary functions. These equations (4) and (6) show (what was found forty-two years ago by Stokes) that the diffusion of velocity in parallel layers, *provided it is exactly in parallel layers*, through a viscous fluid, follows Fourier's law of the "linear" diffusion of heat through a homogeneous solid. Now, towards answering the highly important and interesting question which Stokes raised,—Is this laminar motion unstable in some cases?—go back to (1) and (2), and in them suppose u, v, w to be each infinitely small: (1) is unchanged; (2) with U eliminated by (5), become

$$\frac{dw}{dt} + [v + \tfrac{1}{2}c(b^2 - y^2)]\frac{du}{dx} + v\left(\frac{du}{dy} - cy\right) = \mu\nabla^2 u - \frac{dp}{dx} \quad . \quad (7),$$

$$\frac{dv}{dt} + [v + \tfrac{1}{2}c(b^2 - y^2)]\frac{dv}{dx} \qquad\qquad = \mu \;\; {}^2 v - \frac{dp}{dy} \quad . \quad (8),$$

$$\frac{dw}{dt} + [v + \tfrac{1}{2}c(b^2 - y^2)]\frac{dw}{dx} \qquad\qquad = \mu\nabla^2 w - \frac{dp}{dz} \quad . \quad (9),$$

where

$$c = g \sin I/\mu \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10),$$

and, for brevity, p now denotes, instead of as before the pressure, the pressure $+ g \cos I\, y$.

We will suppose v to be a function of y and t determined by (4) and (6). Thus (1) and (7), (8), (9) are four equations which, with proper initial and boundary conditions, determine the four unknown quantities u, v, w, p; in terms of x, y, z, t.

29. It is convenient to eliminate u and w; by taking $\frac{d}{dx}$, $\frac{d}{dy}$, $\frac{d}{dz}$ of (7), (8), (9), and adding. Thus we find, in virtue of (1),

$$2\left(\frac{dv}{dy}-cy\right)\frac{dv}{dx}=-\nabla^2 p \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11).$$

This and (8) are two equations for the determination of v and p. Eliminating p between them, we find

$$\frac{d\nabla^2 v}{dt}-\left(\frac{d^2 v}{dy^2}-c\right)\frac{dv}{dx}+\left[v-\tfrac{1}{2}c(b^2-y^2)\right]\frac{d\nabla^2 v}{dx}=\mu\nabla^4 v \quad . \quad . \quad (12),$$

a single equation which, with proper initial and boundary conditions, determines the one unknown, v. When v is thus found, (8), (7), (9) determine p, u, and w.

30. An interesting and practically important case is presented by supposing one or both of the bounding planes to be kept oscillating in its own plane; that is, F and $\mathfrak{F}$ of (6) to be periodic functions of t. For example, take

$$\mathrm{F}=a\cos wt,\ \mathfrak{F}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13).$$

The corresponding periodic solution of (4) is

$$v=a\frac{\epsilon^{(b-y)\sqrt{\frac{\omega}{2\mu}}}-\epsilon^{-(b-y)\sqrt{\frac{\omega}{2\mu}}}}{\epsilon^{b\sqrt{\frac{\omega}{2\mu}}}-\epsilon^{b\sqrt{\frac{\omega}{2\mu}}}}\cos\left(\omega t-y\sqrt{\frac{\omega}{2\mu}}\right) \quad . \quad . \quad (14).$$

In connection with this case there is no particular interest in supposing a current to be maintained by gravity; and we shall therefore take $c=0$, which reduces (7), (8), (9), (11), (12) to

$$\frac{du}{dt}+v\frac{du}{dx}+\frac{dv}{dy}v=\mu\nabla^2 u-\frac{dp}{dx} \quad . \quad . \quad . \quad . \quad (15),$$

$$\frac{dv}{dt}+v\frac{dv}{dx}=\mu\nabla^2 v-\frac{dp}{dy} \quad . \quad . \quad . \quad . \quad (16),$$

$$\frac{dw}{dt}+v\frac{dw}{dx}=\mu\nabla^2 w-\frac{dy}{dz} \quad . \quad . \quad . \quad . \quad (17),$$

$$2\frac{dv}{dy}\frac{dv}{dx}=-\nabla^2 p \quad . \quad . \quad . \quad . \quad . \quad (18),$$

$$\frac{d\nabla^2 v}{dt}+\frac{d^2 v}{dy^2}\frac{dv}{dx}+v\frac{d\nabla^2 v}{dx}=\mu\nabla^4 v \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19);$$

in all of which v is the function of (y, t) expressed by (14).

These equations (15) . . . (19) are of course satisfied by $u=0$, $v=0$, $w=0$, $p=0$. The question of stability is, Does every possible solution of them come to this in time? It seems to me probable that it does; but I cannot, at present at all events, enter on the

investigation. The case of $b = \infty$ is specially important and interesting.

31. The present communication is confined to the much simpler case in which the two bounding planes are kept moving relatively with constant velocity; including as sub-case the two planes held at rest, and the fluid caused by gravity to move between them. But we shall first take the much simpler sub-case, in which there is relative motion of the two planes, and no gravity. This is the very simplest of all cases of the general question of the Stability or Instability of the Motion of a Viscous Fluid. It is the second of the two cases prescribed by the Examiners for the Adams Prize of 1888. I have ascertained, and I now proceed to give the proof, that in this sub-case the steady motion is wholly stable, however small or however great be the viscosity; and this without limitation to two-dimensional motion of the admissible disturbances.

32. In our present sub-case, let βb be the relative velocity of the two planes; so that in (6) we may take $\mathrm{F}=0$, $\mathfrak{F}=\beta b$; and the corresponding steady solution of (4) is

$$v = \beta y \qquad \text{. . . . . (20).}$$

Thus equation (19) becomes reduced to

$$\left.\begin{aligned} \frac{d\zeta}{dt} + \beta y \frac{d\zeta}{dx} &= \mu \nabla^2 \zeta, \\ \text{where} \qquad \zeta &= \nabla^2 v \end{aligned}\right\} \qquad \text{. . . . . . . (21);}$$

and (18), (15), (16), (17) become

$$2\beta \frac{dv}{dx} = - \nabla^2 p \qquad \text{. . . . . . . (22),}$$

$$\frac{du}{dt} + \beta y \frac{du}{dx} + \beta v = \mu \nabla^2 u - \frac{dp}{dx} \qquad \text{. . . . (23),}$$

$$\frac{dv}{dt} + \beta y \frac{dv}{dx} = \mu \nabla^2 v - \frac{dp}{dy} \qquad \text{. . . . (24),}$$

$$\frac{dw}{dt} + \beta y \frac{dw}{dx} = \mu \nabla^2 w - \frac{dp}{dz} \qquad \text{. . . . (25).}$$

It may be remarked that equations (22) . . . (25) imply (1), and that any four of the five determines the four quantities u, v, w, p.

It will still be convenient occasionally to use (1). We proceed to find the complete solution of the problem before us, consisting of expressions for u, v, w, p satisfying (22) . . . (25) for all values of x, y, z, t; and the following initial and boundary conditions:—

$$\left.\begin{array}{r}\text{when } t=0:\ u,\ v,\ w, \text{ to be arbitrary functions}\\ \text{of } x,\ y,\ z, \text{ subject only to (1)}\end{array}\right\} \quad . \quad . \quad (26);$$

$$\left.\begin{array}{l}u=0,\ v=0,\ w=0, \text{ for } y=0 \text{ and all values of } x,\ z,\ t\\ u=0,\ v=0,\ w=0, \text{ for } y=b \qquad ,, \qquad ,,\end{array}\right\} \quad . \quad (27).$$

33. First let us find a particular solution $\mathbf{u}$, $\mathbf{v}$, $\mathbf{w}$, $\mathbf{p}$, which shall satisfy the initial conditions (26), irrespectively of the boundary conditions (27), except as follows:—

$$\left.\begin{array}{l}\mathbf{v}=0, \text{ when } t=0 \text{ and } y=0\\ \mathbf{v}=0, \text{ when } t=0 \text{ and } y=b\end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad (28).$$

Next, find another particular solution, $\mathfrak{u}$, $\mathfrak{v}$, $\mathfrak{w}$, $\mathfrak{p}$, satisfying the following initial and boundary equations:—

$$\mathfrak{u}=0,\ \mathfrak{v}=0,\ \mathfrak{w}=0, \text{ when } t=0 \quad . \quad . \quad . \quad . \quad (29),$$

$$\left.\begin{array}{r}\mathfrak{u}+\mathbf{u}=0,\ \mathfrak{v}+\mathbf{v}=0,\ \mathfrak{w}+\mathbf{w}=0, \text{ when } y=0\\ \text{and when } y=b\end{array}\right\} \quad . \quad . \quad (30).$$

The required complete solution will then be

$$u=\mathfrak{u}+\mathbf{u}, \quad v=\mathfrak{v}+\mathbf{v}, \quad w=\mathfrak{w}+\mathbf{w} \quad . \quad . \quad . \quad . \quad (31).$$

34. To find $\mathbf{u}$, $\mathbf{v}$, $\mathbf{w}$, remark that, if μ were zero, the complete integral of (21) would be

$$\zeta = \text{arb. func. } (x-\beta yt)\,;$$

and take therefore as a trial for a type-solution with μ not zero,

$$\zeta = \mathrm{T}\epsilon^{\iota[mx+(n-m\beta t)y+qz]} \quad . \quad . \quad . \quad . \quad . \quad . \quad (32);$$

where T is a function of t, and ι denotes $\sqrt{-1}$. Substituting accordingly in (21), we find

$$\frac{d\mathrm{T}}{dt} = -\mu[m^2+(n-m\beta t)^2+q^2]\mathrm{T} \quad . \quad . \quad . \quad . \quad (33);$$

whence, by integration,

$$\mathrm{T} = \mathrm{C}\epsilon^{-\mu t\left[m^2+n^2+q^2-nm\beta t+\frac{m^2}{3}\beta^2 t^2\right]} \quad . \quad . \quad . \quad . \quad (34).$$

By the second of (21) and (32) we find

$$v = -\mathrm{T}\frac{\epsilon^{\iota[mx+(n-m\beta t)y+qz]}}{m^2+(n-m\beta t)^2+q^2} \quad . \quad . \quad . \quad . \quad (35);$$

whence, by (22),

$$p = -2\beta m\iota\mathrm{T}\frac{\epsilon^{\iota[mx+(n-m\beta t)y+qz]}}{[m^2+(n-m\beta t)^2+q^2]^2} \quad . \quad . \quad . \quad . \quad (36).$$

Using this in (25), and putting

$$w = \mathrm{W}\epsilon^{\iota[mx+(n-m\beta t)y+qz]} \quad . \quad . \quad . \quad . \quad . \quad (37),$$

we find

$$\frac{d\mathrm{W}}{dt} = -\mu[m^2+(n-m\beta t)^2+q^2]\mathrm{W} - \frac{2\beta mq\mathrm{T}}{[m^2+(n-m\beta t)^2+q^2]} \quad (38),$$

which, integrated, gives W.

Having thus found v and w, we find u by (1), as follows:—

$$u = -\frac{(n-m\beta t)v+qw}{m} \quad . \quad . \quad . \quad . \quad . \quad (39).$$

35. Realising by adding type-solutions for $\pm\iota$ and $\pm n$, with proper values of C, we arrive at a complete real type-solution with, for v, the following—in which K denotes an arbitrary constant:—

$$v = \tfrac{1}{2}\mathrm{K}\left\{\frac{\epsilon^{-\mu t[m^2+n^2+q^2-nm\beta t+\frac{1}{3}m^2\beta^2t^2}}{m^2+(n+m\beta t)^2+q^2}\,{}^{\cos}_{\sin}[mx+(n+m\beta t)y+qz]\right.$$
$$\left.-\frac{\epsilon^{-\mu t[m^2+n^2+q^2+nm\beta t+\frac{1}{3}m^2\beta^2t^2]}}{m^2+(n+m\beta t)^2+q^2}\,{}^{\cos}_{\sin}[mx+(n+m\beta t)y+qz]\right\} \quad (40).$$

This gives, when $t=0$,

$$v = \frac{\mp\mathrm{K}}{m^2+n^2+q^2}\sin ny\,{}^{\sin}_{\cos}(mx+qz) \quad . \quad . \quad . \quad (41),$$

which fulfils (28) if we make

$$n = i\pi y/b \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (42);$$

and allows us, by proper summation for all values of i from 1 to ∞, and summation or integration with reference to m and q, with properly determined values of K, after the manner of Fourier, to give any arbitrarily assigned value to $v_{t=0}$ for every value of x, y, z,

$$\left.\begin{array}{lll} \text{from} & x=-\infty & \text{to } x=+\infty, \\ \quad ,, & y=0 & \quad ,,\ y=b, \\ \quad ,, & z=-\infty & \quad ,,\ z=+\infty. \end{array}\right\} \quad . \quad . \quad . \quad . \quad (43).$$

The same summation and integration applied to (40) gives v for all values of t, x, y, z; and then by (38), (37), (39) we find corresponding determinant values of w and u.

36. To give now an arbitrary initial value, w_0, to the z-component of velocity, for every value of x, y, z, add to the solution (u, v, w), which we have now found, a particular solution (u', v', w') fulfilling the following conditions :—

$$\left.\begin{array}{l} v' = 0 \text{ for all values of } t, x, y, z\,; \\ w' = \mathrm{w}_0 - w_0 \text{ for } t = 0, \text{ and all values of } x, y, z \end{array}\right\} \quad . \quad (44),$$

and to be found from (25) and (1), by remarking that $v' = 0$ makes, by (22), $p' = 0$, and therefore (23) and (25) become

$$\frac{du'}{dt} + \beta y \frac{du'}{dx} = \mu \nabla^2 u' \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (45),$$

$$\frac{dw'}{dt} + \beta y \frac{dw'}{dx} = \mu \nabla^2 w' \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (46),$$

Solving (46); just as we solved (21) by (32), (33), (34); and then realising and summing to satisfy the arbitrary initial condition, as we did for v in (40), (41), (42), we achieve the determination of w'; and by (1) we determine the corresponding u', *ipso facto* satisfying (45). Lastly, putting together our two solutions, we find

$$\mathrm{u} = u + u', \quad \mathrm{v} = v, \quad \mathrm{w} = w + w' \quad . \quad . \quad . \quad . \quad . \quad (47),$$

as a solution of (26) without (27), in answer to the first requisition of § 33. It remains to find $\mathfrak{u}$, $\mathfrak{v}$, $\mathfrak{w}$, in answer to the second requisition of § 33.

37. This we shall do by first finding a real (simple harmonic) periodic solution of (21), (22), (23), (25), fulfilling the condition

$$\left.\begin{array}{l} \left.\begin{array}{l} u = \mathrm{A} \cos \omega t + \mathrm{B} \sin \omega t \\ v = \mathrm{C} \cos \omega t + \mathrm{D} \sin \omega t \\ w = \mathrm{E} \cos \omega t + \mathrm{F} \sin \omega t \end{array}\right\} \text{when } y = 0 \\ \left.\begin{array}{l} u = \mathfrak{A} \cos \omega t + \mathfrak{B} \sin \omega t \\ v = \mathfrak{C} \cos \omega t + \mathfrak{D} \sin \omega t \\ w = \mathfrak{E} \cos \omega t + \mathfrak{F} \sin \omega t \end{array}\right\} \text{when } y = b \end{array}\right\} \quad . \quad . \quad . \quad (48),$$

where A, B, C, D, E, F, $\mathfrak{A}$, $\mathfrak{B}$, $\mathfrak{C}$, $\mathfrak{D}$, $\mathfrak{E}$, $\mathfrak{F}$ are twelve arbitrary functions of (x, z). Then by taking $\int_0^\infty d\omega f(\omega)$ of each of these after

the manner of Fourier, we solve the problem of determining the motion produced throughout the fluid, by giving to every point of each of its approximately plane boundaries an infinitesimal displacement of which each of the three components is an arbitrary function of x, z, t. Lastly, by taking these functions each $=0$ from $t=-\infty$ to $t=0$, and each equal to minus the value of $\mathbf{u}$, $\mathbf{v}$, $\mathbf{w}$ for every point of each boundary, we find the $\mathfrak{u}$, $\mathfrak{v}$, $\mathfrak{w}$ of § 33. The solution of our problem of § 32 is then completed by equations (31). To do all this is a mere routine after an imaginary type solution is provided as follows :—

38. To satisfy (21) assume

$$v=\epsilon^{\iota(\omega t+mx+qz)}\mathfrak{V}$$
$$=\epsilon^{\iota(\omega t+mx+qz)}\left\{\mathrm{H}\epsilon^{y\sqrt{(m^2+q^2)}}+\mathrm{K}\epsilon^{-y\sqrt{(m^2+q^2)}}+\mathrm{L}f(y)+\mathrm{MF}(y)\right\} \quad . \quad (49),$$

where H, K, L, M are arbitrary constants and f, F any two particular solutions of

$$\iota(\omega+\beta y)\zeta=\mu\left[\frac{d^2\zeta}{dy^2}-(m^2+q^2)\zeta\right] \quad . \quad . \quad . \quad (50).$$

This equation, if we put

$$m\beta/\mu=\gamma, \text{ and } m^2+q^2+\iota\omega/\mu=\lambda \quad . \quad . \quad . \quad . \quad (51),$$

becomes

$$\frac{d^2\zeta}{dy^2}=(\lambda+\iota\gamma y)\zeta \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (52);$$

which, integrated in ascending powers of $(\lambda+\iota\gamma y)$, gives two particular solutions, which we may conveniently take for our f and F, as follows :—

$$\left.\begin{aligned} f(y)&=1-\frac{\gamma^{-2}(\lambda+\iota\gamma y)^3}{3.2}+\frac{\gamma^{-4}(\lambda+\iota\gamma y)^6}{6.5.3.2}-\frac{\gamma^{-6}(\lambda+\iota\gamma y)^9}{9.8.6.5.3.2}+\&c.\\ \mathrm{F}(y)&=\lambda+\iota\gamma y-\frac{\gamma^{-2}(\lambda+\iota\gamma y)^4}{4.3}+\frac{\gamma^{-4}(\lambda+\iota\gamma y)^7}{7.6.4.3}-\frac{\gamma^{-6}(\lambda+\iota\gamma y)^{10}}{10.9.7.6.4.3}+\&c.\end{aligned}\right\}(53).$$

39. *These series are essentially convergent for all values of y.* Hence in (49) we have a solution continuous from $y=0$ to $y=b$; and by its four arbitrary constants we can give any prescribed values to V, and $\dfrac{d\mathrm{V}}{dy}$ for $y=0$ and $y=b$. This done, find p determinately by (24); and then integrate (25) for w in *an essentially convergent series* of ascending powers of $\lambda+\iota\gamma y$, which is easily worked out,

but need not be written down at present, except in abstract as follows:—

$$w = \mathfrak{W}\epsilon^{\iota(\omega t + mx + qz)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (54);$$

where

$$\left.\begin{aligned} \mathfrak{W} = \mathrm{H}\mathfrak{F}_1(\lambda + \iota\gamma y) + \mathrm{K}\mathfrak{F}_2(\lambda + \iota\gamma y) + \mathrm{L}\mathfrak{F}_3(\lambda + \iota\gamma y) \\ + \mathrm{M}\mathfrak{F}_4(\lambda + \iota\gamma y) + \mathrm{P}\epsilon^{y\sqrt{(m^2+q^2)}} + \mathrm{Q}\epsilon^{-y\sqrt{(m^2+q^2)}} \end{aligned}\right\} \quad (55).$$

Here P and Q are the two fresh constants, due to the integration for w. By these we can give to W any prescribed values for $y=0$ and $y=b$. Lastly, by (1), with (49), we have

$$\left.\begin{aligned} u = \mathfrak{U}\epsilon^{\iota(\omega t + mx + qz)} \\ \text{where} \qquad \mathfrak{U} = -\left(\frac{1}{m\iota}\frac{d\mathfrak{V}}{y} + \frac{q}{m}\mathfrak{W}\right) \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (56).$$

Our six arbitrary constants H, K, L, M, P, Q clearly allow us to give any prescribed values to each of $\mathfrak{U}$, $\mathfrak{V}$, $\mathfrak{W}$, for $y=0$ and for $y=b$. Thus the completion of the realised problem with real data of arbitrary functions, as described in § 37, becomes a mere affair of routine.

40. Now remark that the (**u**, **v**, **w**) solution of § 34 comes essentially to nothing, asymptotically as time advances, as we see by (33), (34), and (38). Hence the ($\mathfrak{u}$, $\mathfrak{v}$, $\mathfrak{w}$) of § 37, which rise gradually from zero at $t=0$, come asymptotically to zero again as t increases to ∞. We conclude that the steady motion is stable.

2. Note on the Epiblastic Origin of the Segmental Duct in Teleostean Fishes and in Birds. By George Brook, F.L.S., Lecturer on Comparative Embryology in the University of Edinburgh. Communicated by Prof. Sir Wm. Turner, F.R.S.

Our knowledge of the development of the excretory system in both vertebrates and invertebrates is as yet very incomplete, perhaps more so than of any other system. Until quite recently the whole of the urogenital system of the vertebrates was supposed to be derived from the mesoblast. This view received a sudden check

when, on the publication of Graf Spee's researches on the guinea pig in 1884, the segmental (pronephric) duct was shown to have an epiblastic origin. Hensen, indeed, had noted the fact some years previously, but no notice had been taken of his discovery until Graf Spee called attention to it. Hensen has recently taken up the subject again, and Flemming has published a confirmatory account for the rabbit. Thus there appears no further room for doubting the epiblastic origin of the segmental duct in mammals. It does not necessarily follow that the whole excretory system has an epiblastic origin, but further information is required on the subject. Towards the end of 1886 Van Wijhe demonstrated the epiblastic origin of the segmental duct in Elasmobranchs, and during the present year Von Perenyi has announced that the epiblast plays a similar part in *Rana* and *Lacerta*. During the past few months I have been enabled to confirm Von Perenyi's researches so far as *Rana* is concerned, and have also found that, in regard to the formation of the segmental duct, Teleostean fishes, and probably also birds, are in agreement with other types. The epiblastic origin of the segmental duct is probably a feature common to the Vertebrata generally.

In the trout the segmental duct arises almost precisely in the manner described and figured by Flemming for the rabbit. In a twenty-seven days' embryo the duct is well marked in the middle trunk region, and thins out both anteriorly and posteriorly. Anteriorly the duct appears as a thickening of that portion of the surface epiblast overlying the intermediate cell mass; that is to say, the segmental duct arises from that part of the epiblast dorsal to the portion of the mesoblast from which it was formerly supposed to be derived. Passing posteriorly the epiblastic thickening becomes more and more important, and in the middle trunk region forms a large rounded mass of cells still partly attached to the epiblast, and situated between the vertebral plate and the lateral mesoblast. The lumen of the duct appears first as an irregular cavity, and later the whole mass loses its connection with the epiblast, and becomes pressed in amongst the "intermediate cell mass" during the formation of the lateral body folds.

The origin of the segmental duct in birds does not appear to be quite as clear. Anteriorly, the epiblast covering the central nervous

system and the vertebral plates in chick embryos of forty to forty-eight hours forms a thin membrane. On nearing the ventral portion of the vertebral plates the epiblast becomes slightly thickened, while immediately beyond the vertebral plates there is a slight involution and a considerable thickening in the outer layer. On passing to the lateral mesoblast the epiblast again thins out. Here, evidently, is an epiblastic thickening corresponding precisely in position with that forming the segmental duct in other vertebrates. In the chick, however, the "intermediate cell mass" is comparatively large, and the epiblastic thickening soon becomes fused with the mesoblast. In a forty-eight hour chick embryo I have noticed a curved line more distinctly shown in some sections than in others, which I take to define the limit of the epiblast. In the posterior portion of the embryo the epiblast and "intermediate cell mass" are quite separate, and I was unable to trace any thickening in the epiblast of that region. Probably, therefore, the duct is pushed backwards from the anterior portion without coming into contact with the epiblast. This, at any rate, is the mode of development previously described in Elasmobranchs and birds, when the segmental duct was supposed to have a mesoblastic origin.

The whole of these recent researches must necessarily lead to a modification in our views of the morphology of the vertebrate excretory apparatus. Haddon has recently suggested that primitively the nephridia (derived from the mesoblast) opened on each side into a lateral groove, that later this groove deepened and formed a closed canal, which subsequently acquired a secondary opening to the exterior through the cloaca.

I propose to discuss this subject more fully in a future paper.

3. Preliminary Note on the Chemistry of Strophanthin. By Thomas R. Fraser, M.D., F.R.S., Professor of Materia Medica in the University of Edinburgh.

Since my former communications, in which several facts relating to the chemistry of *Strophanthus hispidus* have been stated, I have completed a systematic examination of various parts of this plant, and more particularly of the seeds. Reserving a detailed descrip-

tion of the results of this examination, I propose now to mention only a few of these results in a brief form.

The active principle, to which I have given the name Strophanthin, occurs most abundantly in the seeds. By a very simple process, consisting essentially of the separation of oil by means of ether from the alcoholic extract, I obtained some years ago a crystalline body having great pharmacological activity, and possessing the characteristics of a glucoside. In subsequent experiments, however, although the same process was followed, a well-marked crystalline product was not always obtained, and it soon became evident that this difference was due to some difference in the condition of the seeds which had been operated with. Thus, from seeds collected by the late Bishop Mackenzie more than twenty years ago, and also from seeds sent to me by Mr Buchanan of Blantyre, East Africa, in 1881, I had no difficulty in separating an active principle in the form of well-marked minute crystals; but from seeds obtained from Mr Buchanan in 1885, and also from seeds liberally placed at my disposal by Mr Moir and by Messrs Burroughs, Wellcome & Co. last year, I failed to obtain an equally definite crystalline body. I also found that the body obtained by the process formerly described, whether in well-defined crystals or not, was resolvable by acetate of lead into at least two bodies, one of which is an extremely active glucoside, and the other an acid, for which I would suggest the name kombic acid. It having become apparent, therefore, that the strophanthin first described is not a simple substance, attempts were made to improve the process so as to separate strophanthin in a more pure form than I had originally succeeded in doing. The result of these attempts has been the adoption of a process whose essential steps are the following :—

Starting with the product obtained by the earlier process, it is dissolved in water, tannic acid is added to the solution, the tannate is digested with recently precipitated oxide of lead, and then extracted with rectified and proof spirit. This extract is dissolved in a small quantity of rectified spirit, and the solution is precipitated by ether. The precipitate is finally dissolved in weak alcohol, and through this solution carbonic acid is passed for several hours, by which means lead is completely got rid of. After filtration the solution is evaporated at a low temperature, and the

product is dried *in vacuo* over sulphuric acid. In the process of drying, it first assumes a translucent, gummy appearance, and then becomes opaque and white.

Strophanthin thus obtained is imperfectly crystalline, neutral or faintly acid in reaction, intensely bitter, freely soluble in water, less so in rectified spirit, and practically insoluble in ether and chloroform. It burns without residue, and it does not contain nitrogen. When subjected to ultimate analysis its percentage composition, taking, for the sake of brevity, the average of several closely agreeing combustions, was found to be—

Carbon, 55·976.

Hydrogen, 7·754.

Oxygen, 36·283.

This percentage composition fairly corresponds with the formula, $C_{29}H_{48}O_{14}$.

The effects of a number of reagents upon it have been determined. In the meantime the following may be stated :—Strong sulphuric acid produces a bright green colour, which soon becomes greenish yellow and brown; sulphuric acid and bichromate of potash, in addition to the changes produced by sulphuric acid, a blue colour; phospho-molybdic acid, after contact for a few hours, a bluish green, which, on the addition of a few drops of water, becomes pure blue ;* nitric or hydrochloric acid, a yellowish brown ; and caustic potash, ammonia, and other alkalies a faint yellow. With a 1 per cent. solution in water, phospho-molybdic acid causes, somewhat slowly, a bright green colour, which after prolonged contact becomes greenish blue ;* nitrate of silver, a reddish brown colour, and a slight dark precipitate; caustic potash and other alkalies, a very faint yellow ; dilute sulphuric acid, a faint white opalescence ; and tannic acid, an abundant white precipitate, soluble in excess both of strophanthin and of tannic acid. The solution, tested at the ordinary temperature, is not changed in appearance by acetate or subacetate of lead, perchloride of platinum, chloride of gold, perchloride of iron, perchloride of iron and sulphuric acid, perchloride of mercury, sulphate of copper, bichromate of potassium,

* Since this paper was communicated, I have found that the blue colour may be almost instantaneously produced by adding an alkali, such as solution of potash, after the addition of the phospho-molybdic acid.

iodide of potassium, nor by many other reagents; except that nearly all acid reagents cause the solution to become slightly hazy, and it is then found that the solution contains glucose. This decomposition is also produced by sulphuretted hydrogen, and for this reason it is not advisable to use sulphuretted hydrogen in any process for preparing strophanthin.

Indeed, all the mineral acids, excepting carbonic acid and many of the organic acids, resolve strophanthin, even in the cold, into glucose and a substance which I have named strophanthidin. A very pretty crystallisation of the latter is spontaneously produced, in a few hours, in a solution of strophanthin in 1·5 per cent. sulphuric acid. Contact at the ordinary temperature for even three days with dilute sulphuric acid does not apparently entirely decompose the strophanthin, as an additional quantity of glucose seems to be afterwards produced when the solution, filtered from strophanthidin, is heated at 212° F. for a few hours. Thus, when strophanthin was decomposed at the ordinary temperature by contact for about three days with 1·5 per cent. sulphuric acid, there was obtained 37·5 per cent. of crystalline strophanthidin, and about 20 per cent. of glucose.* The crystals of strophanthidin having been removed by filtration, and the almost colourless, bitter, and acid fluid having been boiled for four hours, it was now found that the glucose had increased to 26·64 per cent., and that about 4·3 per cent. of an amorphous brownish substance had been formed.

This action of acids renders it apparent that an acid, and especially a mineral acid, should not be used in the preparation of strophanthin. Thus, in 1877, seven years after the publication of my first communications on this subject, Hardy and Gallois described a process in which, by using for the extraction of the seeds rectified spirit acidulated with hydrocholic acid, they obtained a crystalline product which they believed to be strophanthin. There can be little doubt, however, that their crystalline product was strophanthidin, not only because the process they employed would decompose the strophanthin into strophanthidin and glucose, but also because

* In the solution obtained by this decomposition, the exact estimation of glucose by Fehling's solution is rendered difficult and uncertain by a green colour being produced, which persists after the blue colour of Fehling's solution has disappeared.

their crystalline product was found by them not to yield glucose when it was heated with dilute sulphuric acid. Hence they concluded that strophanthin is not a glucoside (*Comptes Rendus de l'Academie des Sciences*, lxxxiv., 1877, p. 261; and *Journal de Pharmacie et de Chémie*, xxv., 1877, p. 177). The glucosidal character of strophanthin, however, has now been amply demonstrated by a large number of experiments which I have made, and by the experiments of subsequent observers, and especially by those of A. W. Gerrard, described in an interesting paper published this year (*The Pharmaceutical Journal and Transactions*, 14th May 1887, p. 923). Further, the solution obtained when strophanthin is decomposed by sulphuric acid has been fermented with yeast, and carbonic acid, representing 23·64 per cent. of glucose, has been obtained.

4. On a New Diffusiometer and other Apparatus for Liquid Diffusion. By J. J. Coleman, F.I.C., F.C.S.

Supposing a tall glass tube open at both ends be cemented into a reservoir packed full of common salt, and the tube then carefully filled up with water, and the whole apparatus immersed overhead in a jar of water, in a few days or weeks (depending upon the length of the tube) particles of salt will arrive at the top of the tube and diffuse into the water atmosphere.

When this condition arrives, which Fick calls "dynamic equilibrium," diffusion takes place at a uniform rate, the mathematical expression of the process being stated as follows:—

Let K denote the quantity of salt which in a normal state of diffusion passes in a unit of time through a unit of horizontal section of a cylindrical tube whose height is equal to the unit of length, this being called the diffusion coefficient; also let Q be the quantity of salt which in the time t flows from the mouth of the tube; S its horizontal section; d the density of the liquid at the bottom; and h the height of the tube; then

$$Q = Kd\frac{S}{h}t.$$

Experiments subsequent to those of Fick have caused some doubt as to whether the "coefficient of diffusion" is the same for all

densities, but the conclusion he came to, that under the conditions of his experiments *the quantities diffused are directly as the times of diffusion*, is easily and elegantly shown by using concentrated acids or alkalies instead of common salt.

Thus taking a tube 9 millimetres in diameter and 20·5 centimetres long, and cementing it into a reservoir, which in shape may conveniently be that of the reservoir of a glass spirit-lamp, holding 350 grammes of hydrochloric acid of 1·17 sp. gr., and immersing the whole in a jar 25 centimetres high and 12 cm. diameter, kept at a uniform temperature of 16° C., containing 2000 c.c. of water, and changing the water every two or three days, and commencing after the 21st day to estimate the acid diffused, it was found to be very uniform, viz. :—

Milligrams.			Milligrams.
99·9	from 21st to 24th day,	=	33·3 per day.
98·4	„ 24th to 27th „	=	32·8 „
103·2	„ 27th to 30th „	=	34·4 „
98·4	„ 30th to 33rd „	=	32·8 „
		Average,	33·3 per day.

The use of such modern indicators as phenolphthaleïn and methyl orange has rendered the end reaction of a volumetric process so delicate, that no difficulty is experienced in measuring such small quantities as one part of acid in 20,000 of water, which were about the conditions of these experiments.

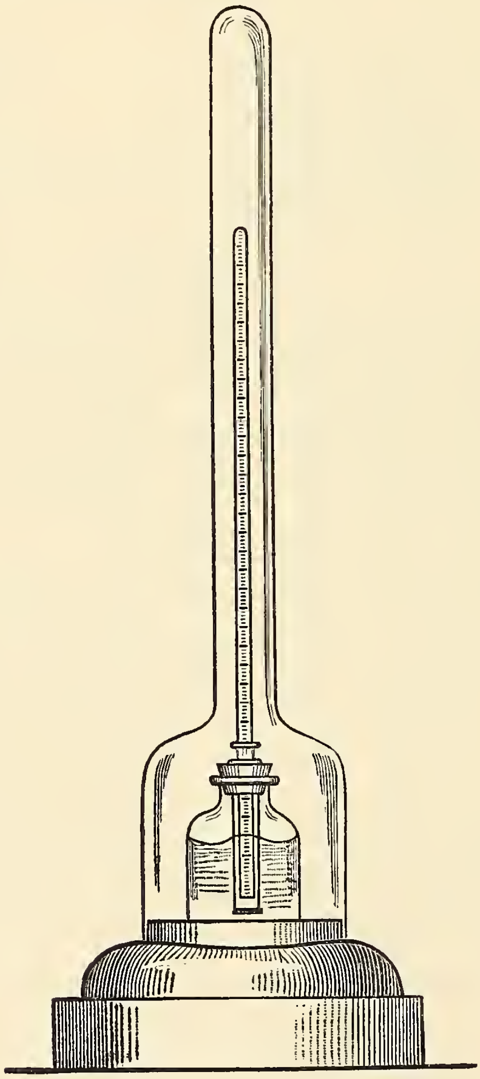
Turning attention now to what happens in a diffusion tube before "dynamic equilibrium" is established, which indeed is typical of all cases in which the diffused column is constantly being elongated by ascent of fresh particles from the bottom of a tube of constant diameter, I have devised a piece of apparatus which renders this motion visible to the eye, and which mathematical considerations developed by physicists indicate, should *be as the square root of the times of diffusion.*

The principle upon which the apparatus is constructed is as follows:—

If a glass jar is nearly filled with very dilute acid coloured red with methyl orange (sodium methyl-amido-azo-benzene-sulphonate), and a solution of caustic soda or potash is run by a fine pipette to the bottom of the jar underneath the acid, the alkali diffuses and changes

the colour of the methyl red to bright yellow, the line of demarcation being as strongly defined as that of oil floating upon water, and this even if the diffusion is carried on for thirty-five or forty days.

The caustic alkali solution is most advantageously placed in an open-mouthed vessel of such capacity that a barometer tube, 12 mm. to 15 mm. inside diameter, containing the reddened acid can be overturned therein with its mouth downwards, the acid being kept in the tube by means of an india-rubber disc, attached by three platinum wires to a cork sliding on the tube, which thus acts as a valve to be thrust down when the time for starting diffusion arrives. The tubes I prefer are about 12·5 mm. diameter and 600 mm. long, accurately graduated in millimetres.



The drawing herewith illustrates the construction of this instrument or "liquid diffusiometer," surrounded by a glass bell jar to prevent its being affected in temperature by air currents.

The following experiments were made by diffusing caustic potash of three different densities into reddened dilute hydrochloric acid, which latter in every case required ·145 milligramme of potash (KHO) per c.c. to turn the colour from red to yellow. The reservoirs of each were about 60 millimetres diameter, and contained about 200 c.c. of alkali solution, so that its strength was approximately constant during the diffusions.

The diffusions were made in a chest of wood, of 2 cubic metre capacity, with hollow walls filled with dry sawdust, this being again placed in a room of nearly constant temperature. Similar precautions were also adopted with the experiments verifying Ficks' law, already detailed in this paper.

It will be seen from this table that the result of the experiments demonstrate to the eye a fundamental law of diffusion, common not only to material particles, but to the imponderable agents, heat and electricity. Sir W. Thomson has also pointed out to me that, according to theory, and supposing the coefficient of diffusion is not variable, the heights of the columns in Table I. should have been identical, provided the acid had been regulated of varying densities to correspond with the alkali.

Further experiments detailed in Table II. were then made which confirm this anticipation,* or rather, which show that if there is any variation in the coefficient it must be small.

Experiments were also made with caustic soda (NaHO), which are recorded in Table III., and I have found that the apparatus can also be made available for measuring the diffusibility of acids by diffusing them into slightly ammoniacal water coloured yellow with methyl orange.† We are thus supplied with a new method of ascertaining the diffusibility of a large number of chemical compounds, and also a method of checking the accuracy of the burette method of determining diffusibility, which I described in the *Phil. Mag.* in January last. The instrument may be also constructed on a larger scale for lecture demonstrations.

* The difference between the heights was reduced to 6 millimetres instead of 31. See also confirmatory experiments detailed in Table IV., added September 1887.

† See Table IV. for details.

Table I.—*Diffusion of Caustic Potash Solutions of Unequal Densities into very Dilute Hydrochloric Acid of Constant Density at a Temperature of 16° C.*

	Times of Diffusion in Days of 24 hours,	5	10	15	20	25	30
A. Reservoir contained 330 mgs. of caustic potash (KHO) per c.c., being 2275 times stronger than at the point where the reddened acid turned yellow (as determined by titration of the acid).	Actual height of diffused column in millimetres,	170	241	293	327	376	412
	Height *calculated* after the first 5 days, in ratio of square root of times,	...	240	295	340	380	416
B. Reservoir contained 185 mgs. of caustic potash (KHO) per c.c., being 1280 times stronger than at the point where the reddened acid turned yellow.	Actual height of diffused column in millimetres,	156	219	267	309	347	381
	Height *calculated* as above,	...	221	270	312	349	382
C. Reservoir contained 100 mgs. per c.c., being 700 times stronger than at the point where the reddened acid turned yellow.	Actual height of diffused column in millimetres,	145	205	250	291	322	352
	Height *calculated* as above,	...	205	251	290	324	355

TABLE II.—*Diffusion of Caustic Potash of Different Densities into very Dilute Hydrochloric Acid of Densities to correspond at a Temperature of* 16° C.

Times of Diffusion in Days of 24 hours,		5	10	15	20	25	30
A_2. Reservoir contained 307 mgs. of caustic potash per c.c., and at the point it was reddened with the acid ·426 mg. per c.c., equal to $\frac{1}{720}$ of initial strength.	Actual height of the diffused column in mm.,	147	208	256	297	332	362
	Height calculated after 1st five days in ratio of square root of times in mm.,	...	208	255	294	329	360
B_2. Reservoir contained 195 mgs. of caustic potash per c.c., and at the point it was reddened with the acid ·426 mg. per c.c., or $\frac{1}{677}$ of initial strength.	Actual height in mm.,	144	204	251	290	328	356
	Height calculated as above in mm.,	...	204	249	288	322	353

TABLE III.—*Diffusion of Caustic Soda.*

Times of Diffusion in Days of 24 hours,		5	10	15	20	25	30
C_2. Reservoir contained 285 mgs. caustic soda per c.c., and at the point it was reddened by the acid ·426 mg. per c.c., or $\frac{1}{669}$ of initial strength.	Actual height in mm.,	122	173	212	246	274	301
	Height calculated as above in mm.,	...	173	211	244	273	299

TABLE IV.—[Added Sept. 20.]—(*a*) *Confirmatory of* TABLE II. *Temp.* 16° C.

Times of Diffusion in Days of 24 hours,		5	10	15	20
A_3 Reservoir contained 392 mgs. of caustic potash per c.c., and at the point it was reddened with the acid ·561 mg. per c.c., equal to $\frac{1}{700}$ of initial strength.	Actual height of diffused column in mm.,	149	210	256	297
	Calculated,	...	211	258	298
B_3 Reservoir contained 200 mgs. of caustic potash per c.c., and at the point it was reddened with acid ·289 mg. per c.c., or about $\frac{1}{700}$ of initial strength.	Actual height in mm.,	145	205	250	289
	Calculated,	...	205	251	290

(*b*) *Diffusion of Hydrochloric, Nitric, and Sulphuric Acids into very dilute Ammonia coloured Yellow with Methyl Orange. Temp.* 16° C.

Times of Diffusion in Days of 24 hours,		5	10	15	21	34	36	43
A_4 Reservoir contained 216 mgs. of hydrochloric acid per c.c., and at the point it was turned yellow ·311 mg. per c.c., $=\frac{1}{700}$ of initial strength.	Actual height of diffused column in mm.,	167	242	290	344	435	448	490
	Calculated,	...	237	290	343	436	449	490
B_4 Reservoir contained 377 mgs. of nitric acid per c.c., and at the point it was turned yellow ·539 mg. per c.c., $=\frac{1}{700}$ of initial strength.	Actual height in mm.,	158	228	274	325	410	422	463
	Calculated,	...	224	270	325	413	425	463
C_4 Reservoir contained 289 mgs. of sulphuric acid per c.c., and at the point it was turned yellow ·410 mg. $=\frac{1}{700}$ of initial strength.	Actual height in mm.,	125	182	220	262	321	340	370
	Calculated,	...	177	217	257	326	337	367

5. On the Minute Structure of the Eye in certain Cymothoidæ. By Frank E. Beddard, Esq., M.A., F.Z.S.

6. On the Mean Height of the Land of the Globe. By John Murray, Esq.

7. The *Chætopoda Sedentaria* of the Firth of Forth. By J. T. Cunningham, Esq., B.A.

Monday, 18th July 1887.

Sheriff FORBES IRVINE, Vice-President, in the Chair.

The Chairman intimated the foundation by Dr Gunning of the *Victoria Jubilee Prize*, and the conditions of award which have been approved by the Donor, and added that the Prize of One Hundred Guineas from this source had been this year awarded by the Council to Sir William Thomson, for a remarkable series of papers on Hydrokinetics, especially of Waves and Vortices, forming some of the most valuable that have been communicated to the Society.

The following Communications were read :—

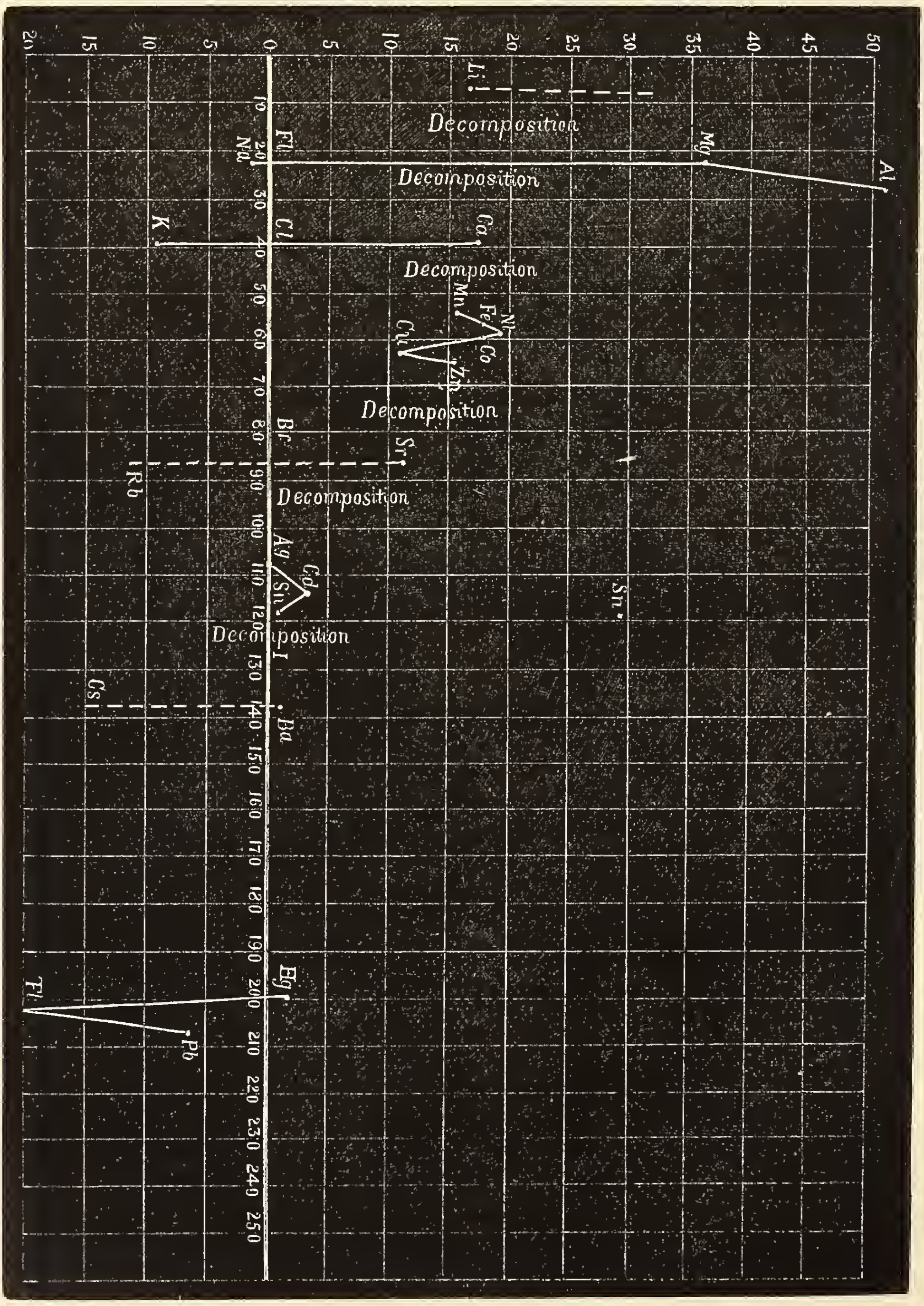
1. Laws of Solution. Part II. By W. Durham, Esq.

From the note in my former paper on the above subject it is easy to deduce the following formula, which expresses the relations between the heats of chemical combination and the heats of solution :—

$$\underset{\text{Heat of Combination.}}{\left\{\begin{matrix}[M,X^2]\\ -[H^2,X^2,Aq]\end{matrix}\right\}}=\underset{\text{Heat of Combination.}}{\left\{\begin{matrix}[M,O,Aq]\\ -[H^2O]\end{matrix}\right\}}+\underset{\text{Heat of Neutrality.}}{[MOAq,H^2X^2Aq]}\pm\underset{\text{Heat of Solution.}}{\{MX^2,Aq\mp\}}.$$

This formula is perfectly general for chlorides, bromides, iodides, sulphates, and nitrates, and whether the oxides and salts are soluble or insoluble. It shows that the heats of solution pass from negative

to positive values through zero when the salts are insoluble. As in soluble salts the heat of neutralisation is practically constant, it follows that the heats of solution vary with the relative variations of [M,X²] and [M,O,Aq] involving definite chemical actions. We should expect, therefore, that the heats of solution would vary in a periodic manner with the nature of the elements, as with other chemical phenomena. The following diagram, representing the heat of solution of the chlorides, although defective in many places from want of data, seems distinctly to show that this is so, and that solution is a periodic function of the weights of the elements.



In this diagram there are several points worthy of particular notice.

1. Although given to illustrate solution it would equally well illustrate the relations between [M,X^2] and [M,O,Aq], which are relations of chemical affinity.
2. The rapid rise of heat of solution after Na and K. I have not data for Rb and Cs, but have no doubt they would exhibit analogous relations, and have connected them by dotted lines to Sr and Ba.
3. After each rapid rise there are several chlorides which are more or less decomposed by solution. These chlorides occur between Li and Fl, Al and Cl, Ca and Mn, Zn and Br, Sr and Rh, and Sn and I, perfectly regular recurring phenomena.
4. The peculiar nature of the curve between Mn and Zn noticed in many other phenomena, and especially the sudden rise from Cu to Zn, this latter relation is repeated between Ag and Cd. Now, it is remarkable that the ătomic weights of Cu and Zn are almost exactly as much lower than the atomic weight of Br as those of Ag and Cd are than that of I, the next negative element.
5. The first maximum point is at Al, whose atomic weight is almost exactly midway between the atomic weights of Fl and Cl. The next maximum is Ni, with atomic weight between Cl and Br. The third maximum should be at Rh, but data are wanting.
6. The curve between Hg, Tl, and Pb suggests a repetition of the curve between Ni, Cu, and Zn.
7. The remarkably regular relations between Ca, Sr, and Ba, whose chemical similarity is well known.

If we pass from chlorides to bromides or iodides the change in the heat of solution can be represented by a very simple formula. For instance, the change from chlorides to bromides is as follows :—

$$\overset{\text{Heat of Combination.}}{\left\{\begin{matrix}[M,Cl^2] \\ -[H^2,Cl^2Aq]\end{matrix}\right\} - \left\{\begin{matrix}[M,Br^2] \\ -[H^2,Br^2,Aq]\end{matrix}\right\}} = \overset{\text{Heat of Solution.}}{[MBr^2,Aq]-[MCl^2,Aq]}.$$

That is to say, the heats of solution of any metallic chloride and bromide vary inversely as the difference between the excess of the heat of combination of the metallic chloride over the heat of combination of hydrogen chloride in water, and the excess of the heat of combination of the metallic bromide over the heat of combination of the hydrogen bromide in water. The following examples will make this plain :—

Heat of Combination.				Heat of Solution.
$[Ca,Cl^2]$	$= 169820$			17410
$-[H^2,Cl^2,Aq]$	$= 78630$			
		91190		
$[Ca,Br^2]$	$= 140850$			24510
$-[H^2,Br^2Aq]$	56760			
		84090		
Difference	+	7100	Difference −	7100
$[Sr,Cl^2]$	$= 184550$			11140
$-[H^2,Cl^2,Aq]$	78630			
		105920		
$[Sr,Br^2]$	157700			16110
$-[H^2,Br^2,Aq]$	56760			
		100940		
Difference	+	4980	−	4970
$[Ba,Cl^2]$	194740			2070
$-[H^2,Cl^2Aq]$	78630			
		116110		
$[BaBr^2]$	169960			4980
$-[H^2,Br^2,Aq]$	56760			
		113200		
Difference	+	2910	Difference −	2910

We again see from these results how intimately heat of solution is related to heat of chemical combination. Whenever an element develops less energy in combination with bromine than with chlorine relatively to the hydrogen compounds of these same negative elements, the energy is not lost; it immediately appears in the heat of solution. It is worthy of note also how regularly the difference increases by about 2000 units as we pass from the barium to the strontium and calcium salts.

Perfectly analogous results are obtained on changing the positive element of the salt instead of the negative. In every case we find the heat of solution regulated by the chemical affinities (as measured by heat) of the elements. Another instructive instance of the relations of chemical affinity and solution is found in the double salts of the form MSO_4, $R''SO_4$, $6H^2O$, where M forms crystals of the composition MSO_47H_2O. In the double salts $R''SO_4$ takes the place of one molecule of H_2O, and develops more or less heat in so doing. Now the thermal results of solution of the double salt seems to indicate that decomposition is brought about.

Consider the following :—

Heats of Combination.

$$
\begin{array}{lcr}
[ZnSO_4,K_2SO,6H_2O] & = & 23950 \\
[ZnSO_4,7H_2O] & = & 22690 \\
\hline
\text{Difference} & = & +1260
\end{array}
$$

Heats of Solution.

$$
\begin{array}{lrr}
[ZnSO_4K_2SO_46H_2OAq] & & = -11900 \\
[ZnSO_47H_2O,Aq] & = -4260 & \\
[K_2SO_4,Aq] & -6380 & \\
 & & -10640 \\
\hline
\text{Difference} & & -\ 1260
\end{array}
$$

In fact, putting the double crystalline salt into solution brings the mixture to exactly the same thermal state as if the constituent sulphates were separately dissolved in water.

(*Added July* 16, 1887.)

It has been said that no argument as to residual affinity can be based on thermal results because we do not know the fundamental units, but it appears to me there is no force whatever in this objection, as we are not dealing with absolute affinity but only with differences, and thermal chemistry is particularly fitted to show these differences. Thus, for instance, Cl in combining with Sr develops 10190 units less heat than it does when combining with Ba. Now the question is, What becomes of these 10190 units? Are they lost entirely, or is there residual affinity left in $SrCl_2$ to that amount?

The heat of solution appears to me to answer this question at once when we take into account that Sr acts upon O with less energy than Ba does. Thus—

$[Ba,Cl^2]$ − $[Sr,Cl^2]$	= 10190		$[BaCl^2,Aq]$	2070
$[Ba,O,Aq]$ − $[Sr,O,Aq]$	980		$[SrCl^2Aq]$	11140
Difference	+ 9210			− 9070

or, in other words, the heat deficient in the combination $[Sr,Cl^2]$ as compared with $[Ba,Cl^2]$ appears in the extra heat of solution of $SrCl_2$ as compared with $BaCl_2$. This is not an isolated case, but appears in every chloride, so that if the heat of combination with O of the various metals was constant, the heat of solution would vary inversely in every case as the heat of combination. The slight difference of 140 units in above case appears as a difference in the heats of neutrality of the two salts.

We can, however, get rid of all consideration of the heats of combination with oxygen by the help of the formula already given,

$$\left\{\begin{matrix} M''Cl^2 \\ -H^2,Cl^2,Aq \end{matrix}\right\} - \left\{\begin{matrix} M''Br^2 \\ -H^2,Br^2,Aq \end{matrix}\right\} = \{M''Br^2,Aq\} - \{M''Cl^2,Aq\},$$

for we can take the metals in pairs and have on the one side of the equation only the differences of the heats of combination, and on the other the differences of the heats of solution, and we shall see they are exactly complementary; as the one increases the other decreases. That is to say, the more energy that is run down to the form of heat in the formation of any salt the less energy is there left to run down to the same form in solution, and *vice versa*. In fact, the one is entirely dependent on the other, and it seems to me absolutely certain that if the one phenomenon is due to chemical affinity, so is the other. The following list, taken at random, of bromides, chlorides, sulphates, and nitrates will show this :—

Heat of Combination.			Difference	Heat of Solution.	Difference.
$[Ba,Cl^2]$	− $[Ba,Br^2]$	= 24780		− 2910	
$[Sr,Cl^2]$	− $[Sr,Br^2]$	= 26850		− 4970	
			− 2070		+ 2060
$[Sr,Cl^2]$	− $[Sr,Br^2]$	= 26850		− 4970	
$[Ca,Cl^2]$	− $[Ca,Br^2]$	= 28970		− 7100	
			− 2120		+ 2120

Heat of Combination.		Difference.	Heat of Solution.	Difference.
$[Zn,SO^4] - [Zn,Cl^2]$	$= 132860$		$+ 2800$	
$[Cd,SO^4] - [Cd,Cl^2]$	$= 128310$		$+ 7700$	
		$+ 4550$		$- 4900$
$[Ca,SO^4] - [Ca,Br^2]$	$= 177520$		$- 20070$	
$[Zn,SO^4] - [Zn,Br^2]$	$= 154140$		$- 3400$	
		$+ 23380$		$- 23470$
$[Li^2,N^2,O^6] - [Li^2,Cl^2]$	39620		$- 16280$	
$[Ca,N^2,O^6] - [Ca,Cl^2]$	36820		$- 13460$	
		$+ 2800$		$- 2820$
$[Sr,N^2,O^6] - [Sr,Cl^2]$	39280		$- 15760$	
$[Na^2N^2,O^6] - [Na^2,Cl^2]$	31120		$- 7700$	
		$+ 8160$		$- 8060$

The only exceptions to above rule occur when the heat of neutralisation differs very greatly from the average, but this difference exactly accounts for the discrepancy. Thus in the case of $BaSO_4$, the heat of neutralisation is abnormal to the extent in which it differs from above law.

2. On the Partition of Energy between the Translatory and Rotational Motions of a set of non-homogeneous Elastic Spheres. By Professor W. Burnside. Communicated by Professor Tait.

3. On the Salinity, Temperature, &c. of the Firth of Forth. By H. R. Mill, D.Sc.

4. The Direct Measurement of the Peltier Effect. By Albert Campbell, B.A. Communicated by Professor Tait. (Plate XIII.)

The researches described in this paper had for their object the direct measurement of the Peltier effect, with a view to verify the hypotheses regarding it and the laws deduced from these. Hitherto it has been a pure assumption that the Peltier effect vanishes at the neutral point. As this important assumption forms part of the basis of the received theory of thermoelectricity, to prove it was one of the main objects of these experiments.

Very recently, Signor Batelli (*Atti della Società Reale de Torino,*

May 1887) has attempted to prove experimentally the vanishing of the Peltier effect at the neutral point in the case of certain alloys, whose neutral points with lead are at temperatures not much above the ordinary temperature of the air. In his method the Peltier effects produced by a current at the junctions of a thermopile (of the alloys A and B) are measured by the thermopile itself. Now at the neutral point of A and B, the thermoelectric power of the thermopile becomes zero, and hence the pile becomes, at that temperature, a wholly inefficient means of measuring any heating or cooling effect. Therefore Signor Batelli's experiments by no means establish the vanishing of the Peltier effect at the neutral point.

In the following experiments, measurements were made of the Peltier effect both above and below the neutral point, and these were compared with the values calculated from the directly observed neutral point, taking Professor Tait's supposition $\sigma = kt$. Thus if t_0 be the absolute temperature of the neutral point of any two metals (whose lines are straight), then, according to the theory of the thermoelectric diagram, their Peltier effect at absolute temperature t is proportional to $t(t - t_0)$. The neutral points of the specimens used were accordingly found in the ordinary way by heating up a junction in oil, and these were the only data necessary in finding the calculated values with which the observed values were compared.

The apparatus used was a modification of that described by the writer in a paper read before this Society in 1882; it was constructed in the following manner:—

The ends of two strips of sheet iron (say) were soldered (or in the case of some metals, brazed) to the lower ends of an arch of sheet cadmium (say). The other ends of the iron strips had several copper wires soldered to them, leading to a rocking commutator connected with four cells of a Thomson's "tray-Daniell" battery. In this circuit was also included a Helmholtz tangent galvanometer, which served to measure the battery current. The differences of temperatures of the iron-cadmium junctions were measured by means of an iron-German-silver thermopile (of 16 or 20 junctions), bent into the shape of an arch, whose ends were inserted in the trenches between the iron and cadmium, being insulated from them by thin asbestos paper. The whole was packed

tightly in asbestos wool in the middle of a series of copper boxes or tubes, separated from one another by asbestos packing. A thermometer was inserted with its bulb as near to the junctions as possible. By properly arranging Bunsen burners underneath, it was possible to maintain the junctions at a constant and uniform temperature. Throughout the whole series of experiments, the mercury thermometers employed were regularly compared (at the ordinary temperature) with a standard one which was never heated up, and which had been compared with a Kew standard. All the temperature readings were thus corrected.

The following was the usual mode of observation:—When the temperature had become sufficiently uniform (which was often not until several hours after the heating was begun), the current was sent in the positive direction for 30 seconds, then broken for 30 seconds, put on in the negative direction for 30 more, broken for 30 more, and so on, for about 10 minutes. The deflections of the mirror galvanometer (connected with the measuring thermopile) and of the Helmholtz galvanometer were observed simultaneously at the end of each period of 30 seconds. If the scale-readings of the mirror galvanometer be $a_1, a_2, a_3, a_4, a_5, \ldots\ldots\ldots$ (a_1 being the original zero-point), then the average of $a_1 - a_2$, $a_4 - a_3$, $- a_6 - a_5$, $\ldots\ldots\ldots$ is taken as the measure of the temperature difference caused by the given current (in 30 seconds).

Since the specific heats of most metals increase considerably as the temperature rises, it is necessary, in comparing the Peltier effects at two temperatures, to make a correction for the change in specific heats. In the absence of definite measurements, this increase in specific heat was taken as approximately $= \frac{1}{700}$ per degree centigrade for iron and nickel, and $= \frac{1}{1000}$ per degree for cadmium, zinc, and German silver. This correction was reckoned from 20° C.

The correctness of the method was tested by applying it to prove that the Peltier effect is proportional to the strength of the current. As was to be expected, the temperature nearly always rose slowly during the ten minutes' observations (owing to the Joule effect); a small correction was applied for this. Table I. gives some of the measurements of the Peltier effect for different current-strengths. The first three sets are for iron-cadmium, and the fourth for iron-zinc.

TABLE I.

Temperature C.	Mean Thermopile Deflection.	Mean Current Deflection.	Observed Ratio of Peltier Effects.	Ratio of Currents.
18°·7 to 18°·9 19°·1 to 19°·3	265·9 129·7	798 398	2·044	2·005
95°·5 to 95°·9 95°·9 to 96°·2	121·6 52·25	877·9 415·9	2·143	2·106
95°·7 to 96°·2 96°·2 to 96°·7	72·17 36·13	718·1 375·9	1·899	1·910
20°·7 to 21°·3 21°·3 to 21°·3	174·0 97·9	206·7 116·7	1·796	1·772

The following are some of the measurements at different temperatures:—

Iron and Cadmium.

Iron and cadmium were selected as having a comparatively low neutral point. The specimens used were ordinary sheet iron and tolerably pure cadmium. The neutral point found by heating up a junction in oil was 144°·0 C. At 144° C. the direct measurement showed *absolutely no Peltier effect*, a small irreversible effect alone being visible; while the same current gave a mean deflection of 308 scale divisions at 21° C, and −240 divisions at 199°·1 C. Also at 139°·0 the Peltier effect was small, but still of the same sign as that at 21° C. Table II. gives some measurements at other temperatures, the last column being the ratio of the Peltier effects

TABLE II.

No.	Low Temperature.	High Temperature.	Observed Ratio.	Calculated Ratio.
1	18°·8	66°·9	1·475	1·447
2	17°·0	72°·6	1·782	1·878
3	18°·8	95°·7	2·073	2·050
4	19°·2	95°·1	2·051	2·024
5	17°·8	199°·1	−1·271	−1·410

(low temperature to high) calculated from neutral point 144° C. In Nos. 3 and 4 the temperature was kept uniform by means of a double steam-jacket surrounding a small copper box in which the junctions and thermopile were packed. By this means the temperature could be kept *very* steady and uniform.

Zinc and Iron.

The metals here used were ordinary sheet zinc and thin tinplate. In order that the junctions might stand a temperature above 200° C., they were soldered with a suitable alloy of zinc and tin. Thin strips were cut from the same specimens, and the neutral point was found (by heating their junction in oil) to be 196°·7 C. The directly observed Peltier effect was found to vanish about 204° C. The temperature, however, was falling slowly at the time, which would account for this disagreement. Table III. gives a measurement taken just before the temperature had fallen to 204°. The last column gives the ratio calculated from 204° as neutral point :—

TABLE III.

Low Temperature.	High Temperature.	Ratio Observed.	Ratio Calculated from 204° C.
22°·5	215°	− 9·738	− 9·80

Nickel and German Silver.

The peculiar form of the nickel line between 150° and 300° C. made it interesting to find whether the Peltier effect between nickel and any other metal (or alloy) agrees with the theory between these temperatures. German silver was chosen as being an alloy whose Peltier effect with nickel ought to vary in a striking manner. According to the thermoelectric diagram, their Peltier effect divided by the absolute temperature should remain constant till at least 150° C., and then decrease uniformly till it vanishes at the neutral point; beyond this it should change sign and increase till about 300° C. (above which it should probably remain constant for some distance).

The usual form of apparatus was used, but in this case the

German silver strips had to be *brazed* to the nickel, that the junctions might stand the high temperatures. The neutral point of the pair was found as usual by long strips cut from the same sheets. Since, in the brazing, the metals had to be brought to a high temperature, these long strips, as well as the pieces that were to be brazed, were all annealed by heating to bright redness and slowly cooling.

In order to measure the thermoelectric power of nickel German silver at the various temperatures, the following method was used:—The current, instead of being sent through Ni-Arg junctions, was sent through the measuring (FeArg) thermopile, and the Peltier effects caused in the thermopile junctions measured by connecting the nickel and German silver strip to the galvanometer. The deflection here in the galvanometer would be proportional to the product of the Peltier effect of FeArg, and the thermoelectric power of NiArg. Now, it has been shown by former experiments by the writer* that the Peltier effect of FeArg varies as the absolute temperature. Hence we can at once find the thermoelectric power of the NiArg at any temperature.

The junctions in this case were packed in four thick copper tubes, one inside the other, with asbestos wool between. As this arrangement gave a very *uniform* temperature at the junctions, most of the readings were taken with the temperature rising slowly, except those at 17°·8, 19°·0, 23°·0, and 254°·3 C., when the temperature was fairly steady. The temperatures above 285° C. are not very certain. For convenience in heating, the tubes had to be almost horizontal. This, unfortunately, caused the mercury in the thermometer to boil at a temperature much below its boiling point at atmospheric pressure. The readings 340° and 330°, therefore, are only estimated.

In Table IV. are given some of the measurements of the Peltier effect in NiArg. The second column gives D/Ct, where D is the FeArg thermopile deflection, C the battery current through NiArg, and t the absolute temperature. In Table V. are the measurements of the thermoelectric power of NiArg (by sending the current through the FeArg thermopile). The second column gives the

* *Proc. Roy. Soc. Edin.*, 1882. If we introduce the specific-heat correction (·12 % per ° C.) we get a much nearer agreement than that shown there.

values of D_1/C_1t, where D_1 = deflection in NiArg circuit, C_1 = battery current, and t = absolute temperature. In both tables the correction for increase in specific heat was introduced.

TABLE IV.

(Battery Current through NiArg.)

Temp. C.	$\frac{D}{Ct}$	Temp. C.	$\frac{D}{Ct}$
17°·8	·0683	220°·0	·0321
19°·0	·0691	222°·0	·0304
64°·1	·0637	223°·8	·0308
69°·0	·0689	225°·3	·0293
70°·0	·0686	226°·9	·0314
75°·7	·0657	228°·5	·0310
81°·8	·0657	230°·1	·0267
98°·0	·0649	231°·9	·0258
101°·0	·0659	233°·2	·0253
118°·8	·0632	234°·5	·0234
121°·0	·0657	235°·8	·0235
124°·9	·0652	237°·2	·0223
206°·2	·0471	237°·9	·0190
208°·8	·0439	254°·3	– ·0006
210°·7	·0431	283°	– ·0153
212°·9	·0435	284°	– ·0208
214°·8	·0390	285°·4	– ·0228
216°·4	·0369	300°·?	– ·0640
218°·4	·0335		

TABLE V.

(Battery Current through FeArg pile.)

Temp. C.	$\frac{D_1}{C_1t}$
23°·0	·134
114°·5	·143
153°·3	·134
157° 3	·123
166°·3	·0982
169°·9	·1023
173°·8	·0975
177°·4	·0915
181°·0	·0925
210°·8	·0740
254°·3	– ·0012
273°·0	– ·0204
275°·6	– ·0392
278°·0	– ·0527
290° ?	– ·0614
302°	– ·0879
330° ?	– ·16

The neutral point of the NiArg, found by heating up a junction in oil, was 250°·6 C. The curve drawn from Table IV. shows a vanishing of the *Peltier effect* at 253°·7, while from Table V. the *thermoelectric power* vanishes at 253°·4. No measurement was made exactly *at* the directly observed neutral point, but the small-

ness of the effect at 254°·3 is pretty strong evidence that it vanishes *at most* a degree or two from the directly found neutral point.

The numbers in Tables IV. and V. agree fairly well with the fact that the nickel and German silver lines are parallel up to at least 150° C., and that between its two bends the nickel line is straight. Above 250° C. the deflections of the FeArg thermopile cannot be taken as accurately measuring the small temperature differences, for the Arg line is no longer parallel to the iron line. The numbers have not been corrected for this. The very small correction due to the resistance of the measuring thermopile increasing with the temperature has been neglected.

In conclusion, I must express my thanks to Professor Tait, in whose laboratory these investigations were carried out, for kindly placing at my disposal much of the necessary apparatus, as well as for his ever-ready advice. I also desire to express my thanks to Messrs J. T. Morrison and A. H. Mackenzie for their most valuable aid, and to Messrs Shand and Buchan for their kind help in the determination of the neutral points.

(*Added December* 1887.)

§ 1. *Description of Apparatus.*

The above investigations were continued by the writer and Mr J. T. Morrison, at the laboratory of the former, near Londonderry. As the galvanometers were arranged so as to give much more delicate measurements than those in the preceding experiments, a few words of description are here necessary. The galvanometer connected with the measuring thermopile had a lens of about 10 feet focal length. Of the galvanometers used for measuring the battery current, the Helmholtz had a lens of 12 feet focal length, and the other mirror galvanometer one of 6 feet. The scales, each 1 metre in length, were of translucent paper, stretched between two boards whose edges were circular arcs of the proper radius. The divisions were 2 millimetres each, and could be read to $\frac{1}{10}$ths. In order that the battery current might be more steady, the commutator was so arranged that the moment the current was broken it was immediately short-circuited through a similar resistance. For greater accuracy

and convenience, the high temperature thermometer was always read with a telescope.

At first it was thought desirable, instead of employing the Helmholtz galvanometer, to measure the battery current in all cases by the Peltier effect in a standard set of iron-German-silver junctions, which would, it was hoped, integrate the current during the period of time for which it was run. Although this arrangement was subsequently discarded as a current-measurer, some interesting results were obtained from it. It consisted of 23 squares of sheet iron and German silver (each 3 cms. square), soldered, three by three, into seven strips, which were then soldered to one another in zigzag form. Along the middle junctions were placed the ends (100 in all) of 7 iron German silver thermopiles, insulated from the junctions by thin paper. The whole was wrapped tightly in wadding, and placed in a tin box surrounded by cold water. A thermometer was inserted with its bulb touching the metals. The current was sent through the iron-German-silver zigzag, and the Peltier effects measured by the thermopiles.

§ 2. *Time Curves of Peltier Effect.*

In order to investigate the rate at which the growing Peltier effect temperature-difference showed itself by the thermopile deflection, the following simple chronographic method was adopted. In this a number of observers were made use of. Observer A held a watch to his ear, and counted half seconds aloud in exact time with its beats, also making and breaking the battery circuit at exactly the proper times. Observer B, watching the galvanometer scale, said sharply the syllables, "Tic, tac, to, tee, tic, tac, to, tee," and so on, as the light-spot passed over certain prearranged divisions of the scale. Eight other observers noted down the numbers after which they heard the tic, tac, to, or tee, one of these words being allotted to each pair of observers. This method proved wonderfully accurate. The pairs of observers very seldom disagreed by half a second, although the numbers had to be noted very rapidly. The results of some of the measurements are given in Tables VI., VII., and VIII., which are for three different current-strengths. In these the current was put on for 100 half-seconds, broken for 100, put on in

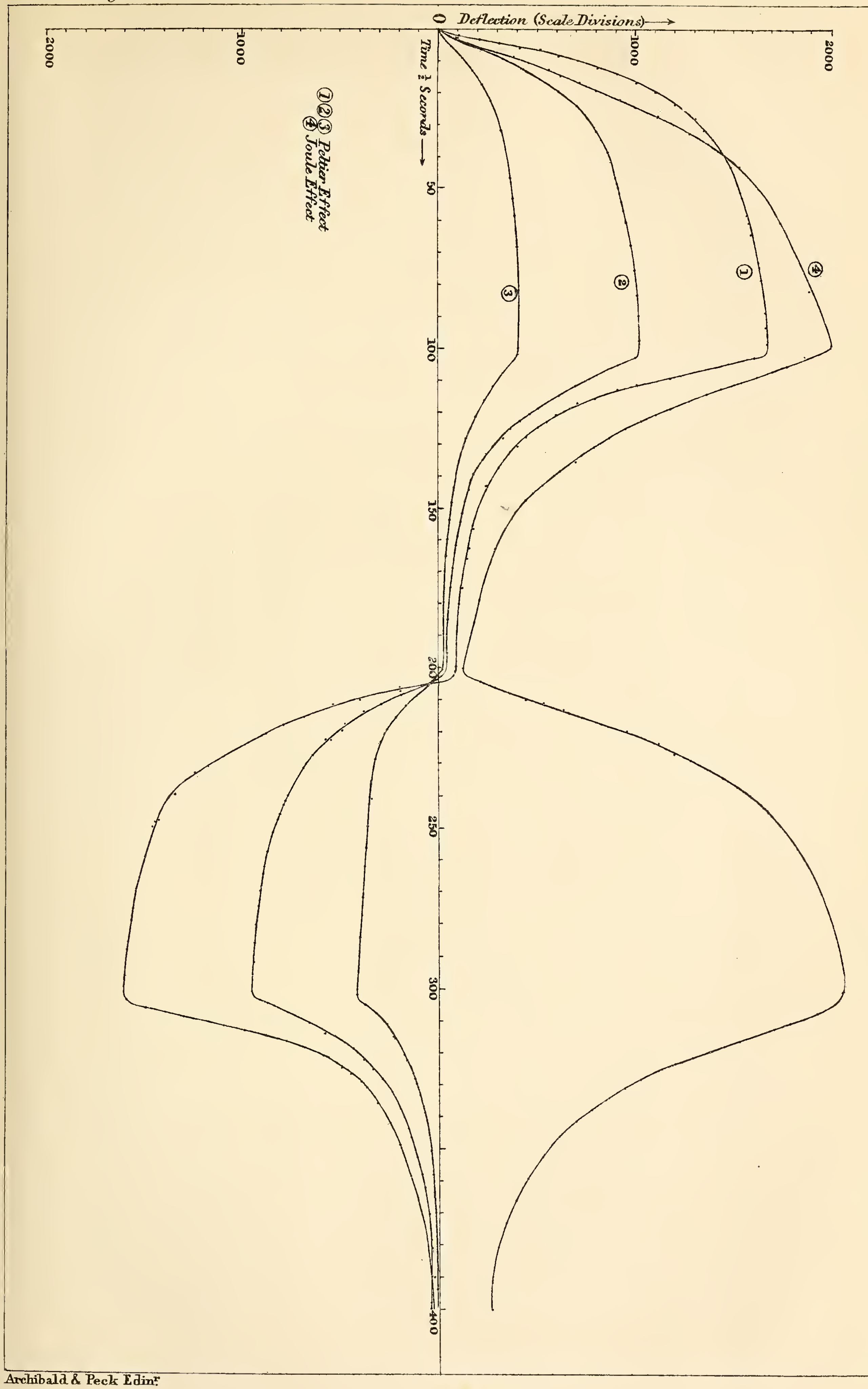
the opposite direction for 100, and broken again for 100 more. These periods were chosen so that the deflection might become nearly constant before the current was broken. The current, which was from 2 or 3 Bunsen cells, was very nearly constant throughout the 200 seconds. The curves (1), (2), and (3) in the diagram are drawn from these tables, the abscissa being the time (in half-seconds) and the ordinate the galvanometer deflection.

Table VI. Curve (1).

Time, ½ Seconds.	Deflection.	Time, ½ Seconds.	Deflection.	Time, ½ Seconds.	Deflection.	Time, ½ Seconds.	Deflection.
0	0	92	1660	200	Reverse current made.	283	−1590
3	100	94	1680	201	100	287	−1600
4	200	99	1690	204	0	293	−1610
5·5	300	100	Current broken.	205	− 100	298·5	−1620
7	400	104	1600	207	− 200	300	Current broken.
8	500	106	1400	210	− 400	302	−1600
10	600	111·5	1000	210	− 500	304	−1500
11	700	113	900	213	− 600	307	−1300
13	800	116	800	215	− 700	309·5	−1100
15	900	117	700	217	− 800	311	−1000
18	1000	121	600	223	−1000	313	− 900
21	1100	123	550	227	−1100	315	− 800
25	1200	125	500	229	−1200	317	− 700
29	1300	128	450	234	−1300	319	− 600
33	1350	130	400	239	−1350	323	− 500
36	1400	133	350	244	−1400	325	− 450
40	1450	137	300	246	−1450	327	− 400
45	1500	143	250	248	−1470	329	− 350
52	1550	149	200	257	−1510	336	− 300
53	1560	162	150	261	−1520	340	− 250
59	1580	165	140	263	−1530	347	− 200
61	1590	167	130	265	−1540	355	− 150
65	1600	174	120	268	−1550	376	− 100
67	1620	179	110	274	−1570	381	− 50
72	1630	187	100	278	−1580	390	− 40
75	1640						
78	1650						

§ 3. *Thermoelectric Power of Iron German Silver.*

As in all the above experiments the measurements were made by means of iron-German-silver thermopiles, it was highly important that the thermoelectric power of the iron and German silver used should be carefully measured throughout the range of temperatures at which the piles were used. For this purpose, the FeArg junction, instead of being heated in oil, was packed in asbestos



Archibald & Peck Edin^r.

Table VII. Curve (2).

Time, ½ Seconds.	Deflection.	Time, ½ Seconds.	Deflection.	Time, ½ Seconds.	Deflection.	Time, ½ Seconds.	Deflection.
0	Current	112	709	210·7	−291	300	Current
	made.	115	609	212·5	−391		broken.
1·5	9	118	509	218	−491	302·5	−891
4	109	120	459	221·4	−541	306	−791
7	209	122·5	409	222	−591	309	−691
10	309	125·5	359	226	−641	312	−591
13	409	129	309	229·5	−691	315	−491
16·5	509	134·5	209	235	−741	318	−441
20·5	609	135	199	236	−751	320·2	−391
25	709	137	189	237	−761	323	−341
28	759	139·5	179	239	−771	326	−291
32	809	141	169	240	−781	330	−241
38·5	859	144	159	241	−791	335·5	−191
475	909	144	149	243	−801	337·5	−181
49·7	929	148	139	244	−811	339	−171
51·5	939	151·5	119	246	−821	340	−161
54·5	949	154	109	248	−831	342	−151
57·5	959	158·5	89	250	−841	344	−141
61	969	161	79	251·5	−851	345	−131
64·5	979	168	69	256	−861	347·5	−121
76·5	999	174	59	258	−871	350·5	−111
83·4	1009	184	49	261	−881	352·5	−101
90	1019	194·5	39	263	−891	355·5	− 91
98	1029		Reverse	268	−901	360	− 81
100	Current	200	current	272·7	−911	364·5	− 71
	broken.		made.	277	−921	371·2	− 61
103	1009	2025	9	284·5	−931	377·5	− 51
106	909	205	− 91	290	−941	392	− 41
108·7	809	208	−191	299·7	−951		

Table VIII. Curve (3).

Time, ½ Seconds.	Deflection.	Time, ½ Seconds.	Deflection.	Time, ½ Seconds.	Deflection.	Time, ½ Seconds.	Deflection.
0	Current	107	323	215·2	−227	314	−227
	made.	112	273	219	−277	319	−177
3·5	23	116	223	227	−327	3207	−167
5·5	73	121	173	236	−337	322	−157
9	123	128	123	241	−347	325	−137
13	173	143	73	243	−357	326·7	−127
·17·7	223	147·7	63	248	−367	328	−117
22·5	273	153·2	53	255	−377	332	− 97
32	323	159	43	260·7	−387	335	− 87
47	373	177	33	270	−397	337·5	− 77
53	383		Reverse	282	−407	339	− 67
59	393	200	current	300	Current	344·5	− 57
68	403		made.		broken.	349·2	− 47
82	413	201	23	303·2	−377	357·5	− 37
98·5	423	201·2	3	306·7	−327	370	− 27
100	Current	208	−127	309·5	−277	394	− 17
	broken.	212	−177				

inside two small copper cylinders, and brought to an almost steady temperature by the well-screened flame of a spirit-lamp beneath. A thermometer was inserted with its bulb touching the junction. The cold junction (well varnished) was kept in a large can of water; the temperature of this seldom varied by more than ·3 of a degree C. Several hours usually elapsed between each reading. The reading at each temperature was the mean of four deflections, two to each side of the scale. Table IX. gives a set of the measurements: t_1 is the cold temperature, t_2 the hot, and D the mean deflection. The third column shows how nearly constant the thermoelectric power remains up to about 250° C.

TABLE IX.

t_1	t_2	$\frac{D}{t_2 - t_1}$	t_1	t_2	$\frac{D}{t_2 - t_1}$
9°·4 C.	63°·5 C.	8·627	9°·5 C.	167°·1 C.	8·739
9°·4	65°·3	8·658	9°·7	192°·3	8·727
9°·3	85°·5	8·734	9°·6	205°·1	8·719
9°·3	127°·1	8·761	9°·7	214°·6	8·715
9°·6	152°·1	8·704	9°·8	233°·6	8·688
9°·5	153°·5	8·675	9°·8	245°·7	8·668

Similar measurements with the sheet iron and German silver used in the experiments described below showed the lines of the specimens to be very nearly parallel. The neutral point of the nickel and German silver used in the experiments tabulated in Tables IV. and V. was carefully redetermined by this method, and found to be 252°·3 C., which agrees with Tables IV. and V. even more closely than the former determination did.

§ 4. *Comparison of Peltier Effect with Thermoelectric Power.*

Further experiments were also made in order to compare the measurements of the Peltier effect with those of the thermoelectric power in the same specimens. The apparatus here used consisted essentially of two thermopiles (one of FeZn and the other of FeArg) of the same size and shape, and having the same number of junctions each. These were arranged, junction to junction, in as symmetrical a manner as possible.

Two forms of this arrangement were used. In the first (*a*) the thermopiles were two broad strips (formed of alternate squares of the metals), bent into zigzags which fitted one within the other. The one zigzag was of iron and zinc, and the other of iron and German silver. The iron used here was thin tinplate, and none of the metals were annealed. This arrangement was not perfectly symmetrical; the second modification (*b*) *was*, however. It consisted of four zigzag thermopiles of 18 junctions each, almost identical in size and form, made of strips of thin sheet metal, 3 cms. long and about 5 mms. broad. Two of them were of FeArg and the other two of FeZn. These were arranged in the form of a square, with their junctions interlaced, the similar piles being at opposite sides of the square. Insulation was ensured by strips of asbestos paper separating the junctions from one another. (The insulation was tested both at high and low temperatures, and was found to be practically perfect.) Clearly this arrangement was perfectly symmetrical. In this case the iron used was thin tinplate which had had the tin almost completely burnt off it at a red heat; the zinc and German silver were also well annealed. In both cases, the copper boxes or tubes in which the piles were heated up were surrounded by asbestos wool and fireclay bricks, and the heating was done by a spirit-lamp carefully shut in from air currents. Nearly all the measurements were made when the temperatures had become *almost perfectly steady.*

The battery current, which was from two Tray-Daniells, was kept as constant as possible by gradually diminishing the resistance of the circuit as the current showed signs of falling. It was measured by a mirror galvanometer doubly shunted by copper shunts. In using both (*a*) and (*b*), the intervals for current, no-current, &c., were chosen of such a length that a permanent state of temperature distribution had been almost reached before the end of each interval. For (*a*) the periods were 60 secs. each, and for (*b*) 90 secs. each. The thermopile measurements were made in much the same manner as in the case of nickel German silver described above [*i.e.*, Peltier effect in pile (1) measured by pile (2), and then Peltier effect in pile (2) measured by pile (1)].

In arrangement (*b*) the resistance of the galvanometer + FeArg thermopiles was 2·053 ohms, while that of galvanometer + FeZn piles was 1·963 ohms. This difference of resistance also diminished

as the temperature rose. As it was the E.M.F.s that were to be compared, the indications of the FeZn piles had to be corrected for this difference of resistance (and also for the change in this difference). In (*a*), as the piles had both such small resistance, this correction was not required.

The specific heat correction was deduced from the measurements described below. Unfortunately no measurement was made in the case of zinc; so the change in specific heat for it was assumed as ·10 % per degree C. If s_1, s_2, s_3 be the specific heats of Fe, Arg, and Zn; w_1, w_2, w_3 the relative weights of the thermopile strips of these metals; a_1, a_2, a_3 the respective percentage increments of the specific heats (per degree C.); then the complete correction (% per deg. C.) has been taken as

$$= \frac{2a_1w_1s_1 + a_2w_2s_2 + a_3w_3s_3}{2w_1s_1 + w_2s_2 + w_3s_3.}.$$

This gives

·104 % per degree C. at 110° C.,
·108 % " at 120° C.,

and so on up to

·136 % per degree C. at 180° C.

From the values within this range of temperature those at lower temperatures were found by extrapolation. The effect of the small amount of brass and solder in the junctions was neglected. The specific heat of German silver was taken as ·10.

In Table X. are the results obtained with arrangement (*a*), and in Table XI. those with arrangement (*b*). In the measurements marked X the current was sent through the FeZn, and the Peltier effect measured by the FeArg; in those marked Y the current was sent through the FeArg and the Peltier effect measured by the FeZn. The third column gives the observed values of $\frac{D}{Ct}$ (corrected as above), where D is the mean thermopile deflection (mean of 6), C the mean current, and t the absolute temperature.

From the following tables it is clear that (within the limits of experimental error) $\frac{D}{Ct}$ *has the same value* in the X as in the Y measurements; but the values of $\frac{D}{Ct}$ found do *not* lie on the curve

$\frac{D}{Ct} = A + Bt$. Now by careful measurements (heating a junction, &c.) the thermoelectric power of the FeArg was found to be quite constant throughout the range of temperatures used. The FeZn line also was found to be straight. Therefore some other correction (possibly for conduction) would have to be introduced if these measurements are to agree, in this respect, with the received theory.

TABLE X.

	Temperature.	$\frac{D}{Ct}$ Observed.	$\frac{D}{Ct}$ Calculated.		Temperature.	$\frac{D}{Ct}$ Observed.	$\frac{D}{Ct}$ Calculated.
X	11°·4 C.	1613	1599	X	74°·3	1304	1322
Y	11°·6	1599	1599	X	81°·0	1275	1279
X	38°·1	1461	1515	Y	106°·8	1079	1080
Y	38°·8	1474	1510	X	109°·0	1053	1061
X	39°·7	1455	1508	Y	116°·6	1039	993
X	72°·9	1332	1332	Y	143°·7	739·8	722
Y	73°·1	1333	1330	X	144°·0	722·6	718·4

TABLE XI.

	Temperature.	$\frac{D}{Ct}$ Observed.	$\frac{D}{Ct}$ Calculated.		Temperature.	$\frac{D}{Ct}$ Observed.	$\frac{D}{Ct}$ Calculated.
X	8°·9 C.	1533	1533	Y	129°·1 C.	699·3	665·9
X	11°·1	1523	1525	X	163°·7	254·9	250·0
X	59°·0	1269	1280	Y	161°·3	251·3	281·2
Y	60°·0	1283	1274	Y	172°·8	147·9	128·3
X	61°·1	1280	1267	X	174°·2	96·9	99·1
X	95°·3	1009	1001	X	180°·6	11·4	19·8
Y	96°·0	988·6	995·0	Y	183°·2	0	− 17·1
X	96°·4	978·6	990·5	X	183°·2	− 28·4	− 17·1
X	129°·5	703·9	661·5	X	191°·5	−123·7	−163·2

The values of $\frac{D}{Ct}$ in Table X. agree approximately with the formula

$$\frac{D}{Ct} = 1622 - 1{\cdot}590\theta - {\cdot}0323\theta^2,$$

where θ = temperature centigrade.

The fourth column in Table X. gives the values calculated from this formula. This would make $\frac{D}{Ct}$ vanish at 200°·8 C. and at −250°·0 C. Now, the directly measured neutral point of the FeZn used is 196°·9 C.

Similarly, the values in Table XI. are (as is shown by the fourth column) in fair agreement with the formula

$$\frac{D}{Ct} = (182 - \theta)(8{\cdot}58 + {\cdot}0315\theta),$$

so that $\frac{D}{Ct}$ vanishes at 182° C. (and would also vanish at $-208{\cdot}8°$ C.). The directly observed neutral point (by heating up junction) was 190°·0 C.

Let us consider now the interpretation of the first clearly proved result, viz., that $\frac{D}{Ct}$ (corrected) has the same value for a given temperature whether the battery current goes through the FeZn pile or the FeArg pile. (Let it be noted that *all the corrections* and *all the conditions* were the same in the corresponding X and Y measurements, so that this result is definitely proved.)

Let the absolute temperature $= t$;
the Peltier effect of FeZn $= tf(t)$;
the Peltier effect of FeArg $= t\phi(t)$;
the thermoelectric power of FeZn $= F(t)$;
and that of FeArg, which by measurement is known to be constant, $= a$.

The above experimental result becomes

$$af(t) = \phi(t) \,.\, F(t) \quad . \quad . \quad . \quad . \quad . \quad . \quad (1).$$

And this is found to be still true (at least to within about 1° C.) when $\phi(t) = 0$, *i.e.*, the Peltier effect, $tf(t)$, vanishes at the neutral point.*

The interpretation of equation (1) is that the Peltier effect in FeZn is equal to the product of *the absolute temperature*, *the thermoelectric power*, and *some function of the absolute temperature* which is the same for all pairs of metals.† That is—

$$\frac{f(t)}{F(t)} = \frac{\phi(t)}{a} = \psi(t) \quad . \quad . \quad . \quad . \quad (2)$$

If it be taken as proved (and it has been to a certain extent) that

* 183° C. (and not 190° C.) is clearly the true neutral point of the FeZn piles, since at 183° their thermoelectric power vanishes.

† Similar measurements with two thermopiles of FeArg and NiArg also warrant this conclusion.

the Peltier effect in FeArg varies as the absolute temperature, the result in equation (1) at once *establishes* the usual theory. In any case, equation (1) perfectly *agrees with* the received theory.

Experiments were also made with the four thermopiles interlaced in the order FeArg, FeArg, FeZn, FeZn, the *alternate* ones being connected. With this difference, everything was done just as in the last described experiments. In Table XII. are the results. The current was passed sometimes through the one pair of piles (FeArg and FeZn) and sometimes through the other pair. These two ways are marked P and Q in the table. The last column gives the values of the square root of $\frac{D}{Ct}$ corrected for change in specific heat, by ·12 % per degree C. [Assumption (1st).] This is not strictly accurate.

TABLE XII.

	Temperature. θ.	Thermopiles Deflection. D.	$\frac{D}{Ct}$	$\sqrt{\frac{D}{Ct}}$ (corrected).
P	7°·1 C.	3191	1635	1283
Q	7°·3	3204	1606	1272
P	7°·7	2504	1568	1259
Q	7°·8	2447	1568	1257
Q	91°·7	3811	1585	1326
P	93°·8	3972	1571	1322
Q	94°·8	3788	1564	1320
P	98°·1	4846	1555	1318
Q	100°·5	6098	1573	1327
P	151°·3	4932	1601	1377
Q	153°·0	5063	1581	1369
P	153°·9	5120	1603	1378

It will be found that the values of $\sqrt{\frac{D}{Ct}}$(corrected) lie *very nearly* on a straight line; we may take the coincidence as exact (assumption 2nd), *i.e.*, that

$$\frac{D}{Ct} = (\alpha + \beta t)^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3).$$

Now, supposing (assumption 3rd) that the four sets of thermopile junctions have, all of them, the same heat capacity (which is true to within about 1 %), we have, following the notation used above—

$$\frac{D}{Ct} = a\phi(t) - af(t) - \phi(t).F(t) + F(t).f(t)^*$$
$$= [a - F(t)]\,[\phi(t) - f(t)]$$

$\therefore$ by the experimental result in equation (2),

$$\frac{D}{Ct} = [a - F(t)]\,[a - F(t)]\psi(t) \quad . \quad . \quad . \quad . \quad . \quad . \quad (4).$$

Also, since $F(t)$, the thermoelectric power of FeZn, is known to be of the form $p + qt$, the experimental result in equation (3) may be written

$$\frac{D}{Ct} = n\,[b \pm F(t)]^2,$$

where n and b are independent of t, $\therefore$ we have proved (by experiment) that

$$[a - F(t)]\,[a - F(t)]\psi(t) = n[b \pm F(t)]^2\,;$$

$\therefore$ $\psi(t)$ *must be independent of t.*

And thus equation (2) becomes

$$\frac{f(t)}{F(t)} = \frac{\phi(t)}{a} \quad = \psi(t) = \text{constant.}$$

Thus we have established by *experiment* (and the three assumptions noted above) that the Peltier effect is proportional to the product of the thermoelectric power and the absolute temperature.

§ 5. *Measurement of Change in Specific Heat.*

As the correction for increase of specific heat due to rise of temperature becomes very large at the higher temperatures, it was necessary to *measure* it for the metals used in the above experiments. A new method of doing this occurred to the writer, and, although he believes that much more accurate results might be got by this method, the results already obtained seem worth publication. The method consists in measuring, at different temperatures, the Joule effect in a strip or strips of the given metal. For this purpose two narrow strips (of iron, say) were doubled along the two sets of junctions of an FeArg thermopile (wrapped in thin asbestos paper),

* These four terms correspond to the four sets of thermopile junctions.

and the whole tied between asbestos boards and copper plates. The whole was packed, in the usual manner, in asbestos within a copper box. The current was passed through the strips alternately (and for such periods as gave almost steady conditions of temperature), and the Joule effect found from the deflections in exactly the same manner as the Peltier effect in the foregoing experiments. Now the rise of temperature thus measured is directly proportional to the resistance of the strips, and inversely proportional to the heat capacity of the strips, asbestos, and thermopile ends. Thus, by observing also the change in resistance, we can at once calculate the change in heat capacity. By this method the correction is measured under almost exactly the same conditions as those under which it is to be applied. The influence of the asbestos paper (which was of very small mass) may be neglected. The widely different values of the correction found for the various metals seem to show that the capacity of the thermopile ends (which were of much smaller mass than the strips) may also be neglected. Thus the results may be considered as tolerably accurate measurements of the changes of specific heat in the various strips used.

The resistance measurements were made with a Wheatstone's bridge of the ordinary form. A thin strip of the given metal, soldered or brazed to two thick copper wires, was packed in asbestos in copper boxes. The measurements were all made at *nearly steady** temperatures, as it was found that when this was not the case, inconsistent results were obtained.

As the thermopile resistance formed a considerable fraction of the resistance of the galvanometer circuit, it had to be measured at different temperatures, and a correction introduced for this. In measuring this resistance, the battery contacts had to be short, otherwise the Peltier effects began soon to show themselves. Table XIII. gives some of these measurements. In this, as in all the other resistance and Joule effect measurements, the whole percentage increase from the value at the lowest temperature was calculated, and from this the *mean* percentage increase per degree C. from the lowest temperature. The resistance of the galvanometer and its connections was 1·667 ohms.

In order to test how far the thermopile deflections were a proper

* *I.e.*, not varying much more than 2° C. in half-an-hour.

measure of the Joule effect, a comparison of the deflections (D) with the square of the current (C^2) was made for several strengths

TABLE XIII.

Temperature.	Resistance of Pile +Galvanometer.	Whole per cent. Increase.	Mean Increase per cent. per 1° C.
8°·8 C.	2·158	0	0
87°·3	2·186	1·30	·0169
92°·6	2·187	1·34	·0159
120°·1	2·200	1·95	·0175
131°·8	2·205	2·18	·0178
155°·7	2·215	2·64	·0180
222°·1	2·248	4·17	·0195
227°·1	2·249	4·22	·0193
247°·4	2·261	4·77	.0200
257°·2	2·267	5·05	·0203
265°·6	2·271	5·24	·0204
278°·1	2·278	5·56	·0206

of current. Table XIV. gives the results for iron strips. The periods were 3 minutes each. As the temperature varied slightly, the last column gives $\frac{D}{C^2}$ corrected for the small changes in resistance and specific heat. That the proportionality of D to C^2 holds pretty nearly will be seen.

TABLE XIV.—*Joule Effect compared with (Current)*^{2}.

Temperature.	D.	C.	$\frac{D}{C^2}\times 10^3$.	$\frac{D}{C^2}\times 10^3$ (Corrected to 15°·8 C.)
15°·8C.	1284	1439	·6203	·6203
16°·3C.	761	1113	·6146	·6134
15°·8C.	113	425	·6256	·6256
15°·3C.	282	668	·6319	·6331

Time curves were also drawn for the Joule effect heating, by the same method as in the case of the Peltier effect. In Table XV. is given a set of the readings for the Joule effect in a narrow copper strip. The periods here were 150 half-seconds (instead of 100). The curve is number 4 in the diagram. For convenience of comparison with the Peltier effect curves, the 150-periods have been reduced in scale, so as to coincide with the 100-periods of (1), (2),

and (3); the deflections, however, are represented on a scale one-third larger than they actually were.

TABLE XV.—*Joule Effect in Copper Strip.*

Time, ½ Seconds.	Deflection.	Time, ½ Seconds.	Deflection.	Time, ½ Seconds.	Deflection.	Time, ½ Seconds.	Deflection.
3·6	19	154	1119	310·5	219	462	1119
6·5	69	160·6	1069	313	269	465·2	1069
9	119	162·6	1019	315·3	319	467·5	1019
13·5	169	165	969	317·6	369	470	969
16	219	168·2	919	320·5	419	473·2	919
18	269	170·6	869	323·5	469	475·5	869
22·5	319	173	819	325·3	519	478	819
24·3	369	176·5	769	329	569	480·7	769
27	419	179·2	719	331·6	619	483·7	719
30·3	469	182	669	334·3	669	487·5	669
32·6	519	185·3	619	338	719	491	619
35·5	569	189	569	341·3	769	495	569
39·3	619	193	519	345·6	819	499	519
43	669	197·5	469	350	869	505	469
47	719	201·6	419	355·6	919	510·5	419
51·2	769	208	369	361	969	518	369
57·6	819	214·6	319	367·5	1019	526·2	319
62·5	869	221·6	269	375·7	1069	536·5	269
69·2	919	230·5	219	385·6	1119	551·5	219
76·6	969	245·3	169	401	1169	581	169
87	1019	265·6	119	426	1219	...	...
101·5	1069	300	74	450	1249	...	...
125·5	1119	305	119	457·3	1219	...	...
150	1199	308	169	460	1169	...	...

In the tables which follow are given the measurements of the resistance and Joule effect, and the changes in specific heat deduced from them. In the case of the iron and cadmium the Joule effects were measured at the same time, iron being at one end of the thermopile and cadmium at the other. Also in this case the battery current was measured by the Joule effect in iron strips round the ends of another thermopile. With regard to the results for German

TABLE XVI.—*Change of Resistance of Cadmium.*

Temperature.	Whole per cent. increase.	Mean increase per cent. per 1° C.
12°·8 C.	0	0
78°·2	12·53	·3863
81°·1	12·65	·3880
97°·8	13·22	·3788
104°·2	13·55	·3885
130°·0	14·07	·3472
137°·8	14·96	·3968
152°·2	15·56	·3988
154°·4	15·52	·4000
175°·5	16·48	·4052

silver, much confidence cannot be placed upon the resistance measurements (in Table XXIII.); the changes were so small that, with the ordinary form of Wheatstone's bridge used, sufficient accuracy could not be obtained.

TABLE XVII.—*Change of Joule Effect in Cadmium.*

Temperature 18° C. to	Whole per cent. increase.	Mean increase per cent. per 1° C.
110°·8	32·9	·354
122°·2	32·53	·315
143°·8	38·25	·304
173°·0	37·76	·218

TABLE XVIII.—*Change of Resistance of Iron.*

Temperature.	Whole per cent. Increase.	Mean Increase per cent. per 1° C.
14°·4 C.	0	0
97°·2	45·4	·548
122°·0	59·9	·556
150°·4	77·7	·5714
178°·9	95·1	·5781
197°·8	109·9	·5994
215°·5	123·7	·6151
277°·8	178·9	·6790

TABLE XIX.—*Joule Effect in Iron.*

Temperature 18° C. to	Whole per cent. Increase.	Mean Increase per cent. per 1° C.	Same corrected.*
110°·8	36·12	·389	·391
122°·2	38·4	·369	·386
143°·8	45·53	·362	·380
173°·0	89·13	·347	·365

TABLE XX.—*Change of Resistance of Nickel.*

Temperature.	Whole per cent. Increase.	Mean Increase per cent. per 1° C.	Temperature.	Whole per cent. Increase.	Mean Increase per cent. per 1° C.
13°·0 C.	0	0	239°·3	115·0	·5084
86°·1	29·39	·4021	236°·8	112·4	·5023
98°·3	34·03	·3990	271°·7	139·1	·5376
149°·7	57·15	·4180	285°·8	149·1	·5466
154°·3	62·84	·4448	289°·1	151·6	·5491
154°·6	63·16	·4454	290°·9	152·4	·5483
199°·1	88·39	·4750	297°·1	159·2	·5604
200°·4	89·78	·4792	307°·2	168·0	·5711
210°·4	94·23	·4823	308°·5	169·4	·5735

* For change in resistance of thermopile.

TABLE XXI.—*Joule Effect in Nickel.*

Temperature.	Pile Deflection D.	$\frac{D}{C^2}$.	Whole per cent Increase.	Mean Increase per cent. per 1° C.	Same Corrected.
14°·2 C.	996·3	924·7	...	...	...
151°·9	928·3	1396·5	50·7	·3681	·386
16°·4	1037·5	989·7	...	...	...
97°·0	987·25	1268·6	27·14	·3367	·343
169°·4	933·8	1519·2	53·5	·3497	·369
223°·6	882·9	1747	76·5	·3681	·388
280°·8	990·7	1918	93·8	·3646	·385

TABLE XXII.—*Joule Effect in German Silver.*

Temperature.	Pile Deflection D.	$\frac{D}{C^2}$.	Whole per cent. Increase.	Mean Increase per cent. per 1° C.	Same corrected.
14°·6*	1046	2357	0	...	...
13°·8	1303·4	2295	0	...	...
80°·0	1022·4	2291	−0·2	−·003	·0139
127°·4	1054	2289	−0·26	−·0023	·0155
142°·0*	1253·9	2322	−1·1	−·008	·0098
181°·6	977·5	2155	−6·09	−·0303	−·0118
262°·3	919·75	2079	−9·43	−·0380	−·0184

TABLE XXIII.—*Change of Resistance of German Silver.*

Temperature.	Whole per cent. Increase.	Mean Increase per cent. per 1° C.
17°·1 C.	0	...
95°·3	1·93	·025
122°·6	3·00	·0280
158°·6	3·78	·0267
193°·4	5·07	·0288
197°·6	5·64	·0311
328°·2	10·0	·0321
281°·0	9·71	·0368
275°·0	9·54	·0370

TABLE XXIV.—*Changes of Specific Heat.*

Cadmium. Mean Increase per cent. per 1° C. from 18° C. to		Iron. Mean Increase per cent. per 1° C. from 18° C. to		Nickel. Mean Increase per cent. per 1° C. from 18° C. to		German Silver. Mean Increase per cent. per 1° C. from 14° C. to	
110°·8	·02	110°	·161	97°	·056	80°	·009
122°·2	·072	122°	·170	152°	·05	127°	·0115
143°·8	·076	144°	·188	170°	·091	142°	·0182
173°·0	·168	173°	·215	224°	·106	181°	·0297
...	...	...	...	280°	·175	262°	·0505

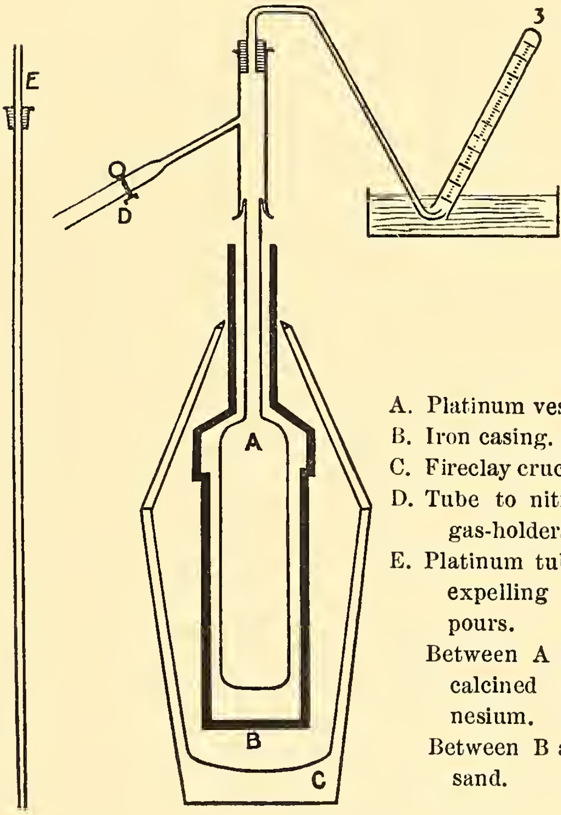
* These two belong to a separate set, and therefore the percentages for 142°·0 C. given are taken from 14°·6 C.

5. On some Vapour Densities at High Temperatures. By Alexander Scott, M.A., D.Sc.

The apparatus used in the following determinations is that of Victor Meyer, modified as described in a paper published in 1879, "On the Vapour Densities of Potassium and Sodium,"* by Professor Dewar and the author. In the experiments here described, the platinum vessel was further protected by a casing of iron, and the intervening space filled with magnesia, the iron casing being embedded in sand enclosed between two crucibles. In Series I. hydrogen was the gas used to fill the apparatus, but in all the others nitrogen prepared from the atmosphere by mixing it with ammonia and drawing the mixture over red-hot copper, then through dilute sulphuric acid into large glass gasholders, from which it was expelled after drying into the vapour-density apparatus. The nitrogen thus prepared almost invariably contains a small quantity of hydrogen, and this is a decided advantage for most of the substances used. The temperature of the furnace (which was an ordinary wind furnace, with 35-feet draught, and fed with coke) was considerably above the melting-point of cast iron, but was barely hot enough to volatilise rapidly potassium iodide and silver chloride, and this gives their results rather high. The best way of weighing the potassium and sodium was found to be to cut rapidly a piece of the required weight as nearly as possible, and instantly wrap it up in a tared piece of thin platinum foil and weigh again. The sodium kept remarkably well thus, but the potassium was not so satisfactory. To check the weights, pieces were similarly weighed and thrown into water, when it was found to require 23·8 to 24·3 milligrams of sodium to give 11·16 c.c. of hydrogen, according as an ordinary Becker's balance, turning with a milligram was used, or a finer one turning with ·1 milligram, the more rapid though coarser one giving thus the best results; of potassium similarly 46·3 milligrams were required.

The weight in milligrams of the substance which would be required to give 22·33 cubic centimetres of vapour is taken as the molecular weight.

* *Proc. Roy. Soc. Lond.*, vol. xxxii., 1879.



A. Platinum vessel.
B. Iron casing.
C. Fireclay crucibles.
D. Tube to nitrogen gas-holders.
E. Platinum tube for expelling vapours.
Between A and B calcined magnesium.
Between B and C sand.

This illustration belongs to paper by Dr Alexander Scott, p. 410.

Series I.—Apparatus filled with hydrogen, which tended to escape when the substance was introduced, and so give too high a number for the molecular weight ; coarser balance used.

Gram.								Molecular weight.
·030	sodium	gave	23·3	c.c.,	at 0° C.	and 760 mm.	pressure, giving	28·7
·035	,,	,,	23·6	,,	,,	,,	,,	33·1
·037	,,	,,	30·4	,,	,,	,,	,,	27·1
·033	,,	,,	27·9	,,	,,	,,	,,	26·4
							Mean for sodium,	28·8

Series II.—Same arrangement, but the apparatus filled with nitrogen.

Gram.								Molecular weight.
·029	sodium	gave	26·8	c.c.,	at 0° C.	and 760 mm.	pressure, giving	24·1
·032	,,	,,	29·7	,,	,,	,,	,,	24·0
·034	,,	,,	31·2	,,	,,	,,	,,	24·3
·030	,,	,,	27·6	,,	,,	,,	,,	24·3
·032	,,	,,	30·5	,,	,,	,,	,,	23·4
·029	,,	,,	25·6	,,	,,	,,	,,	25·3
·024	,,	,,	22·6	,,	,,	,,	,,	23·7
·026	potassium	,,	13·9	,,	,,	,,	,,	41·8
·045	,,	,,	25·1	,,	,,	,,	,,	40·0
·042	,,	,,	21·9	,,	,,	,,	,,	42·9
·052	,,	,,	28·4	,,	,,	,,	,,	40·9
							Mean for sodium,	24·2
							Mean for potassium,	41·4

Series III.—Same arrangement as Series II.

Gram.								Molecular weight.
·042	sodium	gave	36·9	c.c.	at 0° C.	and 760 mm.	pressure, giving	25·4
·026	,,	,,	21·0	,,	,,	,,	,,	27·7
·028	,,	,,	23·4	,,	,,	,,	,,	26·7
·036	potassium	,,	18·2	,,	,,	,,	,,	44·2
							Mean for sodium,	26·6

Series IV.—Same arrangement as above, but the finer balance was used in the weighings.

Gram.								Molecular weight.
·046	potassium	gave	23·2	c.c.	at 0° C.	and 760 mm.	pressure, giving	44·2
·056	,,	,,	27·4	,,	,,	,,	,,	45·7

Series IV.—*continued.*

Gram.							Molecular Weight.
·059	potassium	gave	25·0 c.c.	at 0° C.	and 760 mm. pressure,	giving	52·8
·052	,,	,,	22·8	,,	,,	,,	51·0
·071	,,	,,	35·2	,,	,,	,,	45·1
·060	,,	,,	30·6	,,	,,	,,	43 8
·023	sodium	,,	19·2	,,	,,	,,	26·7
·032	,,	,,	25·9	,,	,,	,,	27·6
·035	,,	,,	28·5	,,	,,	,,	27·4
·029	,,	,,	22·0	,,	,,	,,	29·4
·033	,,	,,	24·6	,,	,,	,,	30·0
·119	pot. iodide	,,	14·3	,,	,,	,,	185·6
					Mean for sodium,		28·2
					Mean for potassium,		47·1

Series V.—Same arrangement of apparatus; fine balance.

Gram.							Molecular weight.
·267	mercury	gave	29·3 c.c.	at 0° C.,	and 760 mm.	giving	203·3
·249	,,	,,	27·4	,,	,,	,,	202·8
·305	lead chloride	,,	25·6	,,	,,	,,	265·4
·293	,,	,,	25·2	,,	,,	,,	260·0
·319	cadmium bromide	,,	29·7	,,	,,	,,	240·0
·340	,,	,,	{31·1 35·3}	,,	,,	,,	{244·4 214·8
·340	cadmium iodide	,,	31·2	,,	,,	,,	243·3
·411	,,	,,	35·4	,,	,,	,,	259·0
·162	silver chloride	,,	22·4	,,	,,	,,	161·5
·191	potassium iodide	,,	22·4	,,	,,	,,	190·5
·203	,,	,,	24·3	,,	,,	,,	186·7
·277	mercuric sulphide	,,	37·2	,,	,,	,,	166·6

The mercury was used to test the apparatus, and indicates that the results obtained are a little too high.

The cadmium bromide seemed to cease giving off gas like the other substances, but if the measuring tube be left in position, an additional quantity of gas is obtained, most probably from further dissociation of the vapour into its elements. The cadmium iodide dissociated very largely as indicated by its low vapour density and by expelling the vapours, when large quantities of free iodine were observed. Silver chloride and potassium iodide volatilised very slowly; the vapour of the latter seemed to be free from every trace of free iodine.

SERIES VI.—Same arrangement of apparatus.

Gram.							Molecular weight.
·227 chromic chloride	gave	29·8	c.c.	at 0° C.	and 760 mm.,	giving	170·0
·100 ,,	,,	14·7	,,	,,	,,	,,	152·2
·120 ,,	,,	18·8	,,	,,	,,	,,	142·5
·149 manganous chloride	,,	25·0	,,	,,	,,	,,	133·0
·142 ,,	,,	24·1	,,	,,	,,	,,	131·6
·164 mercuric chloride	,,	25·8	,,	,,	,,	,,	142·0
·167 ,,	,,	22·0	,,	,,	,,	,,	169·3
·180 mercuric sulphide	,,	25·3	,,	,,	,,	,,	158·7
·145 ,,	,,	20·2	,,	,,	,,	,,	160·1
·250 mercurous chloride	,,	28·9	,,	,,	,,	,,	193·4
·223 ,,	,,	25·6	,,	,,	,,	,,	194·1
·068 sulphur	,,	23·2	,,	,,	,,	,,	65·3

The results of the chromium chloride are not very concordant, and may be due to traces of water. The sample used was sublimed and raised to a red heat before using; green oxide was observed on the tube for expelling the vapours. Mercuric chloride seems to dissociate largely into its elements, as do the sulphide and mercurous chloride. Sulphur was used to check the apparatus.

SERIES VII.—Same arrangement of apparatus.

Gram.							Molecular weight.
·288 cæsium iodide	gave	24·0	c.c.,	at 0° C.	and 760 mm.,	giving	268·0
·305 ,,	,,	25·7	,,	,,	,,	,,	265·0
·270 ,,	,,	22·5	,,	,,	,,	,,	267·9
·203 rubidium iodide	,,	19·3	,,	,,	,,	,,	234·6
·199 ,,	,,	20·1	,,	,,	,,	,,	221·5
·202 ,,	,,	20·3	,,	,,	,,	,,	221·8
·185 potassium iodide	,,	22·5	,,	,,	,,	,,	183·5
·176 ,,	,,	21·9	,,	,,	,,	,,	179·0
·143 ,,	,,	17·8	,,	,,	,,	,,	179·3
·196 cæsium chloride	,,	23·4	,,	,,	,,	,,	187·2
·157 ,,	,,	19·5	,,	,,	,,	,,	179·5
·175 ,,	,,	21·8	,,	,,	,,	,,	178·9
·114 rubidium chloride	,,	18·6	,,	,,	,,	,,	136·9
·121 ,,	,,	19·2	,,	,,	,,	,,	140·4
·130 ,,	,,	20·6	,,	,,	,,	,,	141·0
·147 silver chloride	,,	20·5	,,	,,	,,	,,	160·2

The substances in this series were fused before use, so as to be perfectly dry. They were also very pure, their equivalents being determined by titration with silver nitrate.

SERIES VIII.—Same arrangement of apparatus.

Gram.								Molecular weight.
·173	ferric chloride	gave	28·5	c.c., at	0° C.	and 760 mm.,	giving	135·7
·093	,,	,,	16·0	,,	,,	,,	,,	129·6
·120	,,	,,	19·3	,,	,,	,,	,,	138·6
·076	,,	,,	12·0	,,	,,	,,	,,	140·7
·145	iodine	,,	18·7	,,	,,	,,	,,	173·1
·171	,,	,,	20·6	,,	,,	,,	,,	185·5
·070	sulphur	,,	22·8	,,	,,	,,	,,	68·5
·055	,,	,,	18·0	,,	,,	,,	,,	68·0

The ferric chloride was sublimed immediately before use, but its extremely hygroscopic nature renders it highly probable that the vapour would contain hydrochloric acid.

The iodine indicates the dissociation of its diatomic molecules, as has already been shown by V. Meyer.

Sulphur again used as a check.

The values obtained for the molecular weights from the above experiments may be thus tabulated:—

	Values. Experimental.	Values. Theoretical.	Molecular formula indicated.
Sodium, . . .	25·5	23	Na
Potassium, . . .	37·7	39	K
Mercury, . . .	203	200	Hg
Sulphur, . . .	67·3	64	S_2
Iodine, . . .	179·3	169	(I_2+2I)
Cæsium iodide, . .	267	260	CsI
Cæsium chloride, . .	179·2	168·5	CsCl
Rubidium iodide, . .	221·6	212·3	RbI
Rubidium chloride, .	139·4	120·8	RbCl
Potassium iodide, . .	184·1	164	KI
Silver chloride, . .	160·8	143·5	AgCl
Lead chloride, . .	262·7	278	$PbCl_2$
Manganous chloride, .	132·3	126	$MnCl_2$
Ferric chloride, . .	136·1	162·5	$FeCl_3$
Chromic chloride, .	154·9	159	$CrCl_3$
Cadmium bromide, .	242·2	272	$CdBr_2$
Cadmium iodide, . .	251·1	366	$(CdI_2+Cd+I+I)$
Mercuric sulphide, .	161·8	155	$(2Hg+S_2)$
Mercurous chloride, .	193·7	$2Hg+Cl_2=157$	Mixtures of $Hg+Cl_2$ with some $HgCl_2$
Mercuric chloride, .	155·6	$Hg+Cl_2=135·5$	

The platinum vessel after the first experiment seemed to have no action whatever on the vapours of potassium and sodium, and the tables above given contain the results of *every experiment made*

with the exception of one with sodium, which was the first done in a new platinum vessel. It may be taken then as conclusively proved that the molecules of potassium and sodium are monatomic at high temperatures.

Mercury, sulphur, and iodine were used to test the apparatus and the degree of accuracy to be expected, and gave results quite in accordance with those of other experimenters, as did also mercuric sulphide. Both chlorides of mercury seem to undergo a very large amount of dissociation into their elements, though not to the same extent as the sulphide.

The results for the chlorides and iodides of cæsium, rubidium, and potassium point to the ordinarily received formulæ as the correct ones, as is the case with the chlorides of manganese, silver, and lead, which last seems to dissociate to a certain extent.

The bromide and iodide of cadmium also seem to dissociate largely at the temperature employed. Ferric and chromic chlorides give results which seem to point conclusively to the formulæ $FeCl_3$ and $CrCl_3$ as the true ones, the chromic chloride giving results very closely corresponding to this, and the ferric chloride considerably lower, which may be due to water absorbed during weighing (which was done as above described for potassium), and this giving a larger volume of hydrochloric acid than the ferric chloride from which it would be produced, gives too low a number for the molecular weight.

Potassium fluoride was tried, but gave no vapour whatever, and phosphorus at once destroyed the vessel.

One experiment with arsenic trioxide gave results pointing to As_4O_6 as its formula, but further experiments are in progress with it and several other bodies, the results of which I hope to be able to communicate shortly.

6. On the Determination of the Plane Curve which forms the Outer Limit of the Positions of a certain Point. By Dr G. Plarr. Communicated by Professor Tait.

7. The Thermal Windrose at the Ben Nevis Observatory. By A. Rankine.

The Table, showing the Thermal Windrose, accompanying this paper, was computed from the observations made at the Ben Nevis Observatory during the three years ending May 1887. It shows the mean temperatures, on the mean of the three years, of the different winds for each month, for the year, and for the seasons. The direction of the wind is observed to the thirty-two points of the compass, but in this table the temperatures are only shown for eight points, the intermediate points having all been added on to these eight points in the same way as that described by Mr Omond inh is paper on "Winds and Rainfall" in the *Journal of the Scottish Meteorological Society*, namely, the *by* winds were added to their adjacent octants, and the points half-way between the octants were on the odd day of each month added to the octant to their right, looking out from the centre of the compass card, *i.e.*, they were veered two points, and on the even days to that on their left, *i.e.*, they were backed two points. The mean temperature of each direction of wind for each month was found by tabulating the hourly observations of temperature under the direction of wind observed at the same time, or under its octant as above, and then taking the arithmetical mean. When the wind was variable, or its direction doubtful, the temperature was entered in the column for calms and variables. These variable winds belong chiefly to the northern half of the compass, their existence being principally due to the abrupt and precipitous character of the north side of the Ben. The temperatures given in the table are those indicated by thermometers which in summer and autumn are protected in the regulation Stevenson screen, and in winter and spring in a smaller pattern of the same, which can be shifted up or down a ladder-like stand, so as to be always at or near the standard height of 4 feet above the surface of the snow.

There are two columns of monthly means in the table, one giving the mean temperature for each month, deduced in the ordinary manner from the daily means, and the other giving the mean of the temperatures in the table under the eight wind directions. It will be seen that there is a considerable difference between these mean

temperatures in certain months, which difference is obviously due to the inequality of the frequency of the different winds.

As the character and relative frequency of each wind have already been discussed by Mr Omond, it only remains to add a few words regarding wind-temperature, as far as the accompanying table sheds any light on the subject.

The first point to be noticed is, that on the mean of the year the south is the warmest wind, its temperature being 32°·6 ; and the north-east is the coldest wind, its temperature being 26°·5 ; and also that the winds in their order of highest temperature are—S., S.W., W. (N.W. = S.E.), E., N., N.E.,—the north-west and south-east winds being equal in temperature, and the east and north winds almost so. In each of the seasons the north-east wind is the coldest, and, with one exception, the south is the warmest, the exception being winter, when the warmest wind is the south-west. An inspection of the results for the different months shows that the above order of highest temperature of the directions varies considerably from month to month, as does also the difference between the temperatures of the warmest and coldest winds in each month, the maximum difference being 10°·7 in March, and the minimum 4°·2 in April ; while the mean of all the monthly differences is 6°·7. This difference on the mean of the seasons is greater in winter and spring than in summer and autumn, being least in summer. Though the means for the year and for the seasons show that the warmest and coldest points are almost constant, yet the monthly results show that these points oscillate, the warmest point markedly and the coldest rather less so. During the winter months the warmest point is south-west, but, as the year advances, it swings round through south to south-east, which is its direction in July and September. The coldest point is north-east for nine months ; and though in February it is south-east, in June east, and in November north, it has not so well defined an oscillation as the warmest point.

The differences between the annual ranges of temperature of the different directions of wind seem to point to the cause of this oscillation in the direction of the warmest wind, as being the degree to which the yearly march of temperature affects the areas over which these winds blow. The south-east wind has an annual range,

in mean monthly temperature, of 24°·3, its temperature in February, for which month it is the coldest wind, being 19°·7; and in July, when it is not only the warmest for that month, but also for the whole year, its temperature is 44°·0. The wind having the least annual range is the north-west, its temperature in January being 24°·1, and in August 35°·8, thus giving it a range of 14°·4. The mean annual range for the directions S.W., W., N.W., is 15°·6., and that for S.E., E., N.E., is 20°·7. The difference between these ranges is apparently due to the fact that the easterly winds blow over land, and the westerly winds over sea,—land areas being subject to greater extremes of temperature than sea areas, and this is apparently the cause of the oscillation above referred to. In the tables given with Mr Omond's paper on "Winds and Rainfall" in this number of the Journal,* it is seen that the greater number of south-east winds belong to anticyclonic systems, and this also may have a good deal to do with the great annual range of temperature of the south-east wind. A comparison of the temperatures of the same direction of wind in cyclonic and anticyclonic systems has not as yet been made; but I could see, when working up this table, that such a comparison would probably result in interesting and useful knowledge. It may be noted that each of the directions, S., S.W.,

Thermal Windrose, Ben Nevis Observatory. (Computed from the Observations of the three Years, June 1884 *to May* 1887.)

	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm or Variable.	Mean of Month.	Mean of 8 Winds
January	20°·6	18°·7	18°·8	20°·5	23°·3	25°·3	24°·2	24°·1	22°·1	22°·9	21°·9
February	21·5	21·5	21·9	19·7	24·3	27·8	27·4	24·8	21·1	24·0	23·6
March	19·9	18·2	19·5	19·7	28·9	28·3	27·9	26·5	23·1	22·8	23·2
April	24·3	24·1	24·4	27·8	27·9	28·3	27·1	26·0	26·2	26·5	26·5
May	27·9	27·4	29·5	31·4	31·5	31·8	30·0	29·0	29·9	29·9	29·8
June	35·5	33·6	33·5	37·5	38·1	37·3	36·5	35·8	36·7	36·2	36·0
July	37·3	33·9	38·9	44·0	42·8	41·1	40·1	38·0	38·5	40·6	39·5
August	36·2	35·8	36 2	40·7	42·5	41·8	39·5	38·5	37·9	40·7	38·9
September	31·9	31·1	34·4	40·5	39·5	37·8	36·7	34·7	34·1	36·9	35·8
October	29·9	28·6	29·0	31·4	33·8	32·7	31·8	31·1	30·6	31·2	31·0
November	25·2	25·4	26·3	26·6	30·2	31·3	30·2	29·5	25·8	28·2	28·1
December	21·4	19·5	21·1	22·9	27·9	27·0	25·8	25·8	23·5	23·4	23·9
Year	27·6	26·5	27·8	30·2	32·6	32·5	31·4	30·2	29·1	30·3	29·8
Spring	23·7	23·2	24·5	26·3	30·0	29·5	28·3	27·2	26·4	26·4	26·5
Summer	36·3	34·4	36·2	40·7	41·1	40·1	38·7	37·4	37·7	39·2	38·1
Autumn	29·0	28·4	29·9	32·8	34·5	33·9	32·9	31·8	30·2	32·1	31·6
Winter	21·2	19·9	20·6	21·0	25·2	26·7	25·5	24·9	22·2	23·4	23·1

* See *Journal of Scottish Meteorological Society*, vol. vii. p. 275, and vol. viii. p. 18.

W., and N.W., attains its minimum temperature in January, and each of the directions, N., N.E., E., and S.E., in March; while all the directions except N.E. and N.W. have their maxima in July, the two exceptions occurring a month later.

8. On Ferric Ferricyanide as a Reagent for Detecting Traces of Reducing Gases. By Professor Crum Brown.

The brown solution obtained when solutions of ferric chloride and potassium ferricyanide are mixed, which may be regarded as containing ferric ferricyanide, is, as is well known, very readily turned blue by reducing agents, Prussian blue or Turnbull's blue being formed. The author uses strips of filter paper dipped in the freshly prepared solution to test for traces of reducing gases, such as sulphuretted hydrogen, sulphurous acid, &c. As nitrous fumes also blue the brown solution, reducing it, traces of them can be detected by using together a piece of paper prepared as above and a piece of iodised starch paper.

9. On the Compressibility of Water, of Mercury, and of Glass. By Professor Tait.

10. An Account of some Experiments which show that Fibrin-Ferment is absent from circulating Blood-Plasma, and which support the view, first advanced by Sir Joseph Lister, that the Blood has no spontaneous tendency to Coagulate. By Professor John Berry Haycraft.

(Abstract.)

Sir Astley Cooper and Turner Thackrah taught that blood was a fluid tending to coagulate, but inhibited from doing so by the living vascular walls. This view is erroneously ascribed to Brücke, who himself only professes to support it.

Sir Joseph Lister considers that blood of itself does not tend to

coagulate. When shed from the body this condition is *actively* induced by its contact with foreign matter. It is unnecessary, therefore, to assume that the vascular wall has any inhibitory power.

In order conclusively to determine which view is the correct one, it is necessary to obtain blood which is neither in contact with the vascular wall nor with a solid foreign body This I have succeeded in doing by immersing drops of blood in fluids differing from it in surface tension, such as oil, paraffin, &c.

Many experiments were made, notably the receiving of blood upon the greased surface of a mica plate immersed in a vessel full of paraffin oil. The drops remained fluid sometimes for two or three hours. The most successful experiments were, however, performed by injecting a viscous mixture of vaseline and paraffin oil into the vein of a sheep. It was mixed with the blood so as to isolate drops of blood in the midst of the viscous mass. These remained fluid on more than one occasion for twelve hours afterwards.

The conclusion I drew from these experiments was that blood required the influence of solid matter to bring about coagulation, and that the view of Sir Joseph Lister—which he had himself supported by the strongest arguments—was correct.

Shortly after the completion of these experiments, the important results of Dr Freund's were published. He had been traversing almost exactly the same ground that I had, and at the same time.

There is a general belief that white blood corpuscles are constantly breaking down in the blood-vessels, setting free fibrin-ferment. This view we owe mainly to Alexander Schmidt and his school. Moreover, ferment when artificially injected into the blood-vessels soon disappears.

I had been led by my experiments to the fact that the smallest quantity of fibrous ferment will in time coagulate a considerable mass of blood, and inasmuch as blood remains fluid in a ligatured vein for twelve or twenty-four hours, it is improbable that blood corpuscles are constantly breaking down and setting free ferment, unless we suppose that its action is prevented by the vascular wall. This latter supposition both Freund's and my own experiments have shown to be highly improbable.

In order by a direct experiment to determine whether the vascular

walls destroy the ferment in any way, I performed the following experiment:—

Dilute blood ferment was injected and reinjected several times into the blood-vessels of a dog, previously freed from blood. The injected solution was as powerful in causing the coagulation of hydrocele fluid as a similar solution which had not been passed through the vessels.

It is probable, therefore, that any injected ferment is destroyed within special organs or eliminated from the system. This would of course be *à priori* most probable. One is driven to conclude, therefore, that in the course of the ordinary circulation (whatever may take place in glandular structures) corpuscles do not break down, nor is free ferment present in the plasma.

It is stated, however, by many to be present.

Believing that this assumption is due to want of care in manipulation, I repeated well-known experiments, adopting special precautions.

Previous experimenters had obtained blood either from a cut vessel or from one fitted with a glass cannula. The blood must, therefore, have come in contact for a moment—a sufficient time to produce ferment—with the cut surface of the vessel or with the cannula. The blood was then received into a vessel containing a saturated solution of sulphate of magnesia. After filtration, the plasma was found to possess the power of clotting, on dilution.

I repeated this experiment, mixing the magnesium sulphate solution with the blood *while the latter was still within a blood-vessel* (an excised vein). After filtration, the diluted and dialysed plasma did not coagulate.

I am inclined then to believe that blood corpuscles do not break down in the blood-vessels, or that if they do they do not set free any ferment. This latter is not present in circulating blood, which only tends to coagulate when it is brought in contact with solid matter.

The exact way in which this solid matter acts, I hope to discuss in a subsequent paper.

11. On the Chemical Composition of the Water composing the Clyde Sea Area. By Adam Dickie.

About the beginning of this year I was requested by a sub-committee of the Government Grant Committee* to determine some of the components of a series of samples of sea water, which were to be collected during the year at various parts and at different times in the Clyde sea area by the observers of the Scottish Marine Station. The collections were chiefly made under the immediate direction of Dr H. R. Mill. Since January, accordingly, I have been working at this, and have completed in all eighty-nine analyses, the results of which I now take the liberty of placing before this Society. There are various reasons why this paper should consist of little more than tables of results, one of which is that, having little or no experience in the science of oceanography, it would be presumptuous in me to draw conclusions from my results which would no doubt strike any one acquainted with that science at once. Another reason is that, though acquainted with some of the physical conditions under which the samples were taken, such as depth, temperature, place of collection, and date, I am quite ignorant of other conditions quite as important, if not more so, in my estimation, as, for instance, presence or absence of some freshwater stream near place of collection, state of tide, rainfall, &c.,—all conditions which would no doubt influence more or less materially the salinity of the water.

It is needless for me even to describe the methods of analysis I adopted, as, with one exception, I have adhered strictly to the methods so fully described by Dr Dittmar in his memoir on the "Challenger" waters. The exception was in the case of the chlorine, in the analysis of which, though using the modification of Volhard's method described in the memoir for my final titration, I employed Mohr's method, in which chromate of potash is used as an indicator for the preliminary.

It was intended at first to determine the chlorine, the sulphuric acid, the alkalinity, and the suspended matter, but the latter I only completed in some of the first batch of samples. In estimating this

* The sub-committee consisted of Professor Dittmar, Professor Crum Brown, and Mr John Murray.

I proceeded as follows:—After determining the other components, I weighed the bottle and all that remained of the sea water, filtered the water through a tared filter paper (which was then dried at 100° C. and weighed), and then weighed bottle again, the difference of course being weight of water filtered. I found that, whilst there was generally about a kilogramme of water filtered, the weight of suspended matter never amounted to more than 8 mgrms., and was sometimes not more than $1\frac{1}{2}$ mgrms., and, as the probable error in weighing would amount to a not inconsiderable portion of this small weight, I considered that the importance of the result obtained was not worth the time and labour employed in the getting of it, which was sometimes considerable.

Appended is a table of results.

The first six columns of this table explain themselves. Column A gives the chlorine in grms. per kilo.; column B gives the sulphuric acid (SO_3) in grms. per kilo.; C and D are the alkalinity columns; C gives the amount of CO_2 present as normal carbonate in mgrms. per litre; and D the amount in mgrms. per 100 mgrms. of total salts. In this latter, as I did not estimate the total salts, I have calculated from the chlorine, using the number 55·43 as equivalent to 100 parts total salts, that being the number which Dr Dittmar establishes in his "Challenger" memoir. Column E gives the ratios existing in the different samples between the chlorine and the sulphuric acid; *i.e.*, the weight of SO_3 per unit of chlorine.

On glancing over the chlorines ascertained in the "Challenger" work, we find that in no sample was the chlorine less than 18 grms. per kilo., that the largest number of samples gives quantities between 19 and 20 grms. per kilo., and that the sample having greatest amount contained 20·64 grms. per kilo. In above table we find that the largest amount of chlorine is contained in No. 2619 = 18·946 grms. per kilo., a sample taken in the channel south of Sanda; and the least in No. 1423 = 1·1692 grms. per kilo., a Lochfyne sample. But though the difference between these two figures is considerable, the general variation in the quantity of chlorine is not so great, for we find that out of the eighty-nine samples only eight (all of them surface samples) contain less than 16 grms. per kilo., and only five less than 14 grms. per kilo.

The difference in salinity between surface and bottom water is

Table of Results.

Numbers of Samples.	Place of Collection.	Depth.	Temperature.	Date.	Hour. hrs.	Hour. min.	A.	B.	C.	D.	E.
1227	Off Gantock Beacon, ...	Surface.	48° 0	12/11/86	16	20	16·88	1·9878	46·44	·1491	·11776
1229	do., ...	Bottom, 53 fms.	51 7	12/11/86	16	15	18·428	2·1733	49·42	·1451	·11793
1224	Holy Loch, off Kilmun, . . .	Bottom, 14 fms.	51 4	12/11/86	14	45	18·251	2·1405	47·88	·1420	·11728
1223	do., . . .	Surface.	...	12/11/86	14	45	16·505	1·9228	45·08	·1482	·11648
1234	Clocholar Point, Loch Striven, . .	Bottom, 36 fms.	51 5	13/11/86	11	35	18·561	2·1902	51·12	·1490	·118
1232	do., . .	Surface.	47 4	13/11/86	11	35	17·036	1·9993	45 84	·1459	·11735
1192	Between Helensburgh and Greenock, .	Bottom, 13 fms.	51 0	11/11/86	10	40	18·28	2·1388	48·92	·1448	·117
1191	do., .	Surface.	47 3	11/11/86	10	40	14·982	1·7654	42·4	·1538	·11783
1208	Stuckbeg, Loch Goil, ...	Surface.	49 1	12/11/86	8	40	16·009	1·8758	43·2	·1458	·11717
1210	do., ...	Bottom, 44 fms.	45 6	12/11/86	8	40	18·294	2·1428	49·08	·1452	·11713
1211	Thornbank, Loch Long, ...	Surface.	49 0	12/11/86	10	15	17·8	2·0869	48·68	·1481	·11724
1213	do., ...	Bottom, 30 fms.	51 7	12/11/86	10	15	18·316	2·1468	49·0	·1447	·11720
1198	Gareloch, Shandon, ...	Surface.	49 0	11/11/86	12	30	16·608	1·9479	46·44	·1516	·11728
1199	do., ...	Bottom, 23 fms.	50 9	11/11/86	12	30	17·473	2·033	48·2	·1494	·11635
1277	Kilbrannan Sound, between Imochar Point and Carradale, . . .	Surface.	49 5	18/11/86	11	35	18·142	2·1314	49·72	·1483	·11748
1278	do., . . .	Bottom, 78 fms.	51 5	18/11/86	11	35	18·618	2·1908	50·24	·1460	·11767
1280	Plateau at end of Kilbrannan Sound, .	Bottom, 22 fms.	...	18/11/86	13	45	18·421	2·1714	49·6	·1467	·11787
1266	Off Dunderawe Castle, Loch Fyne, .	From 6 fms.	49 1	17/11/86	10	30	17·631	2·0761	47·12	·1448	·11775
1267	do., .	Bottom, 34 fms.	48 2	17/11/86	10	30	18·188	2·1263	48·2	·1434	·11690
1251	Off Skate Island, Loch Fyne, . .	From 46 fms.	51 6	16/11/86	10	15	18·557	2·1583	49·48	·1442	·11630
1250	do., . .	Surface.	49 3	16/11/86	10	25	18·103	2·1313	47·24	·1412	·11773
1258	Furnace, Loch Fyne, ...	Bottom, 36 fms.	48 6	16/11/86	15	10	18·258	2·1357	48·72	·1444	·11697
1257	do., ...	From 6 fms.	49 3	16/11/86	15	20	17·97	2·1121	48·48	·146	·11753
1243	Ormidale, Loch Ridden, ...	Surface.	49 1	15/11/86	...		14·879	1·7478	39·72	·1451	·11746
1244	do., ...	Bottom, 12 fms.	51 1	15/11/86	12	30	18·195	2·1296	49·36	·1502	·11704
1256	Furnace, Loch Fyne, ...	Surface.	49 1	16/11/86	15	25	17·8	2·087	47·6	·1448	·11724
1238	Between Cumbrae Light and Garroch Head, ...	Surface.	50 5	15/11/86	9	50	18·08	2·124	54·48	·1630	·11747
1240	do., ...	Bottom, 60 fms.	51 3	15/11/86	9	40	18·548	2·1812	50·52	·1473	·11759

1265	Off Dunderawe Castle, Loch Fyne,	Surface.	43	9	17/11/86	10	30	5·575	·6054	16·76	·1656	(·10859)
1252	Off Skate Island, Loch Fyne,	Bottom, 104 fms.	51	7	16/11/86	9	55	18·640	2·1833	48·96	·1421	·11713
1366	Skate Island, Loch Fyne,	From 85 fathoms.	44	3	7/ 2/87	9	40	18·452	2·1553	49·68	·1456	·11680
1365	do.,	Surface.	43	7	7/ 2/87	9	40	17·979	2·1153	49·8	·1499	·11765
1373	Gantock Beacon, Dunoon,	Surface.	39	0	8/ 2/87	9	35	10·192	1·1923	30·84	·1673	·11698
1374	do.,	Bottom, 47 fms.	44	0	8/ 2/87	9	35	18·247	2·1538	48·2	·1462	·11792
1362	Cuill, Loch Fyne,	Bottom, 18 fms.	45	0	5/ 2/87	9	15	17·563	2·0672	43·56	·1346	·1177
1361	do.,	Surface.	40	6	5/ 2/87	9	15	2·667	·2812	7·64	·1588	(·10543)
1363	Gortans, Loch Fyne,	Surface.	43	9	5/ 2/87	14	0	16·674	1·9645	44·84	·1458	·11781
1364	do.,	Bottom, 26 fms.	44	5	5/ 2/87	14	0	18·041	2·1344	48·84	·1465	·11830
1387	Plateau 2 miles south of Rhuad Point,	Surface.	43	5	11/ 2/87	10	25	18·386	2·1587	49·32	·1450	·11740
1388	do.,	Bottom, 23 fms.	43	6	11/ 2/87	10	25	18·487	2·1807	50·28	·1471	·11795
1379	Loch Goil,	Bottom, 32 fms.	47	1	8/ 2/87	15	5	18·209	2·1321	49·12	·1494	·11709
1378	do.,	Surface.	39	0	8/ 2/87	15	5	10·907	1·2875	31·84	·1607	·11804
1383	Imochar, Kilbrannan Sound,	Surface.	43	3	10/ 2/87	10	20	18·274	2·1428	49·44	·1464	·11725
1384	do.,	Bottom.	43	8	10/ 2/87	10	20	18·373	2·1642	48·84	·1438	·11779
1456	Channel, 5 miles south of Sanda,	Bottom, 50 fms.	44	2	30/ 3/87	17	15	18·882	2·2085	50·52	·1525	·11696
1455	do.,	Surface.	44	7	30/ 3/87	17	15	18·661	2·202	49·8	·1443	·118
1453	Carradale, Kilbrannan Sound,	Surface.	44	3	30/ 3/87	12	50	18·309	2·1512	48·84	·1442	·11749
1454	do.,	From 71 fms. bottom, 79 fms.	44	0	30/ 3/87	12	50	18·583	2·1865	49·68	·1446	·11766
1434	Strachur, Loch Fyne,	Bottom, 75 fms.	45	5	29/ 3/87	14	0	18·226	2·1396	48·96	·1454	·11717
1432	do.,	Surface.	44	9	29/ 3/87	14	0	11·335	1·3325	31·56	·1532	·11755
1423	Cuill, Loch Fyne,	Surface.	42	1	29/ 3/87	11	0	1·1692	·1153	3·76	·1682	(·09855)
1424	do.,	From 3 fms. bottom, 13 fms.	44	4	29/ 3/87	11	0	16·906	1·9923	45·36	·1489	·11784
1425	do.,	Bottom, 13 fms.	44	7	29/ 3/87	11	0	17·847	2·0967	46·84	·1421	·11748
1442	Gortans, Loch Fyne,	From 3 fms. bottom, 35 fms.	44	3	29/ 3/87	17	10	17·921	2·0973	46·2	·1396	·11703
1443	do.,	Bottom, 35 fms.	44	0	29/ 3/87	17	10	18·468	2·169	49·6	·1452	·11744
1441	do.,	Surface.	45	0	29/ 3/87	17	10	17·561	2·0566	47·08	·1452	·11711
1433	Strachur, Loch Fyne,	From 3 fms. bottom, 75 fms.	44	3	29/ 3/87	14	0	17·436	2·0492	47·16	·1465	·11752
1422	Skate Island, Loch Fyne,	From 100 fms. bottom, 106 fms.	43	0	28/ 3/87	15	0	18·687	2·193	49·72	·1439	·11735

Table of Results—continued.

Numbers of Samples.	Place of Collection.	Depth.	Temperature.	Date.	Hour. hrs. min.	A.	B.	C.	D.	E.
2620	Off Carradale, ...	Surface.	57° 6	18/ 6/87	12 0	18·418	2·1639	48·16	·1415	·11748
2621	do., ...	Bottom, 70 fms.	45 0	18/ 6/87	12 0	18·728	2·2154	50·96	·1472	·11829
2619	Channel, south of Sanda, . . .	Bottom, 58 fms.	48 7	17/ 6/87	17 45	18·946	2·2281	49·84	·1422	·1176
2618	do., . . .	Surface.	52 1	17/ 6/87	17 45	18·844	2·223	50·24	·1441	·11796
2625	Gantock Beacon, Dunoon, . . .	Bottom, 40 fms.	47 0	18/ 6/87	17 20	18·492	2·182	49·88	·1459	·11799
2607	Skate Island, Loch Fyne, . . .	Bottom, 103 fms.	46 5	16/ 6/87	15 30	18·854	2·2234	50·76	·1455	·11792
2606	do., . . .	Surface.	53 2	16/ 6/87	15 30	18·481	2·1797	55·12	·1613	·11794
2595	Off Strachur, Loch Fyne, . . .	Bottom, 70 fms.	45 2	15/ 6/87	19 30	18·466	2·1712	50·36	·1475	·11757
2594	do., . . .	Surface.	52 4	15/ 6/87	19 30	17·979	2·1142	49·24	·1483	·11759
2571	Off Chlapochlar, Loch Striven, . .	Surface.	51 2	14/ 6/87	19 0	17·878	2·1003	47·44	·1437	·11748
2572	do., . . .	Bottom, 34 fms.	46 2	14/ 6/87	19 0	18·776	2·21	55·6	·1601	·1177
2554	Off Stuckbeg, Loch Goil, . . .	Surface.	52 5	14/ 6/87	8 55	16·583	1·9559	44·64	·1461	·11794
2556	do., . .	Bottom, 43 fms. from 36 fms.	44 9	14/ 6/87	8 55	18·393	2·1643	50·72	·1492	·11766
2542	Off Shandon, Gareloch, . . .	Bottom, 21 fms.	48 2	13/ 6/87	10 50	17·923	2·0954	47·6	·1405	·11691
2541	do., . . .	Surface.	52 4	13/ 6/87	10 50	17·832	2·1008	49·4	·15	·11781
2626	Off Thornbank, Loch Long, . . .	Surface.	59 9	13/ 6/87	20 15	17·137	2·0122	46·08	·1457	·11741
2521	Off Cuill, Loch Fyne, ...	Bottom, 13 fms.	45 7	10/ 5/87	19 30	18·155	2·1269	49·32	·1470	·11715
1	Off Imochar, Kilbrannan Sound, . .	Bottom, 60 fms.	44 2	9/ 5/87	12 40	18·634	2·194	55·48	·1661	·11774
2	do., . .	Surface.	46 9	9/ 5/87	12 40	18·375	2·1538	50·56	·1489	·11721
2495	Off Shandon, Gareloch, . . .	Bottom, 21 fms.	45 3	6/ 5/87	17 15	17·433	2·0512	50·28	·1561	·11766
2494	do., . . .	Surface.	49 3	6/ 5/87	17 15	16·403	1·9319	45·36	·1501	·11777
2503	Off Thornbank, Loch Long, . . .	Surface.	50 5	7/ 5/87	10 30	16·622	1·9576	44·8	·1462	·11777
2504	do., . . .	Bottom, 31 fms.	43 7	7/ 5/87	10 30	18·365	2·163	50·12	·1477	·11777
2521	Off Strachur, Loch Fyne, . . .	Bottom, 72 fms.	44 7	10/ 5/87	15 30	18·334	2·1539	52·36	·1545	·11748
2520	do., . . .	Surface.	48 3	10/ 5/87	15 30	17·693	2·0689	47·52	·1455	·11693
2488	Gantock Beacon, Dunoon, . . .	Surface.	46 3	6/ 5/87	13 45	17·224	2·0374	51·88	·1559	·11828
2489	do., . . .	Bottom, 35 fms.	44 4	6/ 5/87	13 45	18·392	2·1713	49·4	·1522	·11805
2500	Off Stuckbeg, Loch Goil, . . .	Bottom, 44 fms.	44 2	7/ 5/87	8 15	18·225	2·1429	49·12	·1459	·11758
2499	do., . . .	Surface.	49 7	7/ 5/87	8 15	16·121	1·9044	43·96	·1480	·11813
2516	Off Skate Island, Loch Fyne, . .	Surface.	46 5	10/ 5/87	9 30	18·269	2·1522	51·16	·1515	·1178
2517	do., . . .	Bottom, 105 fms.	44 3	10/ 5/87	9 30	18·715	2·1988	49·32	·1425	·11749

very marked. In every instance the amount of chlorine is greater in bottom than in surface samples taken at the same spot. It is also curious to note that, where three samples have been taken (a surface, a bottom, and one from some intermediate depth), the chlorine in the latter is always greater than in the surface, and less than in the bottom samples, leaving one to infer that the chlorine in estuary water, at least, increases steadily from surface to bottom. I may here state that in five cases were surface, intermediate, and bottom samples collected, and in every one of these the above rule holds good.

The alkalinity was determined in 250 c.c. of the water, by means of titration with $\frac{1}{22}$ normal acid and alkali. The mean over all the samples for alkalinity, in terms of 100 parts total salts, or, in other words, the weight of CO_2 present as normal carbonates, expressed in terms of 100 parts total salts, or of 55·43 parts chlorines, is as ·1482 is to 100. The mean alkalinity of the forty-one surface samples is ·15, and of the thirty-nine bottom samples ·1470. In the "Challenger" water the mean over all was ·1520, that of fifteen surface waters ·1492, and that of sixty-three bottom waters ·1540.

The sulphuric acid was determined in about 50 grms., strictly in accordance with the method described by Dr Dittmar in his "Challenger" memoir. These results are shown in columns B and E of the table, B giving the sulphuric acid (SO_3) per kilo., which, of course, varied with the quantity of chlorine; and E the quantity expressed in mgrms. per mgrm. of chlorine. In the latter column the results, except in three cases, are fairly constant. In the three exceptional cases, which are enclosed in brackets, it is noticeable that the amount of chlorine is remarkably small, and, of course, in the analysis the amount of $BaSO_4$ to be weighed would be correspondingly small, so I suppose the lowness of the results arises from the probable error in weighing, which, of course, in a calculation of this kind is multiplied up enormously. The average quantity of sulphuric acid (SO_3), in terms of the grammes of chlorine, is ·1175; whilst in the "Challenger" water it was ·11576, a somewhat lower figure.

12. An Experimental Critique of the Chloroplatinate Methods for the Determination of Potassium, including a redetermination of the Constant Pt. By Professor Dittmar and Mr John M'Arthur.

13. Addition to Thermometer Screens. Part IV. By J. Aitken, Esq.

(*Added August* 1887.)

I much regret it has not been possible for me, during this summer, experimentally to determine the best forms and sizes of the details in the construction of the C screen; nor have I been able to keep a continuous record of its readings. Only a few observations have been made at intervals with it, as originally constructed, and shown in fig. 1 of this paper, and with the Stevenson screens. The result of all these trials is to confirm the conclusions already arrived at. The C screen always gave the lowest readings when there was any radiation. The Stevenson screen, when worked under the ordinary conditions, that is with bottom open, generally gave on fine days readings of about two degrees too high; while the readings given with the Stevenson screen with bottom closed were higher than those of the C screen, but con-

Date.	Temp. by C Screen.	Error of Stevenson Screen.	Radiation by Large Ball.	Radiation by Vacuum Thermometer.	Temp. of Ball. / Temp. in Vacuum.	Ratio × Temp. of Black Ball.	Force of Wind.
July 23	63°·9	0°·6	20°	45°	·44	9	3
24	65°	1°·0	28°	69°	·40	11	4
25	63°	1°·0	26°	60°	·43	11	4
26	69°·1	0°·9	25°	53°	·47	12	3
27	67°·4	0°·6	22°	41°	·53	12	5
28	67°·1	0°·4	23°	49°	·47	11	5
29	64°·4	0°·6	21°	48°·5	·43	9	4
30	63°·1	1°·6	29°	62°	·47	14	2
31	61°·5	2°·3	38°	57°·5	·66	25	1
Aug. 1	65°·9	1°·6	28°	57°	·49	14	2
16	66°·9	2°·1	28°	44°	·64	18	0·5
17	65°·75	1°·75	29°	51°	·57	16	1
18	61°	2°·6	42°·5	67°·5	·63	27	0·5
19	62°·5	2°·1	38°	66°	·57	22	1

siderably lower than those got with the bottom open. On dull and windy days the differences in the readings were not so great.

To illustrate the influence of the weather on the readings of the Stevenson screen with open bottom, I shall use all the readings taken with the screens between the 23rd July and the 19th August of this year; these are arranged in the table (p. 428).

In the first column of the table are the dates of the observations; in the second column the temperatures of the air as given by the C screen; in the third column is the excess error of the Stevenson screen with bottom open; and in the last column is the estimated force of the wind, on a scale of from 0· to 10. It will be seen from the last column, that from the 23rd to the 29th July the weather was stormy, and that on the following days there was but little wind. The effect of these two conditions on the comparative readings of the screens is very evident. While the wind was strong it will be noticed that the difference between them was often less than one degree, but when the wind fell it was frequently more than two degrees.

One object of meteorological observatories is to tell us something about the climate of the place—that is, something about its effects on animal and vegetable life. Now it is admitted by every one that the indications of the instruments in general use in our observatories do not by any means agree with the indications given by our bodies. The thermometer often says one thing, while our feelings indicate something quite different. No doubt, part of this disagreement is the result of the more or less healthy condition of our bodies at the time; they are, so to speak, instruments with shifting scale. But apart from this, there is frequently a wide difference between the indications of the meteorological instruments and the *average* feeling of a great number of people. This results from the meteorological instruments not being affected by the same causes as our bodies. The thermometer may indicate that the air is warm, while we may feel it to be cold. This may be the result of the air being dry, and causing a cooling effect by a rapid evaporation from our bodies. We have therefore to check the readings of the dry bulb thermometer by a reference to the wet one; we thus get a greater similarity between the indications of the meteorological instruments and those of our bodies.

On our solar radiation measurements, we however have no such check. The thermometer with blackened bulb *in vacuo* may indicate a very strong solar radiation, and yet we may feel it chilly; or it may indicate a comparatively low radiation effect, and we may feel warm. This difference in the indications may be due to different causes, but no doubt wind is one of the most important. The wind has but little effect on the solar radiation thermometer, while it has a most powerful influence in checking the heating effect of the sun on our bodies. It seems therefore desirable that some other instrument be designed for the purpose of showing the radiation effect, as tempered by the wind, in order that our observatories may tell us something more definite about the climate of the place.

The radiation thermometer described in a previous part of this communication is affected by the wind, and might be used for this purpose; but owing to the absorbing surface being flat, it is necessary that it be kept always turned towards the sun; it is not, therefore, suitable for ordinary work. In place of it, I have for some time used a large hollow sphere made of thin metal, and having a thermometer fitted to it, with its bulb in the centre. What size this ball ought to be has not yet been determined, but if observations are to be taken with an instrument of this kind, all that seems necessary is, that a uniform size of ball be adopted for all observatories, and that it be made of the same kind and thickness of metal. The size of the ball, if not too small, does not seem to affect the readings much. Readings have been taken here with two balls—one 15 cm. in diameter, and the other 40 cm. Readings given by the 40 cm. ball will be given later on; those given by the smaller ball were only 2° or 3° lower than the large one.

These hollow balls have also been used for night radiation measurements. The readings obtained with them are not so valuable as those got with flat-surfaced instruments, as they do not get cooled so much. While the flat surface gets cooled 10° or 11° below the temperature of the air on a clear night, the large ball falls only 5° or 6° and the small one not so much by 1° or 2°. When in use, the balls are fixed to a post at the same height as the screen, and at no great distance from it. This method of taking radiation temperatures at night seems to be better than the one in general use. The

plan generally adopted of taking the readings of a thermometer placed on the grass is open to many objections. The temperature indicated by a thermometer so placed is affected by the greater or less amount of heat communicated upwards from the ground, by the amount of air circulating just at the place where the bulb happens to be, and other local influences, to many of which the ball at four feet from the ground is not exposed. By changing very slightly the position of the bulb of the thermometer placed on the grass, we can greatly alter its readings, whereas this is not the case with the ball at four feet from the ground. In the large ball used here there are fixed two tubes in a horizontal position. One of these tubes holds a maximum, the other a minimum thermometer.

Returning to the table showing the difference in the readings of the Stevenson and the C screens. In the fourth column will be found the effect of solar radiation in heating the 40 cm. ball. The figures given are not the temperatures given by the ball, but the excess of this temperature above that of the air, or, in other words, it is the heating effect of the radiation. For instance, on the 23rd of July the temperature of the ball was 84°, the air was 64°, and the heating effect of the sun was thus 20° as entered. In the fifth column are the solar radiation temperatures taken by the black bulb *in vacuo.* These temperatures are treated in the same way as those given by the black ball; the figures show how much the thermometer was heated above the temperature of the air.

It will be noticed that though the error of the Stevenson screen is due to radiation, yet it follows the indications of neither of these radiation thermometers. This is quite to be expected, because the error of the screen is the effect of radiation as modified by wind. Though the readings of the ball give the effect of radiation as modified by wind, yet these readings alone do not tell us how much they are affected by wind. For instance, the black ball might be heated to only a small amount, either by a strong sun checked by a strong wind, or by a feeble radiation unchecked by wind. If, however, we compare the temperatures of the black ball with those of the black bulb *in vacuo*, we at once see the effect of the wind. No two of these readings for any day bear the same relation to each other. When there was much wind, the black ball was heated only to about 0·4 times the temperature of the black bulb; whereas when there was little wind

it was heated to 0·66 times. If then we divide the temperature of the black ball by that of the black bulb, we get a series of numbers inversely proportional to the cooling effect of the wind. These numbers are given in the sixth column. If now we multiply the numbers so obtained by the heating effect of the radiation on the black ball, we get a series of numbers representing the combined effects of solar radiation and wind. These numbers are given in the seventh column.

It will be observed that the numbers so calculated vary with the errors of the Stevenson screen given in the third column, and that when they are divided by 10 the figures in the two columns agree fairly well with each other. It will, however, be noticed that the figures agree much better when the weather is calm and settled than when it is stormy. This was probably due to the fact that during the windy days it was also cloudy, with only short gleams of sunshine, sufficient to heat up the black ball and black bulb, but not long enough continued to heat up the screens; or it may have been due to the maximum radiation temperatures not having happened at the same time as the maximum air temperature. The errors during the cloudy weather were thus probably smaller than they would have been if there had been continued sunshine.

It would thus appear that, by taking observations with a black ball and a black bulb *in vacuo,* we can calculate pretty well what will be the excess in the readings of the Stevenson screen over those of the C screen. And further, a comparison of readings taken in this way also tells us something about the climate of the place which cannot be ascertained by an examination of the readings of the black bulb *in vacuo* alone. On the 24th July, for instance, the black bulb *in vacuo* was heated 69° above the temperature of the air, while the black ball was only heated 28°, and the error of the Stevenson screen was only 1°; whereas on the 18th August, when the black bulb *in vacuo* was not heated quite so much, the black ball had its temperature raised 42°·5, and the error in the Stevenson screen was 2°·6. As the thermometer in the Stevenson screen is influenced very much in the same way as our bodies, by radiation modified by wind, we may look on the figures given in the seventh column of the table as representing more nearly the climatic conditions than those given by any other method at present in use.

14. On the Quotient of a Simple Alternant by the Difference-Product of the Variables. By Thomas Muir, M.A., LL.D.

On 17th March 1879 I gave to the Royal Society of Edinburgh an account of some researches on the subject of Alternants. A short descriptive abstract of the communication was published in the *Proceedings*, vol. x. pp. 102, 103, the concluding paragraph of which is as follows:—

> "Now it is well known that the alternant whose indices are in order 0, 1, 2, 3, ... is equal to the difference-product of its variables. In regard to every other alternant it is evident that it must contain the said difference-product as a factor, but what the co-factor should be is not so readily seen. In particular cases, doubtless, it can be found without much difficulty, but a general method of obtaining it has hitherto been a desideratum. Such a general method the author has discovered along with a number of less important results bearing on the same special form of determinant."

These results were never published, as, very shortly after the date referred to, the volume of the *Giornale di Matematica* for 1878 arrived, and I found that what I had looked upon as my most important theorem, was given and proved by Garbieri at the beginning of the volume.

Now, however, that fresh interest in the subject has been awakened by the very able papers of Professor Woolsey Johnson, which have recently appeared in the *American Journal of Mathematics* (vii. pp. 345–352, 380–388) and *Quarterly Journal of Mathematics* (xxi. pp. 217–224), I desire to resuscitate my method. Happily there is no question of priority involved. The methods are diverse, and the superiority lies entirely with Professor Johnson's. His reduction-theorem

$$a(0, p, q) = \mathrm{H}_{q-2, p-1} + abc \, . \, a(0, p-1, q-2),$$

his discovery of the existence of the symmetric function Q which is the difference of $a(0, p, q, r)$ and $a(1, p, q, r-1)$, and his mode of determining the said function, are things of which I had not dreamed, and which I consider the most notable results added to the theory of alternants for a great many years.

The quotient

$$\begin{vmatrix} 1 & a^3 & a^6 \\ 1 & b^3 & b^6 \\ 1 & c^3 & c^6 \end{vmatrix} \div \begin{vmatrix} 1 & a & a^2 \\ 1 & b & b^2 \\ 1 & c & c^2 \end{vmatrix},$$

i.e. $$| a^0b^3c^6 | \div | a^0b^1c^2 |,$$

or, say, $$A(0, 3, 6) \div A(0, 1, 2),$$

or, still more shortly, $$\mathfrak{a}(0, 3, 6),$$

is evidently a homogeneous symmetric function of degree $3+6-(1+2)$, *i.e.* 6, and when expressed as a sum of single symmetric functions will be of the form

$$\Sigma a^4b^2 + x\Sigma a^4bc + y\Sigma a^3b^3 + z\Sigma a^3b^2c + u\Sigma a^2b^2c^2.$$

The problem is to determine x, y, z, u.

Professor Johnson's method is to use repeatedly his above-mentioned reduction-theorem. Thus

$$\begin{aligned} \mathfrak{a}(0,3,6) &= H_{4,2} + abcH_{2,1} + a^2b^2c^2H_{0,0}, \\ &= \left.\begin{aligned} &\Sigma a^4b^2 + \Sigma a^4bc + \Sigma a^3b^3 + \Sigma a^3b^2c + \Sigma a^2b^2c^2 \\ &\qquad + abc(\Sigma a^2b + \Sigma abc) \\ &\qquad + a^2b^2c^2, \end{aligned}\right\} \\ &= \Sigma a^4b^2 + \Sigma a^4bc + \Sigma a^3b^3 + 2\Sigma a^3b^2c + 3\Sigma a^2b^2c^2. \end{aligned}$$

My method contrasts with this in that it determines the coefficients x, y, z, u separately. Besides, therefore, being of interest as throwing a side-light on Professor Johnson's method, it may be found useful when only one or a very few coefficients are wanted, and it has certainly been the means of arriving at several more or less noteworthy results.

The basis of it is the expansion of the alternants in terms of alternants of lower orders.

Alternants of Third Order.

First Example.—Required the coefficient of Σa^3b^2c in the expansion of $\mathfrak{a}(0, 3, 6)$.

Solution.—Write the integers from 0 to 6, and separate them into three groups A, B, C, by putting a bar before 0, 3 and 6; thus

$$\begin{array}{|ccc|ccc|c} 0 & 1 & \cancel{2} & 3 & 4 & 5 & 6. \\ & A & & & B & & C \end{array}$$

In the group A delete those which are greater than the index of c in Σa^3b^2c. Find in how many ways a number from group A, with a number from group B, will give the sum 4, 4 being 1 more than the sum of the last two indices in Σa^3b^2c. This number of times, 2, is the coefficient required.

The solution is here put at greater length than need be, in order that when taken with the corresponding solution for the case of alternants of the fourth order, the generality of the method may be apparent. It may be enunciated quite shortly as a theorem, viz.:—

The coefficient of $\Sigma a^x b^y c^z$ *in the expansion of* $|a^0b^pc^q| \div |a^0b^1c^2|$ *is the number of ways in which by taking a number from*

$$0, 1, 2, \ldots, z-1, z$$

and a number from

$$p, p+1, \ldots, q-2, q-1$$

the sum $y+z+1$ *may be obtained.*

Second Example.—Required the coefficient of $\Sigma a^3b^3c^2$ in the expansion of $\alpha(0, 3, 8)$.

Solution.—The two groups here are 0, 1, 2 and 3, 4, 5, 6, 7; and the sum 6 can be made up from them in *three* ways, viz. $0+6$, $1+5$, $2+4$. The coefficient therefore is 3.

Alternants of Fourth Order.

First Example.—Required the coefficient of $\Sigma a^4b^2c^2d$ in the expansion of $\alpha(0, 2, 4, 9)$.

Solution.—Write the integers from 0 to 9, and separate them into four groups A, B, C, D, by putting a bar before 0, 2, 4 and 9; thus

0 1	2 3	4 5 6 7 8	9
A	B	C	D

In group A delete, if necessary, those which are greater than the index of d in $\Sigma a^4b^2c^2d$. Take the integers from 0 to the index of d inclusive, and unite each with such a number as will make the sum 4, that is to say, 1 more than the sum of the last two indices of $\Sigma a^4b^2c^2d$: this gives

$$0, 4; \quad 1, 3.$$

Take three numbers, one from A, one from B, and one from C,

whose sum is 8, that is, $1+2$ more than the sum of the last three indices of $\Sigma a^4b^2c^2d$: this gives

$$0,\ 2,\ 6;\quad 0,\ 3,\ 5;\quad 1,\ 2,\ 5;\quad 1,\ 3,\ 4.$$

Bring then each of the pairs to each of the triads, thus

$$\begin{array}{c} h,\quad k \\ l,\quad m,\quad n, \end{array}$$

and inquire if they dovetail, that is to say, if $k<n \not< m$, and $h<m \not< l$. It is seen that 0, 4 dovetails *two* times, and 1, 3 *four* times; that is, altogether, *six* times. The coefficient required is 6.

I had never attempted to deal with expansions of so high a degree as $\alpha(0, 5, 13, 17)$ which is Professor Johnson's example. But the following will show that the work necessary for obtaining three consecutive coefficients of this expansion is not heavy.

Second Example. — Required the coefficients of $\Sigma a^{10}b^9c^7d^3$, $\Sigma a^{10}b^9c^6d^4$, $\Sigma a^{10}b^9c^5d^5$ in the expansion of $\alpha(0, 5, 13, 17)$.

Solution.—The groups are—

0 1 2 3 ~~4~~	5 6 7 8 9 10 11 12	13 14 15 16	17.
A	B	C	D

Pairs with sum 11, *i.e.* $1+(3+7)$, and lowest member $\not> 3$.

$$0,\ 11;\quad 1,\ 10;\quad 2,\ 9;\quad 3,\ 8.$$

Triads from A, B and C with sum 22, *i.e.* $1+2+(3+7+9)$.

$$\begin{array}{llll} 0,\ 6,\ 16; & 0,\ 7,\ 15; & 0,\ 8,\ 14; & 0,\ 9,\ 13; \\ 1,\ 5,\ 16; & 1,\ 6,\ 15; & 1,\ 7,\ 14; & 1,\ 8,\ 13; \\ 2,\ 5,\ 15; & 2,\ 6,\ 14; & 2,\ 7,\ 13; & \\ 3,\ 5,\ 14; & 3,\ 6,\ 13. & & \end{array}$$

Number of dovetailings in the case of	0, 11	4,
" " "	1, 10	8,
" " "	2, 9	11,
" " "	3, 8	12:
	Total,	35,

which is the first coefficient required.

In finding the coefficient of $\Sigma a^{10}b^9c^6d^4$ the integer 4 would not be deleted in group A, and there would therefore be another triad 4, 5, 13. There would also be another pair 4, 17, and consequently

the number of dovetailings would be increased by $2+3+3+2+1$, *i.e.* 11; so that the coefficient in this instance would be 46.

In finding the coefficient of $\Sigma a^{10}b^9c^5d^5$ the only new point of difference would be an additional pair, 5, 6. This would cause $1+1+1+1$ additional dovetailings, and would consequently make the coefficient $46+4$, *i.e.* 50.

The method may sometimes be used to obtain more general results. Thus—

Third Example.—Required the coefficient of $\Sigma a^{s-7}b^2c^2d^2$ in the expansion of $a(0, 1, 4, s)$, where of course $s-7>2$, that is, $s>9$.

0	1 2 3	4 5 . . . . . $s-1$	s
A	B	C	D

Duads: 0, 5; 1, 4; 2, 3.

Triads: 0, 1, 8; 0, 2, 7; 0, 3, 6.

Number of dovetailings; $3+2+1$, *i.e.* 6, which is the coefficient required, and which, be it observed, is independent of s.

In this way have been obtained several important theorems, to which I shall now pass. The first is—

The expansion of $|a^0b^1c^3d^{3+s}| \div |a^0b^1c^2d^3|$ *or* $a(0\ 1\ 3\ 3+s)$ *consists of all the single symmetric functions which have no index greater than s, and the sum of whose indices is* $s+1$, *the coefficient of each function being less by* 1 *than the number of different letters appearing in any of its terms.* . . . . (I.)

For example, to find the expansion of $|a^0b^1c^3d^7| \div |a^0b^1c^2d^3|$ or $a(0\ 1\ 3\ 7)$ we subtract each index of the divisor from the corresponding index of the dividend, and thus are led to the first of the symmetric functions in the quotient, viz. Σa^4b. Then writing down the succeeding symmetric functions $\Sigma a^3b^2, \Sigma a^3bc, \Sigma a^2b^2c, \Sigma a^2bcd$, and prefixing to each a coefficient less by 1 than the number of letters appearing in any term of the function, we have

$$a(0\ 1\ 3\ 7) = \Sigma a^4b + \Sigma a^3b^2 + 2\Sigma a^3bc + 2\Sigma a^2b^2c + 3\Sigma a^2bcd.$$

The proof is as follows:—The only functions which can occur in the expansion must be of the form $\Sigma a^\alpha b^\beta$, $\Sigma a^\alpha b^\beta c^\gamma$, $\Sigma a^\alpha b^\beta c^\gamma d^\delta$; consequently all that we have to do is to determine the coefficients of

these forms. The groups into which the integers from 0 to $3+s$ must be divided are

$$|\ 0\ |\ 1\ 2\ |\ 3\ 4\ \ldots\ldots\ |\ 3+s$$

and for the case of $\Sigma a^\alpha b^\beta c^\gamma d^\delta$

the duads are

$$0,\ \gamma+\delta+1;\quad 1,\ \gamma+\delta;\quad \ldots\ldots;\ \delta,\ \gamma+1:$$

the triads

$$0,\ 1,\ \beta+\gamma+\delta+2;\quad 0,\ 2,\ \beta+\gamma+\delta+1:$$

and therefore the number of dovetailings 3, which is the required coefficient.

For the case of $\Sigma a^\alpha b^\beta c^\gamma$ the only duad is

$$0,\ \gamma+1;$$

the triads are

$$0,\ 1,\ \beta+\gamma+2;\quad 0,\ 2,\ \beta+\gamma+1,$$

and consequently the number of dovetailings is 2, as was required to be shown.

For the case of $\Sigma a^\alpha b^\beta$, there is again only one duad

$$0,\ 1;$$

and two triads

$$0,\ 1,\ \beta+2;\quad 0,\ 2,\ \beta+1:$$

and therefore manifestly only 1 dovetailing.

Our first theorem is thus established.

The second is—*The expansion of* $|\,a^0b^2c^3d^{3+s}\,|\div|\,a^0b^1c^2d^3\,|$ *or* $a(0,\ 2,\ 3,\ 3+s)$ *consists of the single symmetric function* Σa^sbc *and all the like functions succeeding it, the coefficient of every term involving three letters being* 1, *and the coefficient of every term involving four letters being* 3. . . . . (II.)

For example,

$$a(0\ 2\ 3\ 6)=\Sigma a^3bc+\Sigma a^2b^2c+3\Sigma a^2bcd\,.$$

Here only two forms of terms require to be considered, viz. $\Sigma a^\alpha b^\beta c^\gamma$ and $\Sigma a^\alpha b^\beta c^\gamma d^\delta$. The groups into which the integers $0,\ 1,\ 2,\ \ldots\ldots,\ 3+s$ must be separated are

$$|\ 0\ 1\ |\ 2\ |\ 3\ 4\ 5\ \ldots\ldots\ |\ 3+s\,.$$

For the case of $\Sigma a^\alpha b^\beta c^\gamma d^\delta$, the duads are

$$0,\ \gamma+\delta+1;\quad 1,\ \gamma+\delta;\ \ldots\ldots\ldots;\ \delta,\ \gamma+1:$$

the triads are

$$0,\ 2,\ \beta+\gamma+\delta+1;\quad 1,\ 2,\ \beta+\gamma+\delta:$$

and consequently the number of dovetailings 3, as has been asserted.

For the case of $\Sigma a^{\alpha}b^{\beta}c^{\gamma}$, there is only one duad

$$0,\ \gamma+1;$$

two triads

$$0,\ 2,\ \beta+\gamma+1;\quad 1,\ 2,\ \beta+\gamma,$$

and therefore, as is at once seen, only 1 dovetailing. And this establishes the theorem.

These two theorems and one well known before this (*v.* Theory of Determinants, § 123) constitute an interesting group.

Denoting by σ_n the sum of all the symmetric functions whose terms involve n letters and are of the s^{th} degree, we may write the third theorem referred to in the form

$$a(0,\ 1,\ 2,\ 3+s)=\sigma_1+\sigma_2+\sigma_3+\sigma_4.$$

But by the two new theorems

$$\begin{aligned} a(0,\ 1,\ 3,\ 2+s) &= \sigma_2+2\sigma_3+3\sigma_4, \\ a(0,\ 2,\ 3,\ 1+s) &= \sigma_3+3\sigma_4. \end{aligned}$$

And again by the third theorem

$$a(1,\ 2,\ 3,\ s) = \sigma_4.$$

We can thus express $\sigma_1, \sigma_2, \sigma_3, \sigma_4$, in terms of the four functions on the left, the result evidently being

$$\begin{aligned} \sigma_1 &= a(0\,1\,2\,3+s)-a(0\,1\,3\,2+s)+a(0\,2\,3\,1+s)-a(1\,2\,3\,s), \\ \sigma_2 &= a(0\,1\,3\,2+s)-2\,.\,a(0\,2\,3\,1+s)+3\,.\,a(1\,2\,3\,s), \\ \sigma_3 &= a(0\,2\,3\,1+s)-3\,.\,a(1\,2\,3\,s), \\ \sigma_4 &= a(1\,2\,3\,s). \end{aligned}$$

Of course if these last could be established independently, we should have a very simple additional method of obtaining the identities from which they are here derived.

The third theorem is—*The expansion of*

$$a(0,\ 1,\ q,\ r)\ -\ a(0,\ 2,\ q,\ r-1)\ +\ a(1,\ 2,\ q,\ r-2)$$

consists of the symmetric function $\Sigma a^{r-3}b^{q-2}$ *and the like functions succeeding it, the coefficient in every case being* 1. . . . . (III.)

For example,

$$a(0\ 1\ 3\ 7) - a(0\ 2\ 3\ 6) + a(1\ 2\ 3\ 5)$$
$$= \Sigma a^4b + \Sigma a^3b^2 + \Sigma a^3bc + \Sigma a^2b^2c + \Sigma a^2bcd\,,$$

or, as we may for shortness' sake write,

$$= [\Sigma a^4b +]\,.$$

Seeking first for the coefficient of $\Sigma a^\alpha b^\beta$ we find that it is determined for $a(0\ 1\ q\ r)$ by considering

$$\text{the duad } 0,\ 1 \qquad \text{along with the triads} \begin{cases} 0, & 1,\ \beta+2 \\ 0, & 2,\ \beta+1 \\ 0, & 3,\ \beta \\ \cdots & \cdots \\ 0, & q-1,\ \beta-q+4\,; \end{cases}$$

that it is determined for $a(0,\ 2,\ q,\ r-1)$ by considering

$$\text{the duad } 0,\ 1 \qquad \text{along with the triads} \begin{cases} 0, & 2,\ \beta+1 \\ 0, & 3,\ \beta \\ \cdots & \cdots \\ 0, & q-1,\ \beta-q+4 \end{cases}$$

$$\text{and the triads} \begin{cases} 1 & 2\ \ \beta \\ 1 & 3\ \ \beta-1 \\ \cdots & \cdots \\ 1 & q-1\ \ \beta-q+3\,; \end{cases}$$

and that it is determined for $a(1,\ 2,\ q,\ r-2)$ by considering

$$\text{the duad } 0,\ 1 \qquad \text{along with the triads} \begin{cases} 1 & 2\ \ \beta \\ 1 & 3\ \ \beta-1 \\ \cdots & \cdots \\ 1 & q-1\ \ \beta-q+3. \end{cases}$$

Now if the number of dovetailings in the first case be x, and in the last case y, it is clear that in the second case they must be $(x-1)+y$: hence the coefficient of every term of the form $\Sigma a^\alpha b^\beta$ in $a(0,\ 1,\ q,\ r) - a(0,\ 2,\ q,\ r-1) + a(1,\ 2,\ q,\ r-2)$ is 1.

Next, the coefficient of $\Sigma a^\alpha b^\beta c^\gamma$ is determined for $a(0,\ 1,\ q,\ r)$ by considering

the duad $0,\ \gamma+1$ along with the triads $\left\{\begin{array}{l} 0,\quad 1,\ \beta+\gamma+2 \\ 0,\quad 2,\ \beta+\gamma+1 \\ \cdots\cdots\cdots \\ 0,\ q-1,\ \beta+\gamma-q+4\,; \end{array}\right.$

for $a(0,\ 2,\ q,\ r-1)$ by considering

the duad $0,\ \gamma+1$ along with the triads $\left\{\begin{array}{l} 0,\quad 2,\ \beta+\gamma+1 \\ \cdots\cdots\cdots \\ 0,\ q-1,\ \beta+\gamma-q+4 \end{array}\right.$

and the triads $\left\{\begin{array}{l} 1,\quad 2,\ \beta+\gamma \\ 1,\quad 3,\ \beta+\gamma-1 \\ \cdots\cdots\cdots \\ 1,\ q-1,\ \beta+\gamma-q+3\,; \end{array}\right.$

and for $a(1,\ 2,\ q,\ r-2)$ by considering

the duad $0,\ \gamma+1$ along with the triads $\left\{\begin{array}{l} 1,\quad 2,\ \beta+\gamma \\ 1,\quad 3,\ \beta+\gamma-1 \\ \cdots\cdots\cdots \\ 1,\ q-1,\ \beta+\gamma-q+3\,. \end{array}\right.$

Hence, as before, the number of dovetailings is x', $(x'-1)+y'$, y'; and, therefore, the coefficient required is

$$x'-\{(x'-1)+y'\}+y'$$
$$i.e.\ 1.$$

Lastly, the coefficient of $\Sigma a^\alpha b^\beta c^\gamma d^\delta$ is determined for $a(0,\ 1,\ q,\ r)$ by considering

each of the duads $\left\{\begin{array}{l} 0,\ \gamma+\delta+1 \\ 1,\ \gamma+\delta \\ \cdots\cdots \\ \delta,\ \gamma+1 \end{array}\right.$ along with each of the triads $\left\{\begin{array}{l} 0,\quad 1,\ \beta+\gamma+\delta+2 \\ 0,\quad 2,\ \beta+\gamma+\delta+1 \\ \cdots\cdots\cdots\cdots \\ 0,\ q-1,\ \beta+\gamma+\delta-q+4\,; \end{array}\right.$

for $a(0,\ 2,\ q,\ r-1)$ by considering

each of the duads $\left\{\begin{array}{l} 0,\ \gamma+\delta+1 \\ 1,\ \gamma+\delta \\ \cdots\cdots \\ \delta,\ \gamma+1 \end{array}\right.$ along with each of the triads $\left\{\begin{array}{l} 0,\quad 2,\ \beta+\gamma+\delta+1 \\ \cdots\cdots\cdots\cdots \\ 0,\ q-1,\ \beta+\gamma+\delta-q+4 \end{array}\right.$

and along with each of the triads $\left\{\begin{array}{l} 1,\quad 2,\ \beta+\gamma+\delta \\ \cdots\cdots\cdots\cdots \\ 1,\ q-1,\ \beta+\gamma+\delta-q+3\,; \end{array}\right.$

and for $\alpha(1, 2, q, r-2)$ by considering

$$\text{each of the duads}\left\{\begin{array}{l} 0,\ \gamma+\delta+1 \\ 1,\ \gamma+\delta \\ \ldots\ldots \\ \delta,\ \gamma+1 \end{array}\right. \quad \begin{array}{c}\text{along with each} \\ \text{of the triads}\end{array} \left\{\begin{array}{l} 1,\quad 2,\ \beta+\gamma+\delta \\ \ldots\ldots\ldots \\ \\ 1,\ q-1,\ \beta+\gamma+\delta-q+3. \end{array}\right.$$

There is more trouble here in comparing the number of dovetailings in the case of $\alpha(0, 1, q, r)$ with the first set in the case of $\alpha(0, 2, q, r-1)$, but the result is the same as before, viz. x'' and $x''-1$; so that the coefficient is again 1.

And thus the theorem is established.

The case where $q=r-1$ is important, as it furnishes a result similar to those of the first two theorems. Putting $q=2+s$ and $r=3+s$ we obtain

$$\begin{aligned} \alpha(0,1,2+s,3+s) &= [\Sigma a^s b^s +] + \alpha(0,2,2+s,2+s) - \alpha(1,2,2+s,1+s) \\ &= [\Sigma a^s b^s +] + \qquad 0 \qquad + \alpha(1,2,1+s,2+s) \\ &= [\Sigma a^s b^s +] + abcd\,.\,\alpha(0,1,s,1+s)\,; \end{aligned}$$

and as the $\alpha(\quad)$ on the right is the same function of s, as the $\alpha(\quad)$ on the left is of $s+2$, we see that by repeated application of the result, the full expansion of $\alpha(0, 1, 2+s, 3+s)$ in terms of symmetric functions is obtainable. We have, in fact,

$$\alpha(0, 1, 2+s, 3+s)$$

$$\begin{aligned} &= [\Sigma a^s b^s +] + abcd\{[\Sigma a^{s-2} b^{s-2} +] + abcd\,.\,\alpha(0, 1, s-2, s-1)\} \\ &= [\Sigma a^s b^s +] + [\Sigma a^{s-1} b^{s-1} cd +] + [\Sigma a^{s-2} b^{s-2} c^2 d^2 +] + \quad \ldots\ldots \quad \text{(IV.)} \end{aligned}$$

For example,

$$\begin{aligned} \alpha(0, 1, 6, 7) &= [\Sigma a^4 b^4 +] + [\Sigma a^3 b^3 cd +] + [\Sigma a^2 b^2 c^2 d^2 +], \\ &= \left.\begin{aligned} &\Sigma a^4 b^4 + \Sigma a^4 b^3 c + \Sigma a^4 b^2 c^2 + \Sigma a^4 b^2 cd + \Sigma a^3 b^3 c^2 + \Sigma a^3 b^3 cd + \Sigma a^3 b^2 c^2 d + \Sigma a^2 b^2 c^2 d^2 \\ &\qquad + \Sigma a^3 b^3 cd + \Sigma a^3 b^2 c^2 d + \Sigma a^2 b^2 c^2 d^2 \\ &\qquad + \Sigma a^2 b^2 c^2 d^2, \end{aligned}\right\} \\ &= \Sigma a^4 b^4 + \Sigma a^4 b^3 c + \Sigma a^4 b^2 c^2 + \Sigma a^4 b^2 cd + \Sigma a^3 b^3 c^2 + 2\Sigma a^3 b^3 cd + 2\Sigma a^3 b^2 c^2 d + 3\Sigma a^2 b^2 c^2 \end{aligned}$$

The fifth theorem is, in symbols,

$$\alpha(0, 2, 2+s, 3+s) - \alpha(1, 2, 1+s, 3+s) = [\Sigma a^s b^s c +]\,.\,. \quad \text{(V.)}$$

The only point of the proof which requires care is where the dovetailings of the

$$\text{duads}\begin{cases} 0,\ \gamma+\delta+1 \\ 1,\ \gamma+\delta \\ 2,\ \gamma+\delta-1 \\ \cdots\cdots \\ \delta,\ \gamma+1 \end{cases} \quad \text{with the triads} \begin{cases} 0,\ \beta+\gamma+\delta-s+1,\ 2+s \\ 1,\ \beta+\gamma+\delta-s\quad,\ 2+s \end{cases}$$

have to be shown to be 1 more than those of the same duads

$$\text{with the triads} \begin{cases} 1,\ \beta+\gamma+\delta-s+1,\ 1+s \\ 1,\ \beta+\gamma+\delta-s,\quad 2+s. \end{cases}$$

The second triad in the two cases being the same may be neglected; and then we have only to note that the middle elements of the remaining triads are the same, and that the final elements $2+s$ and $1+s$ are, for the purpose in view, as good as if they were the same, because the lesser of them is greater than $\gamma+\delta$.

Returning now to the third theorem, and putting $q=2+s$, and $r=4+s$ we have

$$a(0,\,1,\,2+s,\,4+s)=[\Sigma a^{s+1}b^s+\,]+a(0,\,2,\,2+s,\,3+s)-a(1,\,2,\,2+s,2+s).$$

But by the preceding theorem (v.)

$$a(0,\,2,\,2+s,\,3+s)=[\Sigma a^sb^sc+\,]+abcd\,.\,a(0,\,1,\,s,\,s+2)\,.$$

Hence

$$a(0,1,2+s,4+s)=[\Sigma a^{s-1}b^s+\,]+[\Sigma a^sb^sc+\,]+abcd.\,a(0,1,s,s+2) \quad \text{(vi.)}$$

—a theorem which enables us to write down the full expansion of $a(0,\,1,\,2+s,\,4+s)$ in terms of symmetric functions, because $a(0,\,1,\,s,\,s+2)$ is the same function of s that $a(0,\,1,\,2+s,\,4+s)$ is of $s+2$.

Further, the fifth theorem, with the help of the expansion just obtained, gives us the like expansion for $a(0,\,2,\,2+s,\,3+s)$.

All the theorems assist materially in lightening the labour of tabulating the expansions of the a functions. The following are the tables for the functions of order 4 and degrees 1 to 9.

Degree 1.

$a(0124)=\Sigma a.$

Degree 2.

$a(0125)=\Sigma a^2+\Sigma ab.$
$\quad(0134)=\qquad\Sigma ab.$

Degree 3.

$$\alpha(0126) = \Sigma a^3 + \Sigma a^2 b + \Sigma abc.$$
$$\alpha(0135) = \Sigma a^2 b + 2\Sigma abc.$$
$$\alpha(0234) = \Sigma abc.$$

Degree 4.

$$\alpha(0127) = \Sigma a^4 + \Sigma a^3 b + \Sigma a^2 b^2 + \Sigma a^2 bc + \Sigma abcd.$$
$$\alpha(0136) = \Sigma a^3 b + \Sigma a^2 b^2 + 2\Sigma a^2 bc + 3\Sigma abcd.$$
$$\alpha(0145) = \Sigma a^2 b^2 + \Sigma a^2 bc + 2\Sigma abcd.$$
$$\alpha(0235) = \Sigma a^2 bc + 3\Sigma abcd.$$
$$\alpha(1234) = \Sigma abcd.$$

Degree 5.

$$\alpha(0128) = \Sigma a^5 + \Sigma a^4 b + \Sigma a^3 b^2 + \Sigma a^3 bc + \Sigma a^2 b^2 c + \Sigma a^2 bcd.$$
$$\alpha(0137) = \Sigma a^4 b + \Sigma a^3 b^2 + 2\Sigma a^3 bc + 2\Sigma a^2 b^2 c + 3\Sigma a^2 bcd.$$
$$\alpha(0146) = \Sigma a^2 b^2 + \Sigma a^3 bc + 2\Sigma a^2 b^2 c + 3\Sigma a^2 bcd.$$
$$\alpha(0236) = \Sigma a^3 bc + \Sigma a^2 b^2 c + 3\Sigma a^2 bcd.$$
$$\alpha(0245) = \Sigma a^2 b^2 c + 2\Sigma a^2 bcd.$$
$$\alpha(1235) = \Sigma a^2 bcd.$$

Degree 6.

	Σa^6	$\Sigma a^5 b$	$\Sigma a^4 b^2$	$\Sigma a^4 bc$	$\Sigma a^3 b^3$	$\Sigma a^3 b^2 c$	$\Sigma a^3 bcd$	$\Sigma a^2 b^2 c^2$	$\Sigma a^2 b^2 cd$
$\alpha(0129)$	1	1	1	1	1	1	1	1	1
$\alpha(0138)$		1	1	2	1	2	3	2	3
$\alpha(0147)$			1	1	1	2	3	3	4
$\alpha(0237)$				1	0	1	3	1	3
$\alpha(0156)$					1	1	1	1	2
$\alpha(0246)$						1	2	2	4
$\alpha(1236)$							1	0	1
$\alpha(0345)$								1	1
$\alpha(1245)$									1

Degree 7.

	7000	6100	5200	5110	4300	4210	4111	3310	3220	3211	2221
$\alpha(012\overline{10})$	1	1	1	1	1	1	1	1	1	1	1
$\alpha(0139)$		1	1	2	1	2	3	2	2	3	3
$\alpha(0148)$			1	1	1	2	3	2	3	4	5
$\alpha(0238)$				1	0	1	3	1	1	3	3
$\alpha(0157)$					1	1	1	2	2	3	4
$\alpha(0247)$						1	2	1	2	4	6
$\alpha(1237)$							1	0	0	1	1
$\alpha(0256)$								1	1	2	3
$\alpha(0346)$									1	1	3
$\alpha(1246)$										1	2
$\alpha(1345)$											1

Degree 8.

	8000	7100	6200	6110	5300	5210	5111	4400	4310	4220	4211	3320	3311	3221	2222
$\alpha(012\overline{11})$	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
$\alpha(013\overline{10})$		1	1	2	1	2	3	1	2	2	3	2	3	3	3
$\alpha(0149)$			1	1	1	2	3	1	2	3	4	3	4	5	6
$\alpha(0239)$				1	0	1	3	0	1	1	3	1	3	3	3
$\alpha(0158)$					1	1	1	1	2	2	3	3	4	5	6
$\alpha(0248)$						1	2	0	1	2	4	2	4	6	8
$\alpha(1238)$							1	0	0	0	1	0	1	1	1
$\alpha(0167)$								1	1	1	1	1	2	2	3
$\alpha(0257)$									1	1	2	2	4	5	7
$\alpha(0347)$										1	1	1	1	3	6
$\alpha(1247)$											1	0	1	2	3
$\alpha(0356)$												1	1	2	3
$\alpha(1256)$													1	1	2
$\alpha(1346)$														1	3
$\alpha(2345)$															1

Degree 9.

	9000	8100	7200	7110	6300	6210	6111	5400	5310	5220	5211	4410	4320	4311	4221	3330	3321	3222
$\alpha(012\overline{12})$	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
$\alpha(013\overline{11})$		1	1	2	1	2	3	1	2	2	3	2	2	3	3	2	3	3
$\alpha(014\overline{10})$			1	1	1	2	3	1	2	3	4	2	3	4	5	3	5	6
$\alpha(023\overline{10})$				1	0	1	3	0	1	1	3	1	1	3	3	1	3	3
$\alpha(0159)$					1	1	1	1	2	2	3	2	3	4	5	4	6	7
$\alpha(0249)$						1	2	0	1	2	4	1	2	4	6	2	6	8
$\alpha(1239)$							1	0	0	0	1	0	0	1	1	0	1	1
$\alpha(0168)$								1	1	1	1	2	2	3	3	2	4	5
$\alpha(0258)$									1	1	2	1	2	4	5	3	7	9
$\alpha(0348)$										1	1	0	1	1	3	1	3	6
$\alpha(1248)$											1	0	0	1	2	0	2	3
$\alpha(0267)$												1	1	2	2	1	3	4
$\alpha(0357)$													1	1	2	2	4	6
$\alpha(1257)$														1	1	0	2	3
$\alpha(1347)$															1	0	1	3
$\alpha(0456)$																1	1	1
$\alpha(1356)$																	1	2
$\alpha(2346)$																		1

15. Remarks by the Chairman on Closing the Session.

I am told by our Secretary, whose authority on such a subject I dare not venture to dispute, that on this the last meeting of the Session, the duty falls on the retiring Vice-President of making a few remarks on the business of that Session, including some information as to the number of new and of deceased Fellows, the papers that have been read on each subject, and other similar information.

This duty I now proceed, to the best of my power, to discharge.

It will, I am sure, be gratifying to the members to be told that the supply of papers during the present Session has been even more ample than on former occasions, and I trust that we are not guilty of undue vanity in believing that the contributions of this year show the same deep and patient research, the same knowledge of what has already been done, both in this country and abroad, and the same mastery over the latest and best methods of mathematical and physical investigation, which have hitherto distinguished the Fellows of this Society, and have given value to its transactions in the estimation of similar learned institutions elsewhere.

As matter of statistics, it may be noted that in Physics the number of papers read has been 22, in Mathematics 8, in Astronomy 6, Meteorology 7, Engineering 2, Chemistry 6, Physical Geography 3, Anatomy and Physiology 10, Botany 2, Biology 6, Geology and Palæontology 4, Political Economy 1.

It is also satisfactory to know that of late numerous candidates have sought admission to Fellowship. During the last Session 36 were admitted as Fellows; in the present the number has been 35. Many of these are men of high promise, and not a few have already given evidence of scientific ability by the papers they have published in our *Proceedings* and *Transactions.*

On the other hand, death has, since the opening of the Session, made serious inroads on our ranks. Since last November nine Fellows of our Society have died.

When among these I mention the name of Thomas Stevenson, it is impossible not to think with sadness how short a time has run since, as the valued President of our Society, he occupied this chair, and from it delivered addresses to which we listened with instruction

and pleasure. Those who heard him on these occasions, and those who were associated with him in the discharge of the Society's business, must all deeply regret that his tenure of office was so short, and that the close of his life came so soon. Born of a family to whom we owe the wonderful advance which has taken place in the science of lighthouse-illumination, it was his fortune to carry to still further perfection that branch of scientific knowledge which had been so handed down to him, a branch of knowledge, it need scarcely be added, among the most admirable in its practical results, providing not for one nation alone but "*in salutem omnium*" for the mariners and the argosies of all nations, a series of star-like guides over the otherwise trackless ocean. It has been said in an able and affectionate filial tribute to his memory, which I cannot resist quoting, that "his lights were in every part of the world guiding the mariner; his firm were consulting engineers to the Indian, the New Zealand, and the Japanese Lighthouse Boards; in Germany he had been called the Nestor of lighthouse-illumination; even in France, where his claims were long denied, he was at last, on the occasion of the late Exposition, recognised and medalled." Few men, it has also been truly said, were more beloved in Edinburgh, where he breathed an air that pleased him; and I am sure few men have ever been more highly regarded by this Society than our late President.

It is with feelings of deep and unfeigned regret that we recall the loss which the Society has suffered in the death of Mr Robert Gray. We all feel that in him we have lost a warm personal friend. He was the highest authority on Scottish Ornithology, and had a large and accurate acquaintance with other branches of natural history. He was Secretary of the Royal Physical Society, and by his able and energetic management may almost be said to have given it a new lease of life. He rendered valuable service to our own Association, which devolved on him some of its most delicate and difficult business. His bright and genial presence will long and regretfully be missed at the meetings of this Society and its Council.

The brilliant early career of Adam Gifford, both at the bar and on the bench, and the affecting circumstances under which that career was ended, as it were in the noontide of life, are familiar to

many whose tastes lie altogether outside of the profession which he adorned. But Lord Gifford was not a lawyer merely; and during those years when the shadow of death was upon him he had, along with other and higher consolations, the ample stores of philosophy and poetry, and the varied studies of earlier years.

By the death of Alexander Gibson, advocate, Secretary of the Educational Endowments Commission, the public service, as well as his many personal friends, have suffered a loss which will not easily be repaired. Born at Kirkcaldy—the birth-place, as I need not remind this learned audience, of Adam Smith, the founder of Political Economy as a separate branch of human knowledge,—like Smith, young Gibson received the first of his education at the burgh school of that town. Graduating afterwards in Arts in the University of Edinburgh, he took an active part in the Diagnostic Society, and speedily gained the reputation of a student of great acuteness and of wide reading, both in literature and in law. He was also much interested in natural philosophy and science, and in that department of his professional knowledge which Leibnitz, at once a jurist and a mathematician, pronounced to rank next to geometry in the soundness of its principles and the certainty of its conclusions, the noble system of equity of the Roman law. I quote his words—"I have often said that after the writings of geometricians there exists nothing which, in point of strength, subtility, and depth, can be compared with the works of the Roman lawyers" (Dugald Stewart's Works, by Hamilton, vol. i. p. 186, 1854).*

But to return from this digression. "Time and the hour" forbid my making more than passing mention of many names of which I should willingly have said more in this place.

Mr William Denny of Dumbarton was a distinguished naval constructor, and, as such, a large employer of labour. He took a keen interest in the improvement of the working classes, and his death caused a widely felt sorrow among a large circle of friends, not in this country only but abroad.

* Dixi sæpius, post scripta geometrarum, nihil extare quod vi ac subtilitate cum Romanorum jureconsultorum scriptis comparari potest tantum nervi adest tantum profunditatis (Leibnitz' Works, by Dutens, vol. iv. part 3, pp. 267, 268).

Mr James Pringle, who at the time of his death was Provost of Leith, received his education at the High School of Edinburgh, and was distinguished by his aptitude for classics. During his long connection with Leith he took a prominent part in promoting all its interests, and as chief magistrate rendered important services to the town.

Dr William Brown, who died in January last, was, I believe, the oldest member of this Society, having been admitted in 1835. He had filled the office of President of the Royal College of Surgeons, and enjoyed a high reputation as a consulting physician.

Dr Rutherford Haldane, LL.D., was one of the most respected members of the medical profession in Edinburgh. After leaving our University he went abroad, for further study, to Vienna and Paris, spending eighteen months in the French capital. He became successively a lecturer on Pathology and on the Practice of Medicine in the Extra-Mural School. By his professional brethren he was regarded as an authority on all medical questions. He was a man of great learning; and his early death cannot fail to be much regretted by the Society.

The Rev. Francis Le Grix White, although, from residence at a distance from Edinburgh, personally but little known to many of us, kept up correspondence with the Society, was a man of varied accomplishments, and took an active part in organising scientific lectures in Cumberland and Westmoreland. He also promoted everything which tended to elevate those classes over whom his influence extended.

It only now remains for me to acknowledge the valuable help which I have received in the preparation of these notes from our friend Mr Gordon.

16. Minute of Meeting of Special Committee on the Victoria Jubilee Prize, 27th June 1887.

VICTORIA JUBILEE PRIZE, founded by Dr GUNNING of Rio Janeiro

This Prize, founded in 1887, consisting of the interest of £1000, is to be awarded triennially by the Council of the Royal Society of Edinburgh.

The Prize is to be given in recognition of original work in Physics, Chemistry, or Pure or Applied Mathematics.

Evidence of such work may be afforded either by a paper presented to the Society, or by a paper on one of the above subjects or some discovery in them elsewhere communicated or made, which the Council may consider to be deserving of the Prize.

The Prize is open to men of science resident in, or connected with Scotland.

The first award shall be in the year 1887, and shall consist of a sum of money. In accordance with the wish of the donor, the Council of the Society may on fit occasion award the Prize for work of a definite kind to be undertaken during the succeeding three years by a scientific man of recognised ability.

Before entering on the last of the subjects of this evening's meeting, it will interest you to learn that, heartily joining as we all do in the loyal and affectionate homage of the whole nation on the completion by the Queen of the 50th year of her happy and beneficent reign, the following Address has been forwarded to the Secretary of State for presentation, the arrangements not permitting its presentation by our President:—

"Madam, may it please your Majesty,—We, the President and Council of the Royal Society of Edinburgh, humbly address your Majesty on this, the 50th anniversary of your Majesty's illustrious reign, and desire to express, on behalf of the Society, their loyal attachment to your Majesty, and to the institution of the Crown, represented in the person of our present gracious and distinguished Sovereign.

"The Royal Society of Edinburgh was constituted in the year 1782, for the promotion of scientific and literary research, by a Charter from King George the Third. In times past it has counted amongst its members many statesmen, and men of letters and science, who have discussed freely the public and political questions of the times. But on one subject there has happily been entire unanimity—the maintenance of the principle of constitutional monarchy as established in the British Empire under the guidance of your Majesty and your Royal predecessors.

"We are deeply sensible of the advantages which we have derived, during the past fifty years of social and legislative progress, from the presence, at the head of the affairs of this great Empire, of a Sovereign who is dissociated from the contentions of political parties, and is possessed of those high personal qualities which enable the Crown to exert a moderating and controlling influence in the public life of the country.

"As a Scottish Society, we claim a share in the sentiments of pride and loyal attachment which your Majesty's personal relations with Scotland have evoked in the hearts of our fellow-countrymen. We trust that your Majesty may long continue to derive health and solace from annual visits to your Majesty's Highland home; and we at the same time regard your Majesty's gracious presence amongst us as a guarantee of that interest in, and knowledge of our distinctively national affairs, which, as Scotsmen, we most highly value.

"In addressing the Royal Successor of the ancient Scottish Sovereigns, who in troubled times ruled this country with courage and ability, we may conclude by offering to your Majesty our most respectful and loyal congratulations on the good fortune, peace, and prosperity which have hitherto accompanied your Majesty's brilliant and eventful reign; and by expressing the hope that the interests of science and learning, with which this Society is connected, and whose marvellous development has been one of the characteristic features of that reign, may continue to flourish, as heretofore, under your Majesty's gracious encouragement and protection.

Signed on behalf of the Council of the Royal Society of Edinburgh.

William Thomson, *President.*

June 1887.

17. The Theory of Determinants in the Historical Order of its Development. By Thomas Muir, M.A., LL.D.

PART I. (continued). *Determinants in General* (1779–1812).

Now it is at once manifest that the successive developments here obtained of the determinant $[xyzt]$ are letter by letter identical with the successive "*lignes*" obtained by Bézout from the unreal product $xyzt$; but that instead of having one arbitrary step succeeding another, as in the application of Bézout's rule, there is here a fluent reasonableness characterising the whole process.* As for the peculiarities requiring elucidation in the series of special examples above referred to, they are seen, when looked at in this light, to be but matters of course.

Not only so, but it will be found that the translation of xy into $[xy]$, &c., is an unfailing key to much that follows in Bézout in connection with the subject. For example, let us take the wide extension of the rule which is expounded later on in the treatise, in a section headed

* If the fact at the basis of the process were made use of nowadays, it would be advantageous, of course, in the first instance to simplify the determinant as much as possible. For example, the equations being (Bézout, p. 178)

$$\left.\begin{array}{r} 2x+4y+5z=22 \\ 3x+5y+2z=30 \\ 5x+6y+4z=43 \end{array}\right\},$$

we might proceed as follows:—

$$\begin{vmatrix} 2 & 4 & 5 & -22 \\ 3 & 5 & 2 & -30 \\ 5 & 6 & 4 & -43 \\ x & y & z & t \end{vmatrix} = \begin{vmatrix} 0 & 2 & 11 & -6 \\ 1 & 1 & -3 & -8 \\ 0 & -3 & -3 & 9 \\ x & y & z & t \end{vmatrix}$$

$$= 3\begin{vmatrix} 0 & 0 & 9 & 0 \\ 1 & 0 & -4 & -5 \\ 0 & -1 & -1 & 3 \\ x & y & z & t \end{vmatrix} = 27\begin{vmatrix} 0 & 0 & 1 & 0 \\ 1 & 0 & 0 & -5 \\ 0 & -1 & 0 & 3 \\ x & y & z & t \end{vmatrix}$$

$$=27\{-t+0z-3y-5x\};$$

whence $x=5$, $y=3$, $z=0$.

Considérations utiles pour abréger considérablement le calcul des coëfficients qui servent à l'élimination.

There are in all fifteen pages (pp. 208–223, §§ 252–270) devoted to the subject. The contents of three paragraphs will give a sufficiently clear idea of the nature of the whole. The notation used is identical with that of Laplace, *e.g.*,

$$(ab') = ab' - a'b\,,$$
$$(ab'c'') = (ab' - a'b)c'' - (ab'' - a''b)c' + (a'b'' - a''b')c\,,$$
$$\cdots\cdots\cdots\cdots\cdots\cdots$$

Two of the three selected paragraphs stand as follows :—

"(264.) Cette manière de procéder au calcul des inconnues, en les grouppant, n'est pas applicable seulement à notre objet ; elle peut en général être appliquée dans toutes les équations du premier degré.

"Si l'on avoit, par exemple, les quatre équations suivantes

$$\begin{aligned} ax + by + cz + dt + e &= 0,\\ a'x + b'y + c'z + d't + e' &= 0,\\ a''x + b''y + c''z + d''t + e'' &= 0,\\ a'''x + b'''y + c'''z + d'''t + e''' &= 0. \end{aligned}$$

En se rappellant que chaque inconnue a pour valeur le coëfficient qu'elle se trouve avoir dans la dernière *ligne*, divisé constamment par celui que l'inconnue introduite aura dans cette même *ligne*, on verra bientôt qu'on peut réduire le calcul à chercher le coëfficient de l'une quelconque des inconnues dans la dernière ligne ; parce que de la même manière qu'on en aura calculé un, on calculera de même tous les autres : ou même, lorsqu'on en aura calculé un, on pourra en déduire tous les autres, lorsque les équations auront toute la généralité possible. Or pour avoir la valeur du coëfficient d'une des inconnues dans la dernière ligne, la question se réduit à calculer la valeur du produit des autres inconnues. Mais pour ne pas se tromper sur les signes, il faudra toujours ne pas perdre de vue, la place que cette inconnue est censée occuper dans le produit de toutes les inconnues. Ainsi, dans le cas présent, au lieu de calculer généralement la dernière *ligne* pour avoir *xyztu*, je calcule

seulement cette dernière ligne pour $yztu$: et pour l'avoir de la manière la plus commode, je grouppe en cette manière $yz.tu$, et je procède comme il suit, au calcul des lignes, observant que y est censé à la seconde place.

Première ligne. $-bz.tu-yz.du$,

Seconde ligne. $+(bc').tu-bz.d'u+b'z.du+yz.(de')$,

Troisième ligne. $-(bc').d''u+(bc'').d'u-bz.(d'e'')-(b'c'').du+b'z.(de'')-b''z.(de')$,

Quatrième ligne. $+(bc').(d''e''')-(bc'').(d'e''')+(bc''').(d'e'')+(b'c'').(de''')$
$-(b'c''').(de'')+(b''c''').(de')$;

c'est le coëfficient de x dans la dernière ligne.

"Pour avoir celui de u, je calculerois de même la valeur de $xyzt$, en le grouppant ainsi, $xy.zt$, et je trouverois pour valeur du coëfficient de u dans la dernière ligne, la quantité

$$(ab').(c''d''')-(ab'').(c'd''')+(ab''').(c'd'')+(a'b'').(cd''')$$
$$-(a'b''').(cd'')+(a''b''').(cd');$$

"D'où je conclus

$$x=\frac{+(bc').(d''e''')-(bc'').(d'e''')+(bc''').(d'e'')+(b'c'').(de''')-(b'c''').(de'')+(b''c''').(de')}{(ab').(c''d''')-(ab'').(c'd''')+(ab''').(c'd'')+(a'b'').(cd''')-(a'b''').(cd'')+(a''b''').(cd')};$$

et ainsi de suite.

"(265.) Si j'avois les cinq équations suivantes—

$$\begin{aligned}
ax+by+cz+dr+et+f&=0,\\
a'x+b'y+c'z+d'r+e't+f'&=0,\\
a''x+b''y+c''z+d''r+e''t+f''&=0,\\
a'''x+b'''y+c'''z+d'''r+e'''t+f'''&=0,\\
a^{\text{iv}}x+b^{\text{iv}}y+c^{\text{iv}}z+d^{\text{iv}}r+e^{\text{iv}}t+f^{\text{iv}}&=0.
\end{aligned}$$

Je calculerois, par exemple, le coëfficient de x dans la dernière ligne, en calculant $yzr.tu$, ou $yz.rtu$, ou $yz.rt.u$.

"Si j'avois six équations dont les inconnues fussent x, y, z, r, s et t, je calculerois, par exemple, le coëfficient de x, en calculant ou $yz.rs.tu$, ou $yzrs.tu$, ou $yzr.stu$, et ainsi de suite."

The next paragraph deals with an illustrative example. The twelve equations—

$$
\left.\begin{array}{lllll}
Aa + A'a' + A''a'' & & & & = 0 \\
Ab + A'b' + A''b'' & & & & = 0 \\
Ac + A'c' + A''c'' & + Ba + B'a' + B''a'' & & & = 0 \\
 & + Bb + B'b' + B''b'' & & & = 0 \\
 & + Bc + B'c' + B''c'' & & & = 0 \\
 & + Bd + B'd' + B''d'' & + Ca + C'a' + C''a'' & & = 0 \\
 & & + Cb + C'b' + C''b'' & & = 0 \\
 & & + Cc + C'c' + C''c'' & & = 0 \\
 & & + Cd + C'd' + C''d'' & + Da + D'a' + D''a'' & = 0 \\
 & & & + Db + D'b' + D''b'' & = 0 \\
 & & & + Dc + D'c' + D''c'' & = 0 \\
Ad + A'd' + A''d'' & & & + Da + D'a' + D''a'' & = 0
\end{array}\right\}
$$

are given, and what is required is the result of the elimination (*équation de condition*) of the twelve quantities—$a,a',a'',b,b',b'',c,c',c'',d,d',d''$. This is found to be—

$$(ab'c'').[(bc'd'')^3 - (ab'c'')^2(ab'd'')] = 0.$$

The two paragraphs quoted (§§ 264, 265) show that Bézout could obtain with considerably increased ease and certitude any one of Laplace's expansions of numerator and denominator. What it accomplished in the illustrative example is virtually, in modern symbolism, the reduction of

$$
\left|\begin{array}{cccccccccccc}
a & a' & a'' & . & . & . & . & . & . & . & . & . \\
b & b' & b'' & . & . & . & . & . & . & . & . & . \\
c & c' & c'' & a & a' & a'' & . & . & . & . & . & . \\
. & . & . & b & b' & b'' & . & . & . & . & . & . \\
. & . & . & c & c' & c'' & . & . & . & . & . & . \\
. & . & . & d & d' & d'' & a & a' & a'' & . & . & . \\
. & . & . & . & . & . & b & b' & b'' & . & . & . \\
. & . & . & . & . & . & c & c' & c'' & . & . & . \\
. & . & . & . & . & . & d & d' & d'' & a & a' & a'' \\
. & . & . & . & . & . & . & . & . & b & b' & b'' \\
. & . & . & . & . & . & . & . & . & c & c' & c'' \\
d & d' & d'' & . & . & . & . & . & . & a & a' & a''
\end{array}\right|
$$

to the form

$$|ab'c''|.|bc'd''|^3 - |ab'c''|^3.|ab'd''|.$$

Although this can be done nowadays with ease by means of Laplace's expansion-theorem in its modern garb, it may be safely affirmed that Laplace himself, using his own process, would not have succeeded in making the reduction. Considerable importance thus attaches from more than one point of view to Bézout's curious "rule."

The only other section with which we are concerned bears the heading

> *Méthode pour trouver des fonctions d'un nombre quelconque de quantités, qui soient zéro par elles-mêmes.*

In the second paragraph of the section the principle is explained as follows:—

> "(216) Concevons un nombre n d'équations du premier degré renfermant un nombre $n+1$ d'inconnues, et sans ancun terme absolument connu.
>
> "Imaginons que l'on augmente le nombre de ces équations, de l'une d'entr'elles; alors il est clair que ce que nous appellons la dernière ligne, sera non seulement l'équation de condition nécessaire pour que ce nombre $n+1$ d'équations ait lieu; mais encore que cette équation de condition aura lieu; en sorte qu'elle sera une fonction des coëfficiens de ces équations, laquelle sera zéro par elle-même.
>
> "Voilà donc un moyen très-simple pour trouver un nombre $n+1$* de fonctions d'un nombre $n+1$ de quantités, lesquelles fonctions soient zéro par elles-mêmes."

For example, the pair of equations

$$\left.\begin{array}{r} ax+by+cz=0 \\ a'x+b'y+c'z=0 \end{array}\right\}$$

is taken, the first equation is repeated, and for this set of three equations the *équation de condition* is found to be

$$(ab'-a'b)c-(ac'-a'c)b+(bc'-b'c)a=0\,.$$

> "Or il est clair que la troisième équation n'exprimant rien de différent de la première, cette dernière quantité doit être zéro par elle-même: donc si on a ces deux suites de quantités

* Should be n.

$$\begin{array}{ccc} a, & b, & c \\ a', & b', & c' \end{array}$$

on peut être assuré qu'on aura toujours

$$(ab' - a'b)c - (ac' - a'c)b + (bc' - b'c)a = 0 .$$

"Et si au lieu de joindre la première équation, c'eût été la seconde, nous aurions trouvé de même

$$(ab' - a'b)c' - (ac' - a'c)b' + (bc' - b'c)a' = 0."$$

Similarly in regard to the quantities

$$\begin{array}{cccc} a, & b, & c, & d \\ a', & b', & c', & d' \\ a'', & b'', & c'', & d'' \end{array}$$

the identity

$$\begin{aligned} &[(ab' - a'b)\, c'' - (ac' - a'c)\, b'' + (bc' - b'c)\, a'']d \\ -&[(ab' - a'b)d'' - (ad' - a'd)b'' + (bd' - b'd)a'']c \\ +&[(ac' - a'c)\, d'' - (ad' - a'd)\, c'' + (cd' - c'd)\, a'']b \\ -&[(bc' - b'c)\, d'' - (bd' - b'd)\, c'' + (cd' - c'd)\, b'']a = 0 \end{aligned}$$

and two others are established, the general theorem of course being merely referred to as easily obtainable.

Thus far there is in substance nothing new. What we have obtained is simply a different aspect of Vandermonde's theorem, that *when two indices of either set are alike the function vanishes*, or, as we should now say, *a determinant with two rows identical is equal to zero.* Indeed the identities are used by Vandermonde in Bézout's form when solving a set of simultaneous equations. But what follows is important.

By taking two of these identities

$$\begin{aligned} (ab' - a'b)c - (ac' - a'c)b + (bc' - b'c)a &= 0 \\ (ab' - a'b)c' - (ac' - a'c)b' + (bc' - b'c)a' &= 0 , \end{aligned}$$

multiplying both sides of the first by d', both sides of the second by d, and subtracting, there is obtained in regard to the quantities

$$\begin{array}{cccc} a, & b, & c, & d \\ a', & b', & c', & d' \end{array}$$

the identity

$$(ab' - a'b)(cd' - c'd) - (ac' - a'c)(bd' - b'd) + (bc' - b'c)(ad' - a'd) = 0 .$$

Similarly by taking the three next identities before obtained, which for shortness we may write in modern notation,

$$|ab'c''|d \; - |ab'd''|c \; + |ac'd''|b \; - |bc'd''|a = 0\,,$$
$$|ab'c''|d' - |ab'd''|c' + |ac'd''|b' - |bc'd''|a' = 0\,,$$
$$|ab'c''|d'' - |ab'd''|c'' + |ac'd''|b'' - |bc'd''|a'' = 0\,,$$

there is deduced in regard to the quantities

$$\begin{matrix} a, & b, & c, & d, & e \\ a', & b', & c', & d', & e' \\ a'', & b'', & c'', & d'', & e'' \end{matrix}$$

the identities

$$|ab'c''|.|de'| - |ab'd''|.|ce'| + |ac'd''|.|be'| - |bc'd''|.|ae'| = 0\,,$$
$$|ab'c''|.|de''| - |ab'd''|.|ce''| + |ac'd''|.|be''| - |bc'd''|.|ae''| = 0\,,$$
$$|ab'c''|.|d'e''| - |ab'd''|.|c'e''| + |ac'd''|.|b'e''| - |bc'd''|.|a'e''| = 0\,.$$

Finally these last three identities are taken, both sides of the first multiplied by f'', both sides of the second by $-f'$, both sides of the third by f, and then by addition there is obtained in regard to the quantities

$$\begin{matrix} a, & b, & c, & d, & e, & f \\ a', & b', & c', & d', & e', & f' \\ a'', & b'', & c'', & d'', & e'', & f'' \end{matrix}$$

the identity

$$|ab'c''|.|de'f''| - |ab'd''|.|ce'f''| + |ac'd''|.|be'f''| - |bc'd''|.|ae'f''| = 0\,.$$

The subject of what may appropriately be called *vanishing aggregates of determinant-products* is not pursued farther, the concluding paragraph being

> "(223) En voilà assez pour faire connoître la route qu'on doit tenir, pour trouver ces sortes des théorêmes. On voit qu'il y a une infinité d'autres combinaisons à faire, et qui donneront chacune de nouvelles fonctions, qui seront zéro par elles-mêmes: mais cela est facile à trouver actuellement."*

* It is very curious to observe, in passing, that although Bézout does not obtain all his vanishing aggregates directly by means of the principle which he so carefully states at the commencement, nevertheless every one of them can be so obtained. He does not extend the principle beyond the case where only *one* of the original equations is repeated. If, however, we take the equations

$$ax + by + cz + dw = 0\,,$$
$$a'x + b'y + c'z + d'w = 0\,,$$

Our second list of Bézout's contributions thus is:—

(1) An unexplained artificial process for finding the numerators and denominators of fractions which express the values of the unknowns in a set of linear equations, or for finding the resultant of the elimination of n quantities from $n+1$ linear equations,—a process especially useful when the coefficients have particular values. (II. 3 + III. 4 + IV. 2.)

(2) An improved mode of finding Laplace's expansions, especially (but not exclusively) useful when the coefficients have particular values. (XIV. 3.)

(3) A proof of Vandermonde's theorem regarding the effect of the equality of two indices belonging to the same set. (XII. 3.)

(4) A series of identities regarding vanishing aggregates of products. (XXIII.)

HINDENBURG, C. F. (1784).

[*Specimen analyticum de lineis curvis secundi ordinis, in delucidationem Analyseos Finitorum Kaestnerianæ. Auctore Christiano Friderico Rüdigero. Cum praefatione Caroli Friderici Hindenburgii, professoris Lipsiensis.* (xlviii + 74 pp.) pp. xiv–xlviii. *Lipsiæ.*]*

One of the problems dealt with by Rüdiger being the finding of the equation of the conic passing through five given points ("*coefficientium determinatio Traiectoriae secundi ordinis per data quinque puncta*"), Hindenburg, in his preface, takes occasion to show how the generalised problem for $\frac{1}{2}n(n+3)$ points has been treated, pointing out that it is, of course, immediately dependent on the solution of a set of simultaneous linear equations. He directs attention to the labours of Cramer and Bézout, specially lauding the method of the latter, given in the treatise of 1779. Then he

repeat *both* of them so as to have a set of four, and then proceed by the *méthode pour abrêger* to find the *équation de condition*, we obtain

$$|ab'|.|cd'| - |ac'|.|bd'| + |ad'|.|bc'| + |bc'|.|ad'| - |bd'|.|ac'| + |cd'|.|ab'| = 0,$$

$$i.e. \quad 2\{|ab'|.|cd'| - |ac'|.|bd'| + |ad'|.|bc'|\} = 0.$$

This is the identity at foot of p. 457, and all the others are readily seen to be obtainable in the same way.

* My best thanks are due the Committee of Management of University College, London, for the loan of a copy of Hindenburg's tract from the Graves Library.

says—"*Haec de Opere Bezoldino in universam, quod plurimis adhuc Lectoribus nostris ignotum erit, dicta sufficiant. Nunc Regulam ipsam proponam.*". . . . The seventeen pages which follow, contain a tolerably close Latin translation of the *Règle générale pour calculer* . . . . , and the *Méthode pour trouver* . . . , pp. 172–187, §§ 198–223, which have been expounded above. Cramer's rule is next given, the second mode of putting it being in words, and the first as follows:—

> "Sint plures Incognitæ z, y, x, w, &c. totidemque Aequationes simplices indeterminatæ
>
> $$\begin{array}{l} A^1 = Z^1z + Y^1y + X^1x + W^1w + \&c. \\ A^2 = Z^2z + Y^2y + X^2x + W^2w + \&c. \\ A^3 = Z^3z + Y^3y + X^3x + W^3w + \&c. \\ A^4 = Z^4z + Y^4y + X^4x + W^4w + \&c. \\ \&c. \quad \&c. \quad \&c. \quad \&c. \quad \&c. \quad \&c. \end{array}$$
>
> Erit, ..., positis terminorum signis, ut praecipitur in fine Tabulæ, pag. seq.
>
> $$z = \frac{\overset{\text{A Y X W V U T . . . . .}}{\text{Permut }(1,\ 2,\ 3,\ 4,\ 5,\ 6,\ 7, \ldots\ldots)}}{\underset{\text{Z Y X W V U T . . . . .}}{\text{Permut }(1,\ 2,\ 3,\ 4,\ 5,\ 6,\ 7, \ldots\ldots)}} \qquad \text{(VII. 3.)}$$"

The similar expressions for y, x, w, v, u, t, are given, and then the "*regula signorum.*" After an illustrative example, the question of the *sequence* of the signs is taken up.

> "Quod si itaque $+sg(1, 2, 3, \ldots, n)$ denotet signorum vicissitudines, quibus hic afficiuntur Permutationum a numeris $1, 2, 3, \ldots n$ singulæ species, et $-sg(1, 2, 3, \ldots n)$ signa *contraria* vel *opposita:* appatet fore

$$\begin{array}{lllll} sg(1, 2) & = +sg(1) & -sg(1) & & \\ sg(1, 2, 3) & = +sg(1, 2) & -sg(1, 2) & +sg(1, 2) & \\ sg(1, 2, 3, 4) & = +sg(1, 2, 3) & -sg(1, 2, 3) & +sg(1, 2, 3) & -sg(1, 2, 3) \\ \ldots & & & & \end{array}$$

$$\begin{array}{llll} \text{unde, quia} & sg(1) & \text{est} & +\text{, facile eruitur} \\ & sg(1, 2) & \text{esse} & + - \\ & sg(1, 2, 3) & \ldots\ldots & + - - + + - \\ & sg(1, 2, 3, 4) & \ldots\ldots & + - - + + - - + + - - + \\ & & & + - - + + - - + + - - + \\ & \ldots & & \end{array}$$

and it is pointed out that the first sign is always $+$, and the last $+$ or $-$ according as the number $1+2+3+\ .\ .\ .+(n-1)$ is even or odd.

Bearing in mind that Hindenburg wrote his permutations in a definite order, this remark regarding the sequence of signs entitles us to view him as the author of a combined rule of term-formation and rule of signs, which may be formulated as follows:—

Write the permutations of 1, 2, 3, . . ., n *in ascending order of magnitude as if they were numbers; make the first sign* $+$, *the second* $-$, *the next pair contrary in sign to the first pair, the third pair contrary in sign to the second pair, the next six* (1.2.3) *contrary in sign to the first six, the third six contrary in sign to the second six, the fourth six contrary in sign to the third six, the next twenty-four* (1.2.3.4) *contrary in sign to the first twenty-four, and so on.* (II. 4 + III. 5.)

ROTHE, H. A. (1800).

[Ueber Permutationen, in Beziehung auf die Stellen ihrer Elemente. Anwendung der daraus abgeleiteten Sätze auf das Eliminations-problem. *Sammlung combinatorisch-analytischer Abhandlungen, herausg. v. C. F. Hindenburg*, ii. pp. 263–305.]

Rothe was a follower of Hindenburg, knew Hindenburg's preface to Rüdiger's Specimen Analyticum, and was familiar with what had been done by Cramer and Bézout (see his words at p. 305). His memoir is very explicit and formal, proposition following definition, and corollary following proposition, in the most methodical manner.

The idea which is made the basis of it, that of *place-index* ("Stellenexponent"), is an ill-advised and purposeless modification of Cramer's idea of a "dérangement." The definition is as follows: —In any permutation of the first n integers, the *place-index* of any integer is got *by counting the integer itself, and all the elements after it which are less than it.* For example, in the permutation

$$6,\ 4,\ 3,\ 9,\ 8,\ 10,\ 1,\ 7,\ 2,\ 5$$

of the first ten integers, the place-index of 9 is 6, and that of 7 is 3. The counting of the integer itself makes the place-index always *one more* than the number of "dérangements" connected with the

integer. This necessitates the introduction of a corresponding modification of Cramer's "rule of signs," viz.

> "3. Willkührlicher Satz. Jede Permutation der Elemente 1, 2, 3, . . . , r, werde mit dem Zeichen + versehen, wenn entweder gar keine, oder eine gerade Menge gerader Zahlen, unter ihren Stellenexponenten vorkommt; mit dem Zeichen − hingegen, wenn die Menge der geraden Zahlen, unter den Stellenexponenten ungerade ist." (III. 6.)

It is difficult to suggest any justification for the changes here introduced. The author himself refers to none. Indeed, in the very next paragraph he points out that to ascertain whether there be an even number of even integers among the place-indices is the same as to diminish each of the place-indices by 1, and ascertain whether there be an even number of odd integers, that is, whether the *sum* of the odd integers be even. He then concludes—

> "Man kann also auch die Regel so ausdrücken: Jede Permutation bekommt das Zeichen + wenn die Summe der um 1 verminderten Stellenexponenten gerade, − hingegen, wenn sie ungerade ist."

This is simply Cramer's rule, and it is the only rule of signs employed henceforward in the memoir, the expression "die Summe der um 1 verminderten Stellenexponenten," occurring over and over again as a periphrasis for "the number of dérangements."

The next four pages are occupied with a very lengthy but thorough investigation of the theorem that *two permutations differ in sign, if they be so related that either is got from the other by the interchange of two of the elements of the latter.* Strictly speaking, however, the proposition proved is something more definite than this, viz.—

If in a permutation of the integers 1.2, . . . r *there be* d *integers intermediate in place and value between any two,* A *and* B, *of the integers, the interchanging of the said two would increase or diminish the number of inversions of order by* $2d+1$. (III. 7.)

The proof consists in finding the sum of the place-indices for the given permutation in terms of d as just defined, c the number of elements less than both A and B and situated between them, f the number of such elements situated to the right of B, and e the

number of elements between A and B in value and situated to the right of B; then finding in like manner the sum of the place-indices for the new permutation; and finally comparing the two sums. The concluding sentence is as follows:—

> "Denn da . . . . , so ist die Summe der Stellenexponenten der zweyten Permutation um $d+e+1-e+d$ oder um $2d+1$ grösser, als bey der ersten Permutation; folglich gilt das auch bey der Summe der um 1 verminderten Stellenexponenten, da bey beyden Permutationen r einerley ist. Also ist die eine Summe gerade, die andere ungerade, folglich haben nach (4) beyde Permutationen verschiedene Zeichen."

As immediate deductions from this, it is pointed out that

The sign of any one permutation may be determined when the sign of any other is known, by counting the number of interchanges necessary to transform the one permutation into the other; (III. 8.)
and that

If one element of a permutation be made to take up a new place, by being, as it were, passed over m *other elements, the sign of the new permutation is the same as, or different from, that of the original according as* m *is even or odd.* (III. 9.)

A third corollary is given, but it is, strictly speaking, a self-evident corollary to the second corollary, and is quite unimportant.

Rothe's next theorem is—

The permutations of 1, 2, 3, ... , n *being arranged after the manner in which numbers are arranged in ascending order of magnitude, any two consecutive permutations will have the same sign, if the first place in which they differ be the* $(4n+3)^{th}$ *or* $(4n+4)^{th}$ *from the end, and will be of opposite sign if the said place be the* $(4n+1)^{th}$ *or* $(4n+2)^{th}$ *from the end.* (III. 10.)

Thus if the permutations of 1, 2, 3, ... , 10 be taken, and arranged as specified, two which will occur consecutively are

$$8,\ 4,\ 9,\ 3,\ 10,\ 7,\ 6,\ 5,\ 2,\ \ 1$$
$$8,\ 4,\ 9,\ 5,\ \ 1,\ 2,\ 3,\ 6,\ 7,\ 10;$$

and as the first place in which these differ is the 7^{th} from the end, it is affirmed that the signs preceding them must be alike. The

mode of proving the theorem will be readily understood by seeing it applied to this illustrative example. Taking the permutation

$$8, 4, 9, 3, 10, 7, 6, 5, 2, 1\,,$$

and interchanging 3 and 5 we have the permutation

$$8, 4, 9, 5, 10, 7, 6, 3, 2, 1\,,$$

and thence by cyclical changes the permutation

$$8, 4, 9, 5, 1, 2, 3, 6, 7, 10\,,$$

the number of alterations of sign thus being

$$1 + (5 + 4 + 3 + 2 + 1)$$

$$\textit{i.e.} \quad 1 + \tfrac{1}{2}(5 \times 6)\,,$$

—an even number.

Annexed to the theorem is the following corollary, which is not essentially sufficient from Hindenburg's proposition regarding the sequence of signs,—

If the permutations of $1, 2, 3, \ldots, n-1$ *be arranged after the manner in which numbers are arranged in ascending order of magnitude, and also in like manner the permutations of* $1, 2, 3, \ldots, n-1, n$, *then those permutations of the latter arranged set which begin with* r, *say, have in order the same signs as the permutations of the former arranged set, or different signs, according as* r *is odd or even.* (III. 11.)

For example, arranging the permutations of 1, 2, 3, each with its proper sign in front, we have

$$\begin{array}{l} +1,\ 2,\ 3 \\ -1,\ 3,\ 2 \\ -2,\ 1,\ 3 \\ +2,\ 3,\ 1 \\ +3,\ 1,\ 2 \\ -3,\ 2,\ 1\,; \end{array} \qquad \text{(A)}$$

then arranging those permutations of 1, 2, 3, 4 which begin with 3 say, each with its proper sign, we have

$$\begin{array}{l} +3,\ 1,\ 2,\ 4 \\ -3,\ 1,\ 4,\ 2 \\ -3,\ 2,\ 1,\ 4 \\ +3,\ 2,\ 4,\ 1 \\ +3,\ 4,\ 1,\ 2 \\ -3,\ 4,\ 2,\ 1\,; \end{array} \qquad \text{(B)}$$

and the two series of signs are seen to be identical, 3 being an odd number. Viewing this quite independently of the theorem to which it is annexed, it is evident that a change of sign at any point in the series (A) implies a change at the corresponding point in the other series, and consequently attention need only be paid to the first sign of (B) as compared with the first sign of (A). Now the first sign of (A) must necessarily be always plus, there being no inversions; and the first sign of (B) depends on the changes necessary for the transformation of the natural order 1, 2, 3, 4, into 3, 1, 2, 4. The truth of the corollary is thus apparent.

A second corollary is given, but it is of still less consequence, the difference between it and the first being that in the arranged set (B) the place whose occupant remains unchanged may be any one of the n places. (III. 12.)

The next few paragraphs concern the subject of "conjugate permutations" (*verwandte Permutationen*),—apparently a fresh conception. The definition is—

Two permutations of the numbers 1, 2, 3, . . . , n *are called* CONJUGATE *when each number and the number of the place which it occupies in the one permutation are interchanged in the case of the other permutation.* (XXIV.)

For example, the permutations

$$\begin{array}{ll} 3,\ 8,\ 5,\ 10,\ 9,\ 4,\ 6,\ 1,\ 7,\ 2 & \quad (A) \\ 8,\ 10,\ 1,\ 6,\ 3,\ 7,\ 9,\ 2,\ 5,\ 4 & \quad (B) \end{array}$$

are conjugate, because 3 is in the 1st place of (A) and 1 is in the 3rd place of (B), 8 is in the 2nd place of (A), and 2 is in the 8th place of B, and so on in every case.

The first theorem obtained is—

Conjugate permutations have the same sign. (III. 13.)

This is proved in a curious and interesting way, a special conjugate pair being considered, viz., the pair just given as an example. To commence with, a square divided into 10×10 equal squares is drawn, the vertical rows of small squares being numbered 1, 2, 3, &c. from left to right, and the horizontal rows 1, 2, 3, &c. from the top downwards. The permutation

$$3,\ 8,\ 5,\ 10,\ 9,\ 4,\ 6,\ 1,\ 7,\ 2$$

is then represented by putting a dot in each of the horizontal rows, in the first under 3, in the second under 8, and so on; so that if the rows be taken in order, and the number above each dot read, the given permutation is obtained. For the representation of the conjugate permutation nothing further is necessary: we obtain it at once if we only turn the paper round clockwise until the vertical rows are horizontal, and read off in order the numbers above the dots. In the next place the number of "dérangements" belonging to the permutation 3, 8, 5, ... is indicated by inserting a cross in every small square which is to the left of one dot and above another; thus the two crosses in the first horizontal row correspond to the two "dérangements" 32, 31; the six crosses in the second horizontal row to the six "dérangements" 85, 84, 86, 81, 87, 82; and so on. Then it is observed that if we turn the paper and try to indicate the "dérangements" of the conjugate permutation by inserting a cross in every small square which is to the right of one dot and above another, we obtain exactly the same crosses as before. The signs of the two permutations must thus be alike.

	1	2	3	4	5	6	7	8	9	10
1	×	×	·							
2	×	×		×	×	×	×	·		
3	×	×		×	·					
4	×	×		×		×	×		×	·
5	×	×		×		×	×		·	
6	×	×		·						
7	×	×				·				
8	·									
9		×					·			
10		·								

Immediately following this, the 24 permutations of 1, 2, 3, 4 are given in a column, each one having opposite it, in a parallel column, its conjugate permutation. The existence of *self-conjugate* permutations, *e.g.*, the permutation 3, 4, 1, 2 is thus brought to notice, and the substance of the following theorem in regard to them is given:—

If U_n *be the number of self-conjugate permutations of the first* n *integers, then*

$$U_n = U_{n-1} + (n-1)U_{n-2} \quad . \quad . \quad . \quad . \quad . \quad (\text{xxv.})$$

where $U_1 = 1$ *and* $U_2 = 2$.

This, however, is the only one of his results which Rothe does not attempt to prove.

In the second part of the memoir, which contains the application of the theorems of the first part to the solution of a set of linear

equations, there is not so much that is noteworthy. Methods previously known are followed, the new features being formality and rigour of demonstration.

The coefficients of the equations being

$$\begin{array}{l} 11,\ 12,\ 13,\ \ldots\ldots,\ 1r \\ 21,\ 22,\ 23,\ \ldots\ldots,\ 2r \\ \cdot\ \ \cdot\ \ \cdot\ \ \cdot\ \ \cdot\ \ \cdot\ \ \cdot\ \ \cdot \\ r1,\ r2,\ r3,\ \ldots\ldots,\ rr \end{array}$$

it is noted, as Vandermonde had remarked, that the common denominator of the values of the unknown may be got in two ways, viz., by permuting either all the second integers of the couples, 11, 22, 33, ... , rr, or all the first integers : but this is supplemented by a proof, that *if any term be taken, e.g.*,

$$16\,.\,24\,.\,33\,.\,47\,.\,51\,.\,68\,.\,79\,.\,82\,.\,95$$

with the couples so arranged that the first integers are in ascending order, and the sign be determined from the number of inversions in the series of second integers, then the sign obtained will be the same as would be got by arranging the couples so as to have the second integers in ascending order, and determining the sign from the inversions in the series of first integers. The proof rests entirely on the previous theorem, that conjugate permutations have the same sign ; indeed the new proposition is little else than another form of this theorem. (III. 14.)

The desirability of an appropriate notation for the cofactor, which any one of the coefficients has in the common denominator is recognised,* and the want supplied by prefixing f to the coefficient in question ; for example, the cofactor of 32 is denoted by

$$\mathfrak{f}\,32.$$

It is thus at once seen that the denominator itself is equal to

$$1n.\mathfrak{f}1n + 2n.\mathfrak{f}2n + \ldots\ldots + rn.\mathfrak{f}rn,$$

or

$$n1.\mathfrak{f}n1 + n2.\mathfrak{f}n2 + \ldots\ldots + nr.\mathfrak{f}nr. \qquad \text{(VI. 2.)}$$

Also by this means one of Bézout's (or Vandermonde's) general theorems becomes easily expressible in symbols, viz.,

$$1n.\mathfrak{f}1m + 2n.\mathfrak{f}2m + \ldots\ldots + rn.\mathfrak{f}rm = 0, \qquad \text{(XII. 4.)}$$

* Lagrange's use of a corresponding letter from a different alphabet must not be forgotten.

the proof of which is given as follows. In all the terms of $\mathfrak{f}1m$, every one of the integers except one occurs as the first integer of a couple, and every one of the integers except m occurs as the second integer of a couple: consequently, in every term of $1n \,.\, \mathfrak{f}1m$, the first places of the couples are occupied by the integers from 1 to r inclusive, while in the second places, m is still the only integer awanting, and n occurs twice. Suppose then all the terms of

$$1n.\mathfrak{f}1m \;+\; 2n.\mathfrak{f}2m \;+ \ldots\ldots +\; rn.\mathfrak{f}rm$$

so written, that the first integers of the couples are in ascending order of magnitude, and let us attend to a single term

$$\ldots\ldots\cdot pn\cdot\ldots\ldots\ldots\cdot qn\cdot\ldots\ldots\ldots$$

in which the two couples, having n for second integer, are the p^{th} and q^{th}. If we inquire from which of the expressions $1n.\mathfrak{f}1m$, $2n.\mathfrak{f}2m$, ... this term comes, we see that it is a term of both $pn.\mathfrak{f}pm$ and $qn.\mathfrak{f}qm$, and must, therefore, occur twice. Further, we see that in $pn.\mathfrak{f}qm$ it has the sign of the term

$$\ldots\ldots\cdot pm\cdot\ldots\ldots\ldots\cdot qn\cdot\ldots\ldots\ldots$$

of the common denominator, and that in $qn \,.\, \mathfrak{f}pm$, it has the sign of the term

$$\ldots\ldots\cdot pn\cdot\ldots\ldots\ldots\cdot qm\cdot\ldots\ldots\ldots$$

of the common denominator. But these two terms of the common denominator have different signs: consequently

$$1n.\mathfrak{f}1m + 2n.\mathfrak{f}2m + \ldots\ldots + rn.\mathfrak{f}rm$$

consists of pairs of equal terms with unlike signs, and thus vanishes identically. (XII. 4.)

These preparations having been attended to, the set of r equations with r unknowns is solved by Laplace's method; and a verification made after the manner of Vandermonde. It is also pointed out, that if the solution of a set of equations, say the four

$$\left.\begin{aligned} ax_1 + bx_2 + cx_3 + dx_4 &= s_1 \\ ex_1 + fx_2 + gx_3 + hx_4 &= s_2 \\ ix_1 + kx_2 + lx_3 + mx_4 &= s_3 \\ nx_1 + ox_2 + px_3 + qx_4 &= s_4 \end{aligned}\right\}$$

be

$$\left.\begin{array}{l} x_1 = \mathrm{A}s_1 + \mathrm{B}s_2 + \mathrm{C}s_3 + \mathrm{D}s_4 \\ x_2 = \mathrm{E}s_1 + \mathrm{F}s_2 + \mathrm{G}s_3 + \mathrm{H}s_4 \\ x_3 = \mathrm{I}s_1 + \mathrm{K}s_2 + \mathrm{L}s_3 + \mathrm{M}s_4 \\ x_4 = \mathrm{N}s_1 + \mathrm{O}s_2 + \mathrm{P}s_3 + \mathrm{Q}s_4 \end{array}\right\},$$

then the solution of the set

$$\left.\begin{array}{l} ay_1 + ey_2 + iy_3 + ny_4 = v_1 \\ by_1 + fy_2 + ky_3 + oy_4 = v_2 \\ cy_1 + gy_2 + ly_3 + py_4 = v_3 \\ dy_1 + hy_2 + my_3 + qy_4 = v_4 \end{array}\right\},$$

which has the same coefficients differently disposed, will be

$$\left.\begin{array}{l} y_1 = \mathrm{A}v_1 + \mathrm{E}v_2 + \mathrm{I}v_3 + \mathrm{N}v_4 \\ y_2 = \mathrm{B}v_1 + \mathrm{F}v_2 + \mathrm{K}v_3 + \mathrm{O}v_4 \\ y_3 = \mathrm{C}v_1 + \mathrm{G}v_2 + \mathrm{L}v_3 + \mathrm{P}v_4 \\ y_4 = \mathrm{D}v_1 + \mathrm{H}v_2 + \mathrm{M}v_3 + \mathrm{Q}v_4 \end{array}\right\}; \quad . \quad . \quad . \quad \text{(XXVI.)}$$

and hence, that the solution of a set having the special form

$$\left.\begin{array}{l} ax_1 + bx_2 + cx_3 + dx_4 = s_1 \\ bx_1 + ex_2 + fx_3 + gx_4 = s_2 \\ cx_1 + fx_2 + hx_3 + ix_4 = s_3 \\ dx_1 + gx_2 + ix_3 + jx_4 = s_4 \end{array}\right\}$$

will itself take the same form, viz.

$$\left.\begin{array}{l} \mathrm{A}s_1 + \mathrm{B}s_2 + \mathrm{C}s_3 + \mathrm{D}s_4 = x_1 \\ \mathrm{B}s_1 + \mathrm{E}s_2 + \mathrm{F}s_2 + \mathrm{G}s_4 = x_2 \\ \mathrm{C}s_1 + \mathrm{F}s_2 + \mathrm{H}s_3 + \mathrm{I}s_4 = x_3 \\ \mathrm{D}s_1 + \mathrm{G}s_2 + \mathrm{I}s_3 + \mathrm{J}s_4 = x_4 \end{array}\right\} \quad . \quad . \quad . \quad \text{(XXVI. 2.)}$$

GAUSS (1801).

[*Disquisitiones Arithmeticæ. Auctore D. Carolo Friderico Gauss.* 167 pp. Lips.]

The connection of Gauss with our theory was very similar to that of Lagrange, and doubtless was due to the fact that Lagrange had preceded him. The fifth chapter of his famous work, which is the only chapter we are concerned with, bears the title "*De formis æquationibusque indeterminatis secundi gradus,*" and its subject may be described in exactly the same words as Lagrange used in regard

to his memoir *Recherches d'Arithmétique* (1773: see above), viz. "les nombres qui peuvent être représentées par la formule $Bt^2 + Ctu + Du^2$."

Gauss writes his form of the second degree thus—

$$axx + 2bxy + cyy;$$

and for shortness speaks of it as the form (a, b, c). The function of the coefficients a, b, c, which was found by Lagrange to be of notable importance in the discussion of the form, Gauss calls the "*determinant* of the form," the exact words of his definition being

> "Numerum $bb - ac$, a cuius indole proprietates formæ (a, b, c) imprimis pendere in sequentibus docebimus, *determinantem* huius formæ uocabimus." (XV. 2.)

Here then we have the first use of the term which with an extended signification has in our day come to be so familiar. It must be carefully noted that the more general functions, to which the name came afterwards to be given, also repeatedly occur in the course of Gauss' work, *e.g.* the function $\alpha\delta - \beta\gamma$ in his statement of Lagrange's theorem (XXII.)

$$b'b' - a'c' = (bb - ac)(\alpha\delta - \beta\gamma)^2.$$

But such functions are not spoken of as belonging to the same category as $bb - ac$. In fact the new term introduced by Gauss was not "determinant" but "determinant of a form," being thus perfectly identical in meaning and usage with the modern term "discriminant."

Notwithstanding the title of the chapter Gauss did not confine himself to forms of two variables. A digression is made for the purpose of considering the ternary quadratic form ("formam ternariam secundi gradus"),

$$axx + a'x'x' + a''x''x'' + 2bx'x'' + 2b'xx'' + 2b''xx',$$

or as he shortly denotes it

$$\begin{pmatrix} a, & a', & a'' \\ b, & b', & b'' \end{pmatrix}.$$

In the matter of nomenclature the following paragraph of this digression is interesting

"Ponendo $bb - a'a'' = \mathrm{A}$, $b'b' - aa'' = \mathrm{A}'$, $b''b'' - aa' = \mathrm{A}''$,
$ab - b'b'' = \mathrm{B}$, $a'b' - bb'' = \mathrm{B}'$, $a''b'' - bb' = \mathrm{B}''$,

oritur alia forma

$$\begin{pmatrix} A & A' & A'' \\ B & B' & B'' \end{pmatrix} \quad \ldots . \text{F}$$

quam formæ

$$\begin{pmatrix} a & a' & a'' \\ b & b' & b'' \end{pmatrix} \quad \ldots . f$$

adjunctam dicemus. Hinc rursus inuenitur, denotando breuitatis caussa numerum

$$abb + a'b'b' + a''b''b'' - aa'a'' - 2bb'b'' \quad \text{per D,}$$

$$\text{BB} - \text{A}'\text{A}'' = a\text{D}, \quad \text{B}'\text{B}' - \text{AA}'' = a'\text{D}, \quad \text{B}''\text{B}'' - \text{AA}' = a''\text{D},$$
$$\text{AB} - \text{B}'\text{B}'' = b\text{D}, \quad \text{A}'\text{B}' - \text{BB}'' = b'\text{D}, \quad \text{A}''\text{B}'' - \text{BB}' = b''\text{D},$$

unde patet, formæ F adjunctam esse formam

$$\begin{pmatrix} a\text{D}, & a'\text{D}, & a''\text{D} \\ b\text{D}, & b'\text{D}, & b''\text{D} \end{pmatrix}.$$

Numerum D, a cuius indole proprietates formæ ternariæ f imprimis pendent, *determinantem* huius formæ uocabimus (xv. 2); hoc modo determinans formæ F sit = DD, sive æqualis quadrato determinantis formæ f, cui adjuncta est."

In this there is no advance so far as the theory of modern determinants is concerned, the identities given being those numbered (xx) and (xxi) under Lagrange. On the same page, however, an extension is given of Lagrange's theorem (xxii), regarding the determinant of the new form obtained by effecting a linear substitution on a given form. Gauss' words in regard to this are—

"Si forma aliqua ternaria f determinantis D, cuius indeterminatæ sunt x, x', x'' (puta prima $= x$, &c.) in formam ternariam g determinantis E, cuius indeterminatæ sunt y, y', y'', transmutatur per substitutionem talem

$$\begin{aligned} x &= \alpha y + \beta y' + \gamma y'', \\ x' &= \alpha' y + \beta' y' + \gamma' y'', \\ x'' &= \alpha'' y + \beta'' y' + \gamma'' y'', \end{aligned}$$

ubi nouem coefficientes α, β, &c. omnes supponuntur esse numeri integri, breuitatis caussa neglectis indeterminatis simpliciter dicemus, f transire in g per substitutionem (S)

$$\begin{matrix} \alpha, & \beta, & \gamma \\ \alpha', & \beta', & \gamma' \\ \alpha'', & \beta'', & \gamma'' \end{matrix}$$

atque f *implicare* ipsam g, siue *sub* f *contentam* esse. Ex tali itaque suppositione sponte sequuntur sex equationes pro sex coëfficientibus in g, quas apponere non erit necessarium: hinc autem per calculum facilem sequentes conclusiones euoluuntur:

"I. Designato breuitatis caussa numero

$$\alpha\beta'\gamma'' + \beta\gamma'\alpha'' + \gamma\alpha'\beta'' - \gamma\beta'\alpha'' - \alpha\gamma'\beta'' - \beta\alpha'\gamma''$$

per k inuenitur post debitas reductiones

$$E = kkD, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(XXII. 2.)}$$

. . . ."

When freed from its connection with ternary quadratic forms the theorem in determinants here involved is

$$\begin{aligned} \text{If } A_0 &= a_0\alpha_0^2 + a_1\alpha_1^2 + a_2\alpha_2^2 + 2b_0\alpha_1\alpha_2 + 2b_1\alpha_0\alpha_2 + 2b_2\alpha_0\alpha_1, \\ A_1 &= a_0\beta_0^2 + a_1\beta_1^2 + a_2\beta_2^2 + 2b_0\beta_1\beta_2 + 2b_1\beta_0\beta_2 + 2b_2\beta_0\beta_1, \\ A_2 &= a_0\gamma_0^2 + a_1\gamma_1^2 + a_2\gamma_2^2 + 2b_0\gamma_1\gamma_2 + 2b_1\gamma_0\gamma_2 + 2b_2\gamma_0\gamma_1, \\ B_0 &= a_0\beta_0\gamma_0 + a_1\beta_1\gamma_1 + a_2\beta_2\gamma_2 + b_0(\beta_1\gamma_2 + \beta_2\gamma_1) + b_1(\beta_0\gamma_2 + \beta_2\gamma_0) + b_2(\beta_0\gamma_1 + \beta_1\gamma_0) \\ B_1 &= a_0\alpha_0\gamma_0 + a_1\alpha_1\gamma_1 + a_2\alpha_2\gamma_2 + b_0(\alpha_1\gamma_2 + \alpha_2\gamma_1) + b_1(\alpha_0\gamma_2 + \alpha_2\gamma_0) + b_2(\alpha_0\gamma_1 + \alpha_1\gamma_0) \\ B_2 &= a_0\alpha_0\beta_0 + a_1\alpha_1\beta_1 + a_2\alpha_2\beta_2 + b_0(\alpha_1\beta_2 + \alpha_2\beta_1) + b_1(\alpha_0\beta_2 + \alpha_2\beta_0) + b_2(\alpha_0\beta_1 + \alpha_1\beta_0), \end{aligned}$$

then

$$\begin{aligned} &A_0B_0^2 + A_1B_1^2 + A_2B_2^2 - A_0A_1A_2 - 2B_0B_1B_2 \\ &= (a_0b_0^2 + a_1b_1^2 + a_2b_2^2 - a_0a_1a_2 - 2b_0b_1b_2) \\ &\qquad \times (\alpha_0\beta_1\gamma_2 + \beta_0\gamma_1\alpha_2 + \gamma_0\alpha_1\beta_2 - \gamma_0\beta_1\alpha_2 - \alpha_0\gamma_1\beta_2 - \beta_0\alpha_1\gamma_2)^2. \end{aligned}$$

As thus viewed it is an instance of the multiplication-theorem, the product of three determinants (in the modern sense) being expressed as a single determinant.

The multiplication-theorem is also not very distantly connected with the following other statement of Gauss :—

"Si forma ternaria f formam ternarium f' implicat atque haec formam f'': implicabit etiam f ipsam f''. Facillime enim perspicietur, si transeat

f in f' per substitutionem	f' in f'' per substitutionem
$\alpha,\ \beta,\ \gamma$	$\delta,\ \epsilon,\ \zeta$
$\alpha',\ \beta',\ \gamma'$	$\delta',\ \epsilon',\ \zeta'$
$\alpha'',\ \beta'',\ \gamma''$	$\delta'',\ \epsilon'',\ \zeta''$

f transmutatum iri per substitutionem

$$\begin{array}{lll} \alpha\delta + \beta\delta' + \gamma\delta'', & \alpha\epsilon + \beta\epsilon' + \gamma\epsilon'' & \alpha\zeta + \beta\zeta' + \gamma\zeta'' \\ \alpha'\delta + \beta'\delta' + \gamma'\delta'' & \alpha'\epsilon + \beta'\epsilon' + \gamma'\epsilon'' & \alpha'\zeta + \beta'\zeta' + \gamma'\zeta'' \\ \alpha''\delta + \beta''\delta' + \gamma''\delta'' & \alpha''\epsilon + \beta''\epsilon' + \gamma''\epsilon'' & \alpha''\zeta + \beta''\zeta' + \gamma''\zeta''." \end{array} \qquad \text{(XXII. 3.)}$$

MONGE (1809).

[Essai d'application de l'analyse a quelques parties de la géométrie élémentaire. *Journ. de l'Ec. Polyt.*, viii. pp. 107–109.]

Lagrange, as we have already seen, was led to certain identities regarding the expression

$$xy'z'' + yz'x'' + zx'y'' - xz'y'' - yx'z'' - zy'x''$$

in the course of investigations on the subject of triangular pyramids. The position of Monge is that of Lagrange reversed. From the theory of equations he derives identities connecting such expressions, and translates them into geometrical theorems.

The simpler of these identities, as being already chronicled, we pass over. At p. 107 he takes the three equations

$$\begin{aligned} a_1u + b_1x + c_1y + d_1z + e_1 &= 0 \\ a_2u + b_2x + c_2y + d_2z + e_2 &= 0 \\ a_3u + b_3x + c_3y + d_3z + e_3 &= 0\,, \end{aligned}$$

and eliminating every pair of the letters u, x, y, z, obtains the six equations

$$\begin{aligned} \beta u + \alpha x + \mathrm{P} &= 0 \qquad (1) \\ \gamma x + \beta y + \mathrm{Q} &= 0 \qquad (2) \\ \delta y + \gamma z + \mathrm{M} &= 0 \qquad (3) \\ \alpha z + \delta u + \mathrm{N} &= 0 \qquad (4) \\ \gamma u - \alpha y + \mathrm{S} &= 0 \qquad (5) \\ \beta z - \delta x + \mathrm{R} &= 0 \qquad (6)\,; \end{aligned}$$

the ten letters

$$\alpha,\ \beta,\ \gamma,\ \delta,\ \mathrm{M},\ \mathrm{N},\ \mathrm{P},\ \mathrm{Q},\ \mathrm{R},\ \mathrm{S}$$

being used to stand for the lengthy expressions which we nowadays denote by

$$|b_1c_2d_3|\,,\ |a_1c_2d_3|\,,\ |a_1b_2d_3|\,,\ |a_1b_2c_3|\,,$$
$$|a_1b_2e_3|\,,\ |b_1c_2e_3|\,,\ |c_1d_2e_3|\,,\ -|a_1d_2e_3|\,,\ |a_1c_2e_3|\,,\ |b_1d_2e_3|\,.$$

Then, taking triads of these six equations, *e.g.*, the triads (1), (2), (5), he derives the identities

$$\left.\begin{array}{r} \alpha Q + \beta S - \gamma P = 0 \\ \delta P + \alpha R - \beta N = 0 \\ -\gamma N + \delta S + \alpha M = 0 \\ -\beta M + \gamma R + \delta Q = 0 \end{array}\right\} ,$$

or

$$\left.\begin{array}{r} -|b_1c_2d_3|.|a_1d_2e_3| + |a_1c_2d_3|.|b_1d_2e_3| - |a_1b_2d_3|.|c_1d_2e_3| = 0 \\ |a_1b_2c_3|.|c_1d_2e_3| + |b_1c_2d_3|.|a_1c_2e_3| - |a_1c_2d_3|.|b_1c_2e_3| = 0 \\ -|a_1b_2d_3|.|b_1c_3e_3| + |a_1b_2c_3|.|b_1d_2e_3| + |b_1c_2d_3|.|a_1b_2e_3| = 0 \\ -|a_1c_2d_3|.|a_1b_2e_3| + |a_1b_2d_3|.|a_1c_2e_3| - |a_1b_2c_3|.|a_1d_2e_3| = 0 \end{array}\right\} \text{(XXIII. 2.)}$$

which in their turn, he says, by processes of elimination, may be the source of many others. For example, each of the four being linear and homogeneous in α, β, γ, δ, these letters may all be eliminated with the result

$$RS + QN - PM = 0 ,$$

or

$$|a_1c_2e_3|.|b_1d_2e_3| - |a_1d_2e_3|.|b_1c_2e_3| - |c_1d_2e_3|.|a_1b_2e_3| = 0 .$$

Also, eliminating P from the first and second, S from the first and third, Q from the first and fourth, and so on, we have

$$\begin{array}{r} -\beta\gamma N + \delta\alpha Q + \beta\delta S + \alpha\gamma R = 0 , \\ \alpha\beta M + \gamma\delta P - \beta\gamma N - \delta\alpha Q = 0 , \\ \alpha\beta M - \gamma\delta P + \beta\delta S - \alpha\gamma R = 0 , \\ \&c. \qquad \&c. \end{array}$$

i.e.

$$\left.\begin{array}{r} -|a_1c_2d_3|.|a_1b_2d_3|.|b_1c_2e_3| - |a_1b_2c_3|.|b_1c_2d_3|.|a_1d_2e_3| \\ +|a_1c_2d_3|.|a_1b_2c_3|.|b_1d_2e_3| + |b_1c_2d_3|.|a_1b_2d_3|.|a_1c_2e_3| \end{array}\right\} = 0 ,$$

$$\&c. \qquad \&c. \qquad \text{(XXVIII.)}$$

Monge does not pursue the subject further. His method, however, is seen to be quite general; and we can readily believe that he possessed numerous other identities of the same kind. This is borne out by a statement in Binet's important memoir of 1812. Binet, who was familiar with what had been done by Vandermonde, Laplace, and Gauss, says (p. 286) :—"M. Monge m'a communiqué, depuis la lecture de ce mémoire, d'autres théorèmes très-remarquables sus ces résultantes ; mais ils ne sont pas du genre de ceux que nous nous proposons de donner ici."

HIRSCH (1809).

[Sammlung von Aufgaben aus der Theorie der algebraischen Gleichungen, von Meier Hirsch. pp. 103–107. Berlin, 1809.]

The 4th Chapter *Von der Elimination u. s. w.*, contains five pages on the subject of the solution of simultaneous linear equations. These embrace nothing more noteworthy than a statement, without proof, of Cramer's rule, separated into three parts (iv., iii. 2, v.), and carefully worded.

BINET (May 1811).

[Mémoire sur la théorie des axes conjugués et des momens d'inertie des corps. *Journ. de l'École Polytechnique*, ix. (pp. 41–67), pp. 45, 46.]*

In this well-known memoir, in which the conception of the *moment of inertia of a body with respect to a plane* was first made known, there repeatedly occur expressions, which at the present day would appear in the notation of determinants. There is only one paragraph, however, containing anything new in regard to these functions. It stands as follows :—

"Le moment d'inertie minimum pris par rapport au plan (C), a pour valeur

$$\Sigma mk^2 = f^2 \times$$

$$\frac{\mathrm{ABC}-\mathrm{AF}^2-\mathrm{BE}^2-\mathrm{CD}^2+2\mathrm{DEF}}{g^2(\mathrm{BC}-\mathrm{F}^2)+h^2(\mathrm{AC}-\mathrm{E}^2)+i^2(\mathrm{AB}-\mathrm{D}^2)+2gh(\mathrm{EF}-\mathrm{CD})+2gi(\mathrm{DF}-\mathrm{BE})+2hi(\mathrm{DE}-\mathrm{AF})}.$$

Si, dans le numérateur,

$$\mathrm{ABC}-\mathrm{AF}^2-\mathrm{BE}^2-\mathrm{CD}^2+2\mathrm{DEF}$$

on remplace A, B, C, &c. par Σmx^2, Σmy^2, &c. que ces lettres représentent, on a

$$\begin{aligned}&\Sigma mx^2\Sigma my^2\Sigma mz^2 - \Sigma mx^2(\Sigma myz)^2 - \Sigma my^2(\Sigma mxz)^2\\ &- \Sigma mz^2(\Sigma mxy)^2 + 2\Sigma mxy\Sigma mxz\Sigma myz,\end{aligned}$$

et l'on peut s'assurer que cette expression est identique à

$$\Sigma mm'm''(xy'z''+yz'x''+zx'y''-xz'y''-yx'z''-zy'x'')^2;$$

par une transformation analogue, on peut ramener la quantité

* An abstract of this is given in the *Nouv. Bull. des Sciences par la Société Philomatique*, ii. pp. 312–316.

$$g^2(BC-F^2) + h^2(AC-E^2) + i^2(AB-D^2) \\ + 2gh(EF-CD) + 2gi(DF-BE) + 2hi(DE-AF),$$

à celle-ci

$$\Sigma mm'[g(yz'-zy') + h(zx'-xz') + i(xy'-yx')]^2."$$

Now the numerator referred to would at the present day be written.

$$\begin{vmatrix} A & D & E \\ D & B & F \\ E & F & C \end{vmatrix},$$

and since Σmx^2, &c. stand for $mx^2 + m_1x_1{}^2 + m_2x_2{}^2 + \ldots$, &c., the first identity given may be put in the form

$$\begin{vmatrix} mx^2 + m_1x_1{}^2 + m_2x_2{}^2 + \ldots & mxy + m_1x_1y_1 + m_2x_2y_2 + \ldots & mxz + m_1x_1z_1 + m_2x_2z_2 + \ldots \\ mxy + m_1x_1y_1 + m_2x_2y_2 + \ldots & my^2 + m_1y_1{}^2 + m_2y_2{}^2 + \ldots & myz + m_1y_1z_1 + m_2y_2z_2 + \ldots \\ mxz + m_1x_1z_1 + m_2x_2z_2 + \ldots & myz + m_1y_1z_1 + m_2y_2z_2 + \ldots & mz^2 + m_1z_1{}^2 + m_2z_2{}^2 + \ldots \end{vmatrix}$$

$$= mm_1m_2 \begin{vmatrix} x & x_1 & x_2 \\ y & y_1 & y_2 \\ z & z_1 & z_2 \end{vmatrix}^2 + mm_1m_3 \begin{vmatrix} x & x_1 & x_3 \\ y & y_1 & y_3 \\ z & z_1 & z_3 \end{vmatrix}^2 + \ldots \quad \text{(XVIII. 2.)}$$

where $x_1, y_2, \ldots$ are for convenience written instead of $x', y'', \ldots$ It will be seen that this is an important extension of a theorem of Lagrange, the latter theorem being the very special case of the present obtained by putting $m = m_1 = m_2 = 1$, and $m_3 = m_4 = \ldots = 0$, —a fact which is brought still more clearly into evidence if, instead of the left-hand member of the identity, we write the modern contraction for it, viz.

$$\begin{vmatrix} mx & m_1x_1 & m_2x_2 & m_3x_3 & \ldots \\ my & m_1y_1 & m_2y_2 & m_3y_3 & \ldots \\ mz & m_1z_1 & m_2z_2 & m_3z_3 & \ldots \end{vmatrix} \times \begin{vmatrix} x & x_1 & x_2 & x_3 & \ldots \\ y & y_1 & y_2 & y_3 & \ldots \\ z & z_1 & z_2 & z_3 & \ldots \end{vmatrix}.$$

Again the denominator

$$g^2(BC-F^2) + h^2(AC-E^2) + i^2(AB-D^2) \\ + 2gh(EF-CD) + 2gi(DF-BE) + 2hi(DE-AF)$$

being in modern notation

$$\begin{vmatrix} \cdot & g & h & i \\ g & A & D & E \\ h & D & B & F \\ i & E & F & C \end{vmatrix},$$

the second identity may be written

$$
\begin{vmatrix}
\cdot & g & h & i \\
g & mx^2+m_1x_1^2+\ldots & mxy+m_1x_1y_1+\ldots & mxz+m_1x_1z_1+\ldots \\
h & mxy+m_1x_1y_1+\ldots & my^2+m_1y_1^2+\ldots & myz+m_1y_1z+\ldots \\
i & mxz+m_1x_1z_1+\ldots & myz+m_1y_1z_1+\ldots & mz^2+m_1z_1^2+\ldots
\end{vmatrix}
$$

$$
= mm_1\begin{vmatrix} g & x & x_1 \\ h & y & y_1 \\ i & z & z_1 \end{vmatrix}^2 + mm_2\begin{vmatrix} g & x & x_2 \\ h & y & y_2 \\ i & z & z_2 \end{vmatrix}^2 + m_1m_2\begin{vmatrix} g & x_1 & x_2 \\ h & y_1 & y_2 \\ i & z_1 & z_2 \end{vmatrix} + \ldots \quad \text{(XXIX.)}
$$

This also is an important theorem, and is not so much an extension of previous work as a breaking of fresh ground.

BINET (November 1811).

[Sur quelques formules d'algèbre, et sur leur application à des expressions qui ont rapport aux axes conjugués des corps. *Nouv. Bull. des Sciences par la Société Philomatique*, ii. pp. 389–392.]

In this paper Binet returns to the consideration of the first of the two identities which have just been referred to, writing it now in the form

$$
\begin{aligned}
&\Sigma(xy'z''-xz'y''+yz'x''-yx'z''+zx'y''-zy'x'')^2 \\
&= \Sigma x^2\Sigma y^2\Sigma z^2 - \Sigma x^2(\Sigma yz)^2 - \Sigma y^2(\Sigma xz)^2 - \Sigma z^2(\Sigma xy)^2 + 2\Sigma xy\Sigma xz\Sigma yz\,.
\end{aligned}
$$

He puts it in the same category as the identity

$$\Sigma(y'z-zy')^2 = \Sigma y^2\Sigma z^2 - (\Sigma yz)^2\,,$$

which he speaks of as being then known. Further, he says

"Ces deux formules sont du même genre que la suivante

$$
\left\{\begin{matrix}
ux'y''z'''-ux'z''y'''+uy'z''x'''-uy'x''z'''+uz'x''y'''-uz'y''x'''+xy'u''z'''-xy'z''u''' \\
+xz'y''u'''-xz'u''y'''+xu'z''y'''-xu'y''z'''+yz'u''x'''-yz'x''u'''+yu'x''z'''-yu'z''x''' \\
+yx'z''u'''-yx'u''z'''+zu'y''x'''-zu'x''y'''+zx'y''u'''-zx'u''y'''+zy'x''u'''-zy'u''x'''
\end{matrix}\right\}^2
$$

$$
\begin{aligned}
&= \Sigma u^2\Sigma x^2\Sigma y^2\Sigma z^2 - \Sigma u^2\Sigma x^2(\Sigma yz)^2 - \Sigma u^2\Sigma y^2(\Sigma xz)^2 - \Sigma u^2\Sigma z^2(\Sigma xy)^2 \\
&\quad - \Sigma x^2\Sigma y^2(\Sigma uz)^2 - \Sigma x^2\Sigma z^2(\Sigma uy)^2 - \Sigma y^2\Sigma z^2(\Sigma ux^2) \\
&\quad + 2\Sigma u^2\Sigma xy\Sigma xz\Sigma yz + 2\Sigma x^2\Sigma uy\Sigma uz\Sigma yz + 2\Sigma y^2\Sigma ux\Sigma uz\Sigma xz \\
&\quad + 2\Sigma z^2\Sigma ux\Sigma uy\Sigma xy + (\Sigma ux)^2(\Sigma yz)^2 + (\Sigma uy)^2(\Sigma xz)^2 + (\Sigma uz)^2(\Sigma xy)^2 \\
&\quad - 2\Sigma ux\Sigma xy\Sigma yz\Sigma zu - 2\Sigma uy\Sigma yz\Sigma zx\Sigma xu - 2\Sigma uy\Sigma yx\Sigma xz\Sigma zu\,,
\end{aligned}
$$

—a result which in modern notation would take the form

$$\begin{vmatrix} u & u_1 & u_2 & u_3 \\ x & x_1 & x_2 & x_3 \\ y & y_1 & y_2 & y_3 \\ z & z_1 & z_2 & z_3 \end{vmatrix}^2 + \begin{vmatrix} u & u_1 & u_2 & u_4 \\ x & x_1 & x_2 & x_4 \\ y & y_1 & y_2 & y_4 \\ z & z_1 & z_2 & z_4 \end{vmatrix}^2 + \dots\dots$$

$$= \begin{vmatrix} u^2+u_1^2+\dots & ux+u_1x_1+\dots & uy+u_1y_1+\dots & uz+u_1z_1+\dots \\ ux+u_1x_1+\dots & x^2+x_1^2+\dots & xy+x_1y_1+\dots & xz+x_1z_1+\dots \\ uy+u_1y_1+\dots & xy+x_1y_1+\dots & y^2+y_1^2+\dots & yz+y_1z_1+\dots \\ uz+u_1z_1+\dots & xz+x_1z_1+\dots & yz+y_1z_1+\dots & z^2+z_1^2+\dots \end{vmatrix} . \qquad \text{(XVIII. 3.)}$$

It is thus clear that, in November 1811, Binet was well on the way towards a great generalisation. He even says that the three identities may be looked upon

> "comme les trois premières d'une suite de formules construites d'après une même loi facile à saisir."

He merely indicates, however, the mode of proof he would adopt for the results obtained, and refers to possible applications of them in investigations regarding the Method of Least Squares (Laplace, *Connaissance des Tems*, 1813) and the Centre of Gravity (Lagrange, *Mém. de Berlin*, 1783). The mode of proof need not be given here, as it turns up again in the far more important memoir in which the theorem in all its generality falls to be considered.

DE PRASSE (1811).

[Commentationes Mathematicæ. Auctore Mauricio de Prasse. 120 pp. Lips., 1804, 1812. Pp. 89–102; Commentatio vii.*: Demonstratio eliminationis Cramerianæ.]

Of previous writings the one which De Prasse's most resembles is Rothe's. There is less of it, and it shows less freshness; but there is the same stiff formality of arrangement, and the same effort at rigour of demonstration.

* Separate copies of the *Demonstratio eliminationis Cramerianæ* are also to be found, bearing the invitation title-page:

> *Ad memoriam Kregelio-Sternbachianam in auditorio philosophorum die* xviii *Julii* MDCCCXI. *h.* ix *celebrandam invitant ordinum Academiæ Lips. Decani seniores cæterique adsessores . . . . Demonstratio eliminationis Cramerianæ.*

It is these copies which fix the date. See *Nature*, xxxvii. pp. 246, 247.

The definition of a permutation (*variatio*) being given, the first problem (which, however, is called a theorem) is propounded, viz., to tabulate the permutations of α, β, γ, δ, . . . (" *Variationum ex elementis* α, β, γ, . . . *constructarum et in Classes combinatorias digestarum Tabulam parare*"). The result is

α	β	γ	δ
	$\alpha\beta$	$\alpha\gamma$	$\alpha\delta$
	$\beta\alpha$	$\beta\gamma$	$\beta\delta$
	$\gamma\alpha$	$\gamma\beta$	$\gamma\delta$
	$\delta\alpha$	$\delta\beta$	$\delta\gamma$
		$\alpha\beta\gamma$	$\alpha\beta\delta$
		$\alpha\gamma\beta$	$\alpha\gamma\delta$
		$\alpha\delta\beta$	$\alpha\delta\gamma$
		$\beta\alpha\gamma$	$\beta\alpha\delta$
		$\beta\gamma\alpha$	$\beta\gamma\delta$
		$\beta\delta\alpha$	$\beta\delta\gamma$
		$\gamma\alpha\beta$	$\gamma\alpha\delta$
		$\gamma\beta\alpha$	$\gamma\beta\delta$
		$\gamma\delta\alpha$	$\gamma\delta\beta$
		$\delta\alpha\beta$	$\delta\alpha\gamma$
		$\delta\beta\alpha$	$\delta\beta\gamma$
		$\delta\gamma\alpha$	$\delta\gamma\beta$
			$\alpha\beta\gamma\delta$
			$\alpha\beta\delta\gamma$
			$\alpha\gamma\beta\delta$
			$\alpha\gamma\delta\beta$
			$\alpha\delta\beta\gamma$
			$\alpha\delta\gamma\beta$
			$\beta\alpha\gamma\delta$
			$\beta\alpha\delta\gamma$
			$\beta\gamma\alpha\delta$
			$\beta\gamma\delta\alpha$
			$\beta\delta\alpha\gamma$
			$\beta\delta\gamma\alpha$
			$\gamma\alpha\beta\delta$
			$\gamma\alpha\delta\beta$
			$\gamma\beta\alpha\delta$
			$\gamma\beta\delta\alpha$
			$\gamma\delta\alpha\beta$
			$\gamma\delta\beta\alpha$
			$\delta\alpha\beta\gamma$
			$\delta\alpha\gamma\beta$
			$\delta\beta\alpha\gamma$
			$\delta\beta\gamma\alpha$
			$\delta\gamma\alpha\beta$
			$\delta\gamma\beta\alpha$

The first row of the permutations involving two letters is got by taking the first letter of the previous row and annexing each of the others to it in succession and in the order of their occurrence; the second row is got in like manner from the second letter; and so on. Similarly the first row of permutations involving three letters is got from $\alpha\beta$ the first obtained permutation of two letters, the second row from $\alpha\gamma$ the next obtained permutation of two letters, and so on.*

The second problem (and on this occasion actually so designated) is somewhat quaint in its indefiniteness, viz., to prefix to each permutation the sign + or the sign −, so that the sum of all the permutations involving the same number of letters (>1) may vanish ("*Singulis Variationibus, omissis repetitionibus, signa + et − ita praefigere, ut summa secundæ et cujuslibet classis insequentis evanescat*"). There is no indefiniteness or multiplicity about the solution, which in substance is:—Make the permutations in every row of the preceding table alternately + and −, the first sign of all being +, and the first permutation of every other row having the same sign as the permutation from which it was derived. In this way the table becomes

$$\left.\begin{array}{cccc} +\alpha, & -\beta, & +\gamma, & -\delta \end{array}\right\}$$

$$\left.\begin{array}{ccc} +\alpha\beta, & -\alpha\gamma, & +\alpha\delta \\ -\beta\alpha, & +\beta\gamma, & -\beta\delta \\ +\gamma\alpha, & -\gamma\beta, & +\gamma\delta \\ -\delta\alpha, & +\delta\beta, & -\delta\gamma \end{array}\right\}$$

$$\left.\begin{array}{cc} +\alpha\beta\gamma, & -\alpha\beta\delta \\ -\alpha\gamma\beta, & +\alpha\gamma\delta \\ +\alpha\delta\beta, & -\alpha\delta\gamma \\ -\beta\alpha\gamma, & +\beta\alpha\delta \\ +\beta\gamma\alpha, & -\beta\gamma\delta \\ -\beta\delta\alpha, & +\beta\delta\gamma \\ +\gamma\alpha\beta, & -\gamma\alpha\delta \\ -\gamma\beta\alpha, & +\gamma\beta\delta \\ +\gamma\delta\alpha, & -\gamma\delta\beta \\ -\delta\alpha\beta, & +\delta\alpha\gamma \\ +\delta\beta\alpha, & -\delta\beta\gamma \\ -\delta\gamma\alpha, & +\delta\gamma\beta \end{array}\right\}$$

* It will be seen that the order in which the permutations come to hand in this process of tabulation is the order in which they would be arranged according to magnitude if each permutation were viewed as a number of which α, β, γ, δ were the digits, α being $<\beta<\gamma<\delta$ ("ordo lexicographicus," "lexicographische Anordnung" of Hindenburg).

$$\left.\begin{array}{l}
+\alpha\beta\gamma\delta \\
-\alpha\beta\delta\gamma \\
-\alpha\gamma\beta\delta \\
+\alpha\gamma\delta\beta \\
+\alpha\delta\beta\gamma \\
-\alpha\delta\gamma\beta \\[1ex]
-\beta\alpha\gamma\delta \\
+\beta\alpha\delta\gamma \\
+\beta\gamma\alpha\delta \\
-\beta\gamma\delta\alpha \\
-\beta\delta\alpha\gamma \\
+\beta\delta\gamma\alpha \\[1ex]
+\gamma\alpha\beta\delta \\
-\gamma\alpha\delta\beta \\
-\gamma\beta\alpha\delta \\
+\gamma\beta\delta\alpha \\
+\gamma\delta\alpha\beta \\
-\gamma\delta\beta\alpha \\[1ex]
-\delta\alpha\beta\gamma \\
+\delta\alpha\gamma\beta \\
+\delta\beta\alpha\gamma \\
-\delta\beta\gamma\alpha \\
-\delta\gamma\alpha\beta \\
+\delta\gamma\beta\alpha
\end{array}\right\}.$$

A proof by the method of mathematical induction (so-called) is given that with these signs the sum of all the permutations of any group vanishes.

Up to this point the essence of what has been furnished is a combined rule of term-formation and rule of signs. (II. 5 + III. 15.) In connection with it Bézout's rule of the year 1764 may be recalled.

The third problem is to determine the sign of any single permutation from consideration of the permutation itself. The solution is :—Under each letter of the given permutation put all the letters which precede it in the natural arrangement and which are not found to precede it in the given permutation ; and make the sum + or − according as the total number of such letters is even or odd.

"EXEMP. Datæ complexiones sint hæ :

$$\epsilon\gamma\delta\beta\,, \quad \delta\alpha\epsilon\gamma\,, \quad \epsilon\delta\gamma\alpha\,, \quad \delta\beta\epsilon\gamma\,.$$

Literæ secundum I subjiciantur

$\alpha\alpha\alpha\alpha$	$\alpha.\beta\beta$	$\alpha\alpha\alpha.$	$\alpha\alpha\alpha\alpha$
$\beta\beta\beta$	$\beta\gamma$	$\beta\beta\beta$	$\beta.\gamma$
γ	γ	$\gamma\gamma$	γ
δ		δ	

quarum numeri sunt

9 6 9 7

qui complexionibus datis præfigi jubent signa

$-$ $+$ $-$ $-$."

The proof that this rule of signs, which is manifestly nothing else than Cramer's, leads to the same results as the previous rule, is quite easily understood if a particular permutation be first considered. For example, let the sign of the particular permutation $\delta\beta\alpha\gamma$ be wanted. Following the first rule, we should require to note four different members, viz.,

(1)	the no.	of the column	in which $\delta\beta\alpha\gamma$	occurs	in the 4th	group,	
(2)	"	"	$\delta\beta\alpha$	"	3rd	"	
(3)	"	"	$\delta\beta$	"	2nd	"	
(4)	"	"	δ	"	1st	"	.

The first of these numbers being 1, we should infer that in fixing the sign of $\delta\beta\alpha\gamma$ in the fourth group there had been no change from the sign of $\delta\beta\alpha$ in the third group; the second number being also 1, we should make a like inference; the third number being 2, we should infer that in fixing the sign of $\delta\beta$ in the second group there had been 1 change from the sign of δ in the first group; and finally, the fourth number being 4, we should infer that in fixing the sign of δ in the first group there had been 3 changes from the sign of α in that group. The total number of changes from the sign of α in the first group being thus $3+1+0+0$, *i.e.*, 4, the sign would be made $+$. Now the 3 in this aggregate is simply the number of letters in the first group which precede δ, the 1 is simply the number of letters taken along with δ before β comes to be taken along with it to form $\delta\beta$ in the second group, and the two zeros correspond to the fact that $\delta\beta\alpha$ on the third group and $\delta\beta\alpha\gamma$ on the fourth group have no permutation standing to the left of them. Consequently to count the number of changes $(3+1+0+0)$ from the

sign of α in accordance with the first rule is the same as to count the number of letters placed under the given permutation, thus,

$$\begin{array}{l} \underline{\delta\beta\alpha\gamma} \\ \alpha\alpha\,.\,. \\ \beta \\ \gamma \end{array}$$

in accordance with the second rule.

Another point of resemblance between Rothe and De Prasse is thus made manifest, viz., that they both refused to accept Cramer's rule of signs as fundamental, preferring to base their work on a rule equally arbitrary, and then to deduce Cramer's from it.

In case it may have escaped the reader, attention may likewise be drawn to the fact that De Prasse prefixes a sign not only to permutations involving all the letters dealt with, but also to any permutation whatever involving a less number; so that in reckoning the sign of $\alpha\delta\beta$, say, the full number of letters from which α, δ, β are chosen must be known.

A theorem like Hindenburg's is next given, viz., *If the permutations of any group be separated into sub-groups,* (1) *those which begin with* α, (2) *those which begin with* β, *and so on, then the series of signs of the* 3*rd*, 5*th*, *and other odd sub-groups is identical with the series of signs of the* 1*st sub-group, and the signs of any one of the even sub-groups is got by changing each sign of the first sub-group into the opposite sign.* (III. 16.)

It is more extensive than Hindenburg's in that it is true of permutations which involve less than all the letters, provided such permutations have had their signs fixed in accordance with De Prasse's rule. The proof depends, of course, on the first rule of signs, and consists in showing that if the theorem be true for any group it must, by the said rule, be true for the next group. It will be remembered that Hindenburg gave no proof.

Following this is Rothe's theorem regarding the interchange of two elements of a permutation, or rather an extension of the theorem to signed permutations involving less than the whole number of letters. The proof is as lengthy as Rothe's, even more unnecessary letters than Rothe's c, f, e being introduced. (III. 17.)

The last theorem is Vandermonde's (XII.); and this is followed by

two pages of application to the solution of simultaneous linear equations.

No reference is made by De Prasse to Hindenburg, Rothe, or Vandermonde.

WRONSKI (1812).

[Réfutation de la Théorie des Fonctions Analytiques de Lagrange. Par Höené Wronski, pp. 14, 15, . . . , 132, 133. Paris.]

In 1810 Wronski presented to the Institute of France a memoir on the so-called *Technie de l'Algorithmie,* which with his usual sanguine enthusiasm he viewed as the essential part of a new branch of Mathematics. It contained a very general theorem, now known as "Wronski's theorem," for the expansion of functions,—a theorem requiring for its expression the use of a notation for what Wronski styled *combinatory sums.* The memoir consisted merely of a statement of results, and probably on this account, although favourably reported on by Lagrange and Lacroix, was not printed. The subject of it, however, turns up repeatedly in the *Réfutation* printed two years later; and from the indications there given we can so far form an idea of the grasp which Wronski had of the theory of the said *sums.*

At page 14 the following passage occurs:—

> "Soient X_1, X_2, X_3, &c. pleusieurs fonctions d'une quantité variable. Nommons *somme combinatoire*, et désignons par la lettre hébraïque *sin*, de la manière que voici
>
> $$שׁ[\Delta^a X_1 \,.\, \Delta^b X_2 \,.\, \Delta^c X_3 \,.\,.\,.\, \Delta^p X_\pi]\,, \qquad \text{(xv. 3) (vii. 4)}$$
>
> la somme des produits des différences de ces fonctions, composés de la manière suivante: Formez, avec les exposans $a, b, c, \ldots, p$ des différences dont il est question, toutes les permutations possibles; donnez ces exposans, dans chaque ordre de leurs permutations, aux différences consécutives qui composent le produit
>
> $$\Delta X_1 \,.\, \Delta X_2 \,.\, \Delta_3 \,.\,.\,.\, \Delta X_\pi\,;$$
>
> donnez de plus, aux produits séparés, formés de cette manière, le signe positif lorsque le nombre de variations des exposans a, b, c, etc., considérés dans leur ordre alphabétique, est nul ou

pair, et le signe négatif lorsque ce nombre de variations est impair ; enfin, prenez la somme de tous ces produits séparés.— Vous aurez ainsi, par exemple,

$$\text{ש}[\Delta^a X_1] = \Delta^a X_1\,,$$

$$\text{ש}[\Delta^a{}_1 \,.\, \Delta^b X_2] = \Delta^a X_1 .\, \Delta^b X_2 - \Delta^b X_1 \,.\, \Delta^a X_2\,,$$

. . . ."

The new name, *combinatory sum*, and the new notation, did not originate in ignorance of the work of previous investigators, for memoirs of Vandermonde and Laplace are referred to. The only fresh and real point of interest lies in the fact that the first index of every pair of indices is not attached to the same letter as the second index, but belongs to an operational symbol preceding this letter, and is used for the purpose of denoting repetition of the operation. This and the allied fact that the elements are not all independent of each other, $\Delta^1 X_1$ and $\Delta^2 X_1$, for example, being connected by the equation

$$\Delta^2 X_1 = \Delta(\Delta^1 X_1),$$

indicate that Wronski's combinatory sums form a special class with properties peculiar to themselves.

BINET (November 1812).

[Mémoire sur un système de formules analytiques, et leur application à des considérations géométriques. *Journ. de l'Ec. Polyt.*, ix. cah. 16, pp. 280–302, . . .]

It would seem as if the above-noted frequent recurrence of functions of the same kind had led Binet to a special study of them. In the memoir we have now come to, his standpoint towards them is changed. They are viewed as functions having a history: for information regarding them, the writings of Vandermonde, Laplace, Lagrange, and Gauss are referred to: they are spoken of by Laplace's name for them, *résultantes à deux lettres*, *à trois lettres*, *à quatre lettres*, &c. ; and the first twenty-three pages of the memoir are devoted expressly to establishing new theorems regarding them.

Of these the fundamental, and by far the most notable, is the afterwards well-known *multiplication-theorem.* It is enunciated at the outset as follows :—

"Lorsqu'on a deux systèmes de n lettres chacun, et nous supposerons chaque système écrit avec une seule lettre portant divers accens, qui serviront à ranger dans le même ordre les deux systèmes; on peut former avec ces lettres un nombre $n\frac{n-1}{2}$ de résultantes à deux lettres, en ne prenant dans le second terme de chacune, que des lettres portant les mêmes accens que celles du premier. Si, avec deux autres systèmes de lettres, on forme encore des résultantes à deux lettres, et qu'on les multiplie chacune par sa correspondante obtenue des deux premiers systèmes, c'est-à-dire, par celle dont les lettres portent les mêmes accens; la somme des produits de toutes ces résultantes correspondantes sera elle-même une résultante à deux lettres, dont les termes ou lettres seront des sommes de produits des élémens des deux systèmes portant les mêmes accens. Avec deux groupes de trois systèmes de lettres chacun, on peut former semblablement deux séries de résultantes à trois lettres; faisant ensuite la somme des produits de celles qui se correspondent par les accens de leurs lettres, on aura encore une résultante à trois lettres. Pareille chose ayant lieu pour des résultantes à quatre lettres, &c., on peut conclure ce théorème: Le produit d'un nombre quelconque de sommes de produits * de deux résultantes correspondantes de même ordre, est encore une résultante de cet ordre."

(XVII. 4 + XVIII. 4.)

The mode of proof adopted is lengthy, laborious, and not very satisfactory, except as affording a verification of the theorem for the cases of "résultantes" of low orders. It rests too on certain identities, the demonstration of which is open to similar criticism. All that Binet says regarding these absolutely essential identities is (p. 284)—

"Je représenterai par Σa la somme $a' + a'' + a''' + \&c.$, des quantités a', a'', a''', &c.; par Σab la somme des produits $ab + a'b' + a''b'' + \&c.$, dans chacun desquels les lettres a et b ont le même accent; par $\Sigma ab'$ la somme $a'b'' + b'a'' + a'b''' + \&c.$,

* There is an extension here which one is scarcely prepared for, viz., "*le produit d'un nombre quelconque de sommes de produits,*" instead of *la somme d'un nombre de produits.*

là tous les produits d'un des a par un des b, portent un accent différent de celui de a; par $\Sigma ab'c''$ la somme $a'b''c''' + b'c''a''' + c'a''b''' +$ &c., et ainsi de suite. Cela posé, on vérifie aisément les formules suivantes:

$$\Sigma ab' = \Sigma a \Sigma b - \Sigma ab,$$

$$\Sigma ab'c'' = \Sigma a \Sigma b \Sigma c + 2\Sigma abc - \Sigma a \Sigma bc - \Sigma b \Sigma ca - \Sigma c \Sigma ab,$$

$$\begin{aligned} \Sigma ab'c''d''' = {} & \Sigma a \Sigma b \Sigma c \Sigma d - 6\Sigma abcd \\ & - \Sigma a \Sigma b \Sigma cd - \Sigma a \Sigma c \Sigma bd - \Sigma a \Sigma d \Sigma bc \\ & - \Sigma c \Sigma d \Sigma ab - \Sigma b \Sigma d \Sigma ac - \Sigma b \Sigma c \Sigma ad \\ & + \Sigma ab \Sigma cd + \Sigma ac \Sigma bd + \Sigma ab^* \Sigma bc \\ & + 2\Sigma a \Sigma bcd + 2\Sigma b \Sigma cda + 2\Sigma c \Sigma dab \\ & + 2\Sigma d \Sigma abc, \end{aligned}$$

$$\Sigma ab'c''d'''e^{\text{iv}} = \Sigma a \Sigma b \Sigma c \Sigma d \Sigma e + \&c.,$$

&c.

It is thus seen that not only is no general proof of the identities given, but that even the law of formation of the right-hand members of the identities themselves is left undivulged. The exact words employed in the demonstration of the first case of the multiplication-theorem are (p. 286)—

"Avec un nombre n de lettres y', y'', y''', &c. et un même nombre de z', z'', z'', &c. on peut former $n\frac{n-1}{2}$ résultantes à deux lettres (y', z''), (y', z'''), &c. (y'', z''') &c.; ayant formé pareillement avec les lettres, v', v'', v''', &c., $\zeta', \zeta'', \zeta'''$ &c., les résultantes (v', ζ''), (v', ζ'''), &c., (v'', ζ'''), &c., considérons la somme $\Sigma(y, z')(v, \zeta')$ des produits des résultantes qui se correspondent par les accens dans les deux systèmes. On voit, en développant, par la multiplication, chacun des termes de cette somme, qu'elle revient à

$$\Sigma yv.z'\zeta' - \Sigma zv.y'\zeta'.$$

A ces deux dernières intégrales, on peut appliquer la transformation indiquée par la première des formules de l'art. 1 : on parvient ainsi à

$$\Sigma(y, z')(v, \zeta') = \Sigma yv \Sigma z\zeta - \Sigma zv \Sigma y\zeta.$$

Ce dernier membre pouvant être assimilé à la forme (y, z'), il

* Meant for Σad.

en résulte que le produit d'un nombre quelconque de fonctions, telles que $\Sigma(y, z')(v, \zeta')$, est lui-même de la forme (y, z')."

The application here of the identity

$$\Sigma ab' = \Sigma a \Sigma b - \Sigma ab$$

requires a little attention. The result of multiplication and classification of the terms is

$$\Sigma yv.z'\zeta' - \Sigma zv.y'\zeta',$$

or, as it might preferably be written,

$$\Sigma\{\overline{yv}\,.\,\overline{z\zeta'}\} - \{\Sigma\overline{zv}\,.\,\overline{y\zeta'}\};$$

and this we know from the said identity

$$= [\Sigma\overline{yv}\,.\,\Sigma\overline{z\zeta} - \Sigma(\overline{yv}\,.\,\overline{z\zeta})] - [\Sigma\overline{zv}\,.\,\Sigma\overline{y\zeta} - \Sigma(\overline{zv}\,.\,\overline{y\zeta})],$$

which, because of the equality of $\Sigma(\overline{yv}\,.\,\overline{z\zeta})$ and $\Sigma(\overline{zv}\,.\,\overline{y\zeta})$, becomes

$$\Sigma\overline{yv}\,.\,\Sigma\overline{z\zeta} - \Sigma\overline{zv}\,.\,\Sigma\overline{y\zeta}.$$

The inherent weak points, however, of the mode of demonstration stand out more clearly when the next case comes to be considered, viz., the case for resultants of the third order. From the three sets of n letters

$$\begin{array}{llll} x, & x', & x'', & \ldots\ldots \\ y, & y', & y'', & \ldots\ldots \\ z, & z', & z'', & \ldots\ldots \end{array}$$

all possible "résultantes à trois lettres" are formed, and each resultant is multiplied by the corresponding resultant formed from other three sets of n letters,

$$\begin{array}{llll} \xi, & \xi', & \xi'', & \ldots\ldots\ldots \\ v, & v', & v'', & \ldots\ldots\ldots \\ \zeta, & \zeta', & \zeta'', & \ldots\ldots\ldots \end{array}$$

Each of these $\frac{1}{6}n(n-1)(n-2)$ products consists of 36 terms, there being thus $6n(n-1)(n-2)$ terms in all. But these $6n(n-1)(n-2)$ terms are found to be separable into six groups, viz.

$$+\Sigma\{x\xi\,.\,y'v'\,.\,z''\zeta''\},\ +\Sigma\{y\xi\,.\,z'v'\,.\,x''\zeta''\},\ \ldots\ldots$$

so that the result which we are able to register at this point is

$$\begin{aligned} \Sigma(x,y',z'')(\xi,v',\zeta'') = {} & \Sigma x\xi\,.\,y'v'\,.\,z''\zeta'' + \Sigma y\xi\,.\,z'v'\,.\,x''\zeta'' \\ & + \Sigma z\xi\,.\,x'v'\,.\,y''\zeta'' - \Sigma x\xi\,.\,z'v'\,.\,y''\zeta'' \\ & - \Sigma y\xi\,.\,x'v'\,.\,z''\zeta'' - \Sigma z\xi\,.\,y'v'\,.\,x''\zeta''. \end{aligned}$$

To the right hand member of this the substitution

$$\Sigma ab'c'' = \Sigma a\Sigma b\Sigma c + 2\Sigma abc - \Sigma a\Sigma bc - \Sigma b\Sigma ca - \Sigma c\Sigma ab$$

is now applied six times in succession; that is to say, for

$$\Sigma x\xi \,.\, y'v' \,.\, z''\zeta''$$

and the five other term-aggregates which follow, we substitute

$$\Sigma x\xi\Sigma yv\Sigma z\zeta + 2\Sigma(x\xi \,.\, yv \,.\, z\zeta)$$
$$- \Sigma x\xi\Sigma(yv \,.\, z\zeta) - \Sigma yv\Sigma(z\zeta \,.\, x\xi) - \Sigma z\zeta\Sigma(x\xi \,.\, yv)$$

and five other like expressions. By this means we arrive, "toute réduction faite," at

$$\Sigma(x,y',z'')(\xi,v',\zeta'') = \Sigma x\xi\Sigma yv\Sigma z\zeta + \Sigma y\xi\Sigma zv\Sigma x\zeta + \Sigma z\xi\Sigma xv\Sigma y\zeta$$
$$- \Sigma x\xi\Sigma zv\Sigma y\zeta - \Sigma y\xi\Sigma xv\Sigma z\zeta - \Sigma z\xi\Sigma yv\Sigma x\zeta,$$

which is the result desired.

It is easy to imagine the troubles in store for any one who might have the hardihood to attempt to establish the next case in the same manner.

If Binet's multiplication-theorem be described as expressing *a sum of products of resultants as a single resultant*, his next theorem may be said to give *a sum of products of sums of resultants as a sum of resultants.* The paragraph in regard to it is a little too much condensed to be perfectly clear, and must therefore be given verbatim. It is (p. 288)—

"Désignons par $S(y',z'')$ une somme de résultantes, telle que

$$(y_{,}',z_{,}'') + (y_{,,}',z_{,,}'') + (y_{,,,}',z_{,,,}'') + \&c.\,;$$

c'est-à-dire,

$$y_{,}'z_{,}'' - z_{,}'y_{,}'' + y_{,,}'z_{,,}'' - z_{,,}'y_{,,}'' + y_{,,,}'z_{,,,}'' - z_{,,,}'y_{,,,}'' + \&c.\,;$$

et continuons d'employer la caractéristique Σ pour les intégrales relatives aux accens supérieurs des lettres. L'expression $\Sigma[S(y,z') \,.\, S(v,\zeta')]$ devient par le développement de chacun de ses termes, et en vertu de la première formule de l'art. 1 ou de celle du no. 4,

$$\Sigma y_{,}v_{,}\Sigma z_{,}\zeta_{,} - \Sigma z_{,}v_{,}\Sigma y_{,}\zeta_{,} + \Sigma y_{,,}v_{,}\Sigma z_{,,}\zeta_{,} - \Sigma z_{,,}v_{,}\Sigma y_{,,}\zeta_{,} + \&c.$$
$$+ \Sigma y_{,}v_{,,}\Sigma z_{,}\zeta_{,,} - \Sigma z_{,}v_{,,}\Sigma y_{,}\zeta_{,,} + \Sigma y_{,,}v_{,,}\Sigma z_{,,}\zeta_{,,} - \Sigma z_{,,}v_{,,}\Sigma y_{,,}\zeta_{,,} + \&c.$$
$$+ \&c.$$

En indiquant donc par S_1 des intégrales qui supposent, dans chaque terme, les mêmes accens inférieurs aux lettres du même alphabet, ces accens pouvant être ou non les mêmes pour celles des alphabets différens, on pourra écrire la précédente suite, en faisant usage de ce signe, ce qui donne

$$\Sigma[S(y,z')S(v,\zeta')] = S_1[\Sigma yv\Sigma z\zeta - \Sigma zv\Sigma y\zeta].$$

Cette nouvelle quantité est encore de la forme $S(y', z'')$, en sorte qu'on peut dire que le produit de fonctions, telles que

$$\Sigma\{S(y,z')\,S(v,\zeta')\},$$

sera lui-même de la forme $S(y', z'')$."

This, if I understand it correctly, may be paraphrased and expanded as follows:—

Take the product of two sums of s resultants, viz.

$$\{|y_1{}^1z_1{}^2| + |y_2{}^1z_2{}^2| + |y_3{}^1z_3{}^2| + \dots + |y_s{}^1z_s{}^2|\}$$
$$\times\{|v_1{}^1\zeta_1{}^2| + |v_2{}^1\zeta_2{}^2| + |v_3{}^1\zeta_3{}^2| + \dots + |v_s{}^1\zeta_s{}^2|\}$$

or
$$\sum_{s=1}^{s=s}|y_s{}^1z_s{}^2| \cdot \sum_{s=1}^{s=s}|v_s{}^1\zeta_s{}^2|,$$

where, it will be observed, all the resultants in the first factor are obtained from the first resultant $|y_1{}^1z_1{}^2|$ by merely changing the lower indices into 2, 3, . . . , s in succession, and that the second factor is got from the first by writing v for y and ζ for z. Then form all the like products whose first factors are

$$|y_1{}^1z_1{}^3|,\ |y_1{}^1z_1{}^4|,\ \dots,\ |y_1{}^{n-1}z_1{}^n|;$$

these being along with $|y_1{}^1z_1{}^2|$ the $\frac{1}{2}n(n-1)$ resultants derivable from the two sets of n quantities

$$y_1{}^1,\ y_1{}^2,\ y_1{}^3,\ \dots,\ y_1{}^n$$
$$z_1{}^1,\ z_1{}^2,\ z_1{}^3,\ \dots,\ z_1{}^n.$$

The sum of these $\frac{1}{2}n(n-1)$ products may be represented, if we choose, by

$$\sum_{\substack{n=2\\m<n}}^{n=n}\left[\sum_{s=1}^{s=s}|y_s{}^mz_s{}^n| \cdot \sum_{s=1}^{s=s}|v_s{}^m\zeta_s{}^n|\right].$$

Now if the multiplications be performed, there will be s^2 terms in each product, and the theorem we are concerned with has its origin in the fact that the sum of all the first terms of the products is

expressible as a resultant by applying the multiplication-theorem, likewise the sum of all the second terms, and so on, the result being an aggregate of s^2 resultants. For if we fix upon a particular term of the first product, say the term

$$|y_h^1 z_h^2| \cdot |v_k^1 \zeta_k^2|$$

which arises from the multiplication of the h^{th} term of the first factor by the k^{th} term of the second factor, then take the corresponding term of the other products, and write down their sum

$$|y_h^1 z_h^2| \cdot |v_k^1 \zeta_k^2| + |y_h^1 z_h^3| \cdot |v_k^1 \zeta_k^3| + \ldots\ldots + |y_h^{n-1} \zeta_h^n| \cdot |v_k^{n-1} \zeta_k^n|,$$

it is manifest that this sum is by the multiplication-theorem

$$= \begin{vmatrix} y_h^1 v_k^1 + y_h^2 v_k^2 + \ldots\ldots + y_h^n v_k^n & z_h^1 v_k^1 + z_h^2 v_k^2 + \ldots\ldots + z_h^n v_k^n \\ y_h^1 \zeta_k^1 + y_h^2 \zeta_k^2 + \ldots\ldots + y_h^n \zeta_k^n & z_h^1 \zeta_k^1 + z_h^2 \zeta_k^2 + \ldots\ldots + z_h^n \zeta_k^n \end{vmatrix}.$$

Consequently since h may be any integer from 1 to s, and k likewise any integer from 1 to s, the theorem arrived at is accurately expressed in modern notation as follows :—

$$\sum_{\substack{n=2 \\ m<n}}^{n=n} \left[\sum_{s=1}^{s=s} |y_s^m z_s^n| \cdot \sum_{s=1}^{s=s} |v_s^m \zeta_s^n| \right]$$

$$= \sum_{k=1}^{k=s} \sum_{h=1}^{h=s} \begin{vmatrix} y_h^1 v_k^1 + y_h^2 v_k^2 + \ldots + y_h^n v_k^n & z_h^1 v_k^1 + z_h^2 v_k^2 + \ldots + z_h^n v_k^n \\ y_h^1 \zeta_k^1 + y_h^2 \zeta_k^2 + \ldots + y_h^n \zeta_k^n & z_h^1 \zeta_k^1 + z_h^2 \zeta_k^2 + \ldots + z_h^n \zeta_k^n \end{vmatrix},$$

or

$$\sum_{k=1}^{k=s} \sum_{h=1}^{h=s} \begin{vmatrix} y_h^1 & y_h^2 & \ldots\ldots & y_h^n \\ z_h^1 & z_h^2 & \ldots\ldots & z_h^n \end{vmatrix} \cdot \begin{vmatrix} v_k^1 & v_k^2 & \ldots\ldots & v_k^n \\ \zeta_k^1 & \zeta_k^2 & \ldots\ldots & \zeta_k^n \end{vmatrix}.$$

It is easily seen to be true of resultants of any order, as Binet himself points out. (xxx.)

When s is put equal to 1, it degenerates into the multiplication-theorem.

The theorem which follows upon this, but which is quite unconnected with it, may be at once stated in modern notation. It is—

If $\Sigma|x_1 y_2 z_3|$ denote the sum of the resultants obtainable from the three sets of n quantities

$$\begin{matrix} x_1 & x_2 & x_3 & \ldots\ldots & x_n \\ y_1 & y_2 & y_3 & \ldots\ldots & y_n \\ z_1 & z_2 & z_3 & \ldots\ldots & z_n, \end{matrix}$$

and $\Sigma|x_1y_2|$ denote the like sum obtainable from the first two sets, then

$$\Sigma|x_1y_2z_3| = \Sigma x.\Sigma|y_1z_2| + \Sigma y.\Sigma|z_1x_2| + \Sigma z.\Sigma|x_1y_2| \qquad \text{(XXXI.)}$$

This is arrived at by writing out the terms of $\Sigma|y_1z_2|$, of $\Sigma|z_1x_2|$, and of $\Sigma|x_1y_2|$ in parallel columns, thus

$$\begin{array}{ccc}
|y_1\ z_2| & |z_1\ x_2| & |x_1\ y_2| \\
|y_1\ z_3| & |z_1\ x_3| & |x_1\ y_3| \\
\cdot & \cdot & \cdot \\
\cdot & \cdot & \cdot \\
\cdot & \cdot & \cdot \\
\cdot & \cdot & \cdot \\
|y_{n-1}z_n| & |z_{n-1}x_n| & |x_{n-1}y_n|;
\end{array}$$

then deriving n results from the members of the first row by multiplying by x_1, y_1, z_1 respectively and adding, multiplying by x_2, y_2, z_2, and adding, and so on; then treating the second and remaining rows in the same way; and then finally adding all the $n \cdot \frac{1}{2}n(n-1)$ results together. Each of these results is a vanishing or non-vanishing resultant of the 3rd order, and it will be found that each non-vanishing resultant occurs twice with the sign $+$ and once with the sign $-$.

This process is readily seen to be simply the same as performing the multiplications indicated in the right-hand member of (XXXI.), *i.e.*,

$$\begin{aligned}
&(x_1+x_2+\ldots+x_n)\,(|y_1z_2| + |y_1z_3| + \ldots + |y_{n-1}z_n|) \\
+&(y_1+y_2+\ldots+y_n)\,(|z_1x_2| + |z_1x_3| + \ldots + |z_{n-1}x_n|) \\
+&(z_1+z_2+\ldots+z_n)\,(|x_1y_2| + |x_1y_3| + \ldots + |x_{n-1}y_n|),
\end{aligned}$$

summing every three corresponding terms in the products, and writing the sum as a vanishing or non-vanishing resultant. There would be $n \cdot \frac{1}{2}n(n-1)$ resultants in all; but as each suffix occurs $n-1$ times in the second factors and once in the first factors, there must be in each product $n-1$ terms having the said suffix occurring twice: consequently there must be $n-1$ resultants vanishing on account of this recurrence, and therefore altogether $n(n-1)$ vanishing resultants. Of the non-vanishing resultants,—in number equal to $n \cdot \frac{1}{2}n(n-1) - n(n-1)$, or $\frac{1}{2}n(n-1)(n-2)$,—each one of the form

$$|x_hy_kz_l| \qquad \text{where } h<k<l$$

must be accompanied by two others,

$$|\, x_k y_h z_l \,| \text{ and } |\, x_l y_h z_k \,|,$$

and the sum of these is

$$|\, x_h y_k z_l \,| - |\, x_k y_k z_l \,| + |\, x_h y_k z_l \,|,$$

i.e.,

$$|\, x_h y_k z_l \,|.$$

The final result is thus the sum of the resultants of the form

$$|\, x_h y_k z_l \,| \text{ where } h<k<l, \text{ and } l=3,\ 4,\ \ldots,\ n,$$

the number of them, as we may see from two different standpoints, being

$$\tfrac{1}{6}n(n-1)(n-2).$$

Returning to the series of identities,

$$\begin{aligned} x_3|y_1z_2| + y_3|z_1x_2| + z_3|x_1y_2| &= |x_1y_2z_3|, \\ x_4|y_1z_2| + y_4|z_1x_2| + z_4|x_1y_2| &= |x_1y_2z_4|, \\ \&\text{c.} \qquad\qquad & \quad \&\text{c.} \end{aligned}$$

which by addition give the result

$$\Sigma x\Sigma|y_1z_2| + \Sigma y\Sigma|z_1x_2| + \Sigma z\Sigma|x_1y_2| = \Sigma|x_1y_2z_3|,$$

Binet next raises both sides of all of them to the second power, and obtains

$$\left.\begin{aligned} 3\Sigma|x_1y_2z_3|^2 = \Sigma x^2\Sigma|y_1z_2|^2 + \Sigma y^2\Sigma|z_1x_2|^2 + \Sigma z^2\Sigma|x_1y_2|^2 & \\ + 2\Sigma yz\Sigma(|z_1x_2|.|x_1y_2|) + 2\Sigma zx\Sigma(|x_1y_2|.|y_1z_2|) & \\ + 2\Sigma xy\Sigma(|y_1z_2|.|z_1x_2|). & \end{aligned}\right\}\text{(XXXII.)}$$

Substituting for $\Sigma|y_1z_2|^2$, $\Sigma|z_1x_2|^2$, ... , their equivalents as given by the multiplication-theorem, he then deduces

$$\left.\begin{aligned} \Sigma|x_1y_2z_3|^2 = \Sigma x^2\Sigma y^2\Sigma z^2 + 2\Sigma yz\Sigma zx\Sigma xy - \Sigma x^2(\Sigma z^x)^2 & \\ - \Sigma y^2(\Sigma zx)^2 - \Sigma z^2(\Sigma xy)^2, & \end{aligned}\right\}$$

not failing to note that this is not a fresh result, but merely a case of the multiplication-theorem in which the factors are equal.

By putting the right-hand member here into the form

$$\begin{aligned} \Sigma y^2\{\Sigma z^2\Sigma x^2 - (\Sigma yz)^2\} + \Sigma z^2\{\Sigma x^2\Sigma y^2 - (\Sigma xy)^2\} & \\ - \Sigma x^2\{\Sigma y^2\Sigma z^2 - (\Sigma yz)^2\} + 2\Sigma yz\{\Sigma zx\Sigma xy - \Sigma yz\Sigma x^2\}, & \end{aligned}$$

there is next arrived at the first identity of the set

$$\left.\begin{aligned}&\Sigma|x_1y_2z_3|^2\\ &= \Sigma y^2\Sigma|z_1x_2|^2 + \Sigma z^2\Sigma|x_1y_2|^2 - \Sigma x^2\Sigma|y_1z_2|^2 + 2\Sigma yz\Sigma|z_1x_2||x_1y_2|\,,\\ &= \Sigma z^2\Sigma|x_1y_2|^2 + \Sigma x^2\Sigma|y_1z_2|^2 - \Sigma y^2\Sigma|z_1x_2|^2 + 2\Sigma zx\Sigma|x_1y_2||y_1z_2|\,,\\ &= \Sigma x^2\Sigma|y_1z_2|^2 + \Sigma y^2\Sigma|z_1x_2|^2 - \Sigma z^2\Sigma|x_1y_2|^2 + 2\Sigma xy\Sigma|y_1z_2||z_1x_2|\,,\end{aligned}\right\}\text{(XXXIII.)}$$

and immediately from these the set

$$\left.\begin{aligned}\Sigma|x_1y_2z_3|^2 &= \Sigma x^2\Sigma|y_1z_2|^2 + \Sigma zx\Sigma|x_1y_2|.|y_1z_2| + \Sigma xy\Sigma|y_1z_2|.|z_1x_2|\,,\\ &= \Sigma y^2\Sigma|z_1x_2|^2 + \Sigma xy\Sigma|y_1z_2|.|z_1x_2| + \Sigma yz\Sigma|z_1x_2|.|x_1y_2|\,,\\ &= \Sigma z^2\Sigma|x_1y_2|^2 + \Sigma yz\Sigma|z_1x_2|.|x_1y_2| + \Sigma zx\Sigma|x_1y_2|.|y_1z_2|\,.\end{aligned}\right\}\text{(XXXIV.)}$$

We may note in passing that either of these sets leads at once to the initial theorem

$$\begin{aligned}3\Sigma|x_1y_2z_3|^2 = \Sigma x^2\Sigma|y_1z_2|^2 &+ \Sigma y^2\Sigma|z_1x_2|^2 + \Sigma z^2\Sigma|x_1y_2|^2\\ &+ 2\Sigma yz\Sigma|z_1x_2|.|x_1y_2| + 2\Sigma zx\Sigma|x_1y_2|.|y_1z_2|\\ &+ 2\Sigma xy\Sigma|y_1z_2|.|z_1x_2|\,,\end{aligned}$$

and that with the multiplication-theorem already established this reverse order would be the more natural.

The next step taken is the formation of resultants of the 2nd order from elements which are themselves resultants of the 2nd order; viz., just as from the three rows of n quantities

$$\begin{matrix} x_1 & x_2 & x_3 & \cdots\cdots & x_n\\ y_1 & y_2 & y_3 & \cdots\cdots & y_n\\ z_1 & z_2 & z_3 & \cdots\cdots & z_n\end{matrix}$$

there were formed the three other rows of $\frac{1}{2}n(n-1)$ quantities

$$\begin{matrix} |y_1z_2|\,,\ |y_1z_3|\,,\ \ldots\ldots,\ |y_1z_n|\,,\ |y_2z_3|\,,\ \ldots\ldots\ldots,\ |y_{n-1}z_n|\,,\\ |z_1x_2|\,,\ |z_1x_3|\,,\ \ldots\ldots,\ |z_1x_n|\,,\ |z_2x_3|\,,\ \ldots\ldots\ldots,\ |z_{n-1}x_n|\,,\\ |x_1y_2|\,,\ |x_1y_3|\,,\ \ldots\ldots,\ |x_1y_n|\,,\ |x_2y_3|\,,\ \ldots\ldots\ldots,\ |x_{n-1}y_n|\,,\end{matrix}$$

so from the latter three other rows of quantities

$$\begin{vmatrix} |z_1x_2| & |z_1x_3|\\ |x_1y_2| & |x_1y_3|\end{vmatrix},\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots,\ \begin{vmatrix} |z_{n-2}x_n| & |z_{n-1}x_n|\\ |x_{n-2}y_n| & |x_{n-1}y_n|\end{vmatrix},$$

$$\begin{vmatrix} |x_1y_2| & |x_1y_3|\\ |y_1z_2| & |y_1z_3|\end{vmatrix},\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots,\ \begin{vmatrix} |x_{n-2}y_n| & |x_{n-1}y_n|\\ |y_{n-2}z_n| & |y_{n-1}z_n|\end{vmatrix},$$

$$\begin{vmatrix} |y_1z_2| & |y_1z_3|\\ |z_1x_2| & |z_1x_3|\end{vmatrix},\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots,\ \begin{vmatrix} |y_{n-2}z_n| & |y_{n-1}z_n|\\ |z_{n-2}x_n| & |z_{n-1}x_n|\end{vmatrix},$$

are formed, the number in each new row being clearly

$$\tfrac{1}{2}\{\tfrac{1}{2}n(n-1)\}\{\tfrac{1}{2}n(n-1)-1\}$$

$$i.e., \qquad \tfrac{1}{8}n(n-1)\,(n-2)\,(n+1)\,.$$

The new quantities are, of course, not written by Binet in the form

$$\left\|\begin{matrix} |\quad\quad|\quad\quad|\quad\quad|\quad\quad| \\ |\quad\quad|\quad\quad|\quad\quad|\quad\quad| \end{matrix}\right\|,$$

but the fact that they are resultants of the 2nd order is carefully noted. Each of them is shown to be transformable, by a theorem which may be viewed as an extension of a result given by Lagrange, so as to have two of the elements resultants of the 3rd order, and the others resultants of the 1st order. This is done by taking, for example, the identities

$$x_h|y_i z_j| + y_h|z_i x_j| + z_h|x_i y_j| = |x_h y_i z_j|,$$

$$x_k|y_i z_j| + y_k|z_i x_j| + z_k|x_i y_j| = |x_k y_i z_j|,$$

multiplying both sides of the first by x_k, and both sides of the second by x_h, subtracting, and writing the result in the form

$$|x_k y_h||z_i x_j| + |x_k z_h||x_i y_j| = x_k|x_h y_i z_j| - x_h|x_k y_i z_j|,$$

$$= \begin{vmatrix} x_k & x_h \\ |x_k y_i z_j| & |x_h y_i z_j| \end{vmatrix},$$

where of course it has to be noted that in many cases one of the resultants of the 3rd order will vanish. The quantities, therefore, to be dealt with, are

$$x_1|x_1 y_2 z_3|, \ldots\ldots, x_k|x_h y_i z_j| - x_h|x_k y_i z_j|, \ldots\ldots, x_n|x_{n-2} y_{n-1} z_n|;$$

$$y_1|x_1 y_2 z_3|, \ldots\ldots, y_k|y_h z_i x_j| - y_h|y_k z_i x_j|, \ldots\ldots, y_n|x_{n-2} y_{n-1} z_n|;$$

$$z_1|x_1 y_2 z_3|, \ldots\ldots, z_k|z_h x_i y_j| - z_h|z_k x_i y_j|, \ldots\ldots, z_n|x_{n-2} y_{n-1} z_n|.$$

By raising each of the elements of the first row to the second power, taking the sum and simplifying, we could, we are told, show that the result would be

$$\Sigma x_1^2\ \Sigma|x_1 y_2 z_3|^2.$$

Very prudently, however, another process is chosen. It is recalled that the quantities in the third triad of rows are related to those in the second as those in the second are related to those in the first, and that consequently the required sum of squares of resultants is, by the multiplication-theorem itself, expressible as a resultant, viz.,

$$\Sigma \Big\| |z_1x_2| \, , \, |x_1y_3| \Big\|^2 = \Sigma|z_1x_2|^2 \, . \, \Sigma|x_1y_2|^2 - \left(\Sigma|z_1x_2||x_1y_2|\right)^2 ,$$

where the elements of the resultant on the right are sums of products of quantities in the second triad of rows. Then the same theorem is used to make a further step backwards, viz., to express each of these three sums of products of resultants as a resultant whose elements are sums of products of the quantities in the first triad of rows, the effect of the substitution being

$$\begin{aligned}\Sigma \Big\| |z_1x_2| \, , \, |x_1y_3| \Big\|^2 = \{\Sigma z_1{}^2\Sigma x_1{}^2 - (\Sigma z_1x_1)^2\}\{\Sigma x_1{}^2\Sigma y_1{}^2 - (\Sigma x_1y_1)^2\} \\ - \{\Sigma z_1x_1\Sigma x_1y_1 - \Sigma y_1z_1\Sigma x_1{}^2\}^2 .\end{aligned}$$

Simple multiplication transforms this into

$$\Sigma x_1{}^2 \left\{ \begin{array}{r} \Sigma x_1{}^2\Sigma y_1{}^2\Sigma z_1{}^2 - \Sigma y_1{}^2(\Sigma z_1x_1)^2 - \Sigma z_1{}^2(\Sigma x_1y_1)^2 \\ + 2\Sigma y_1z_1\Sigma z_1x_1\Sigma x_1y_1 - \Sigma x_1{}^2(\Sigma y_1z_1)^2 \end{array} \right\} ,$$

which, by still another use of the multiplication-theorem, we know is equal to

$$\Sigma x_1{}^2 \, \Sigma|x_1y_2z_3|^2 .$$

The set of six results of which this is one, is

$$\left.\begin{aligned} \Sigma X_1{}^2 &= \Sigma x_1{}^2 \, \Sigma|x_1y_2z_3|^2 , \\ \Sigma Y_1{}^2 &= \Sigma y_1{}^2 \, \Sigma|x_1y_2z_3|^2 , \\ \Sigma Z_1{}^2 &= \Sigma z_1{}^2 \, \Sigma|x_1y_2z_3|^2 , \\ \Sigma Y_1Z_1 &= \Sigma y_1z_1\Sigma|x_1y_2z_3|^2 , \\ \Sigma Z_1X_1 &= \Sigma z_1x_1\Sigma|x_1y_2z_3|^2 , \\ \Sigma X_1Y_1 &= \Sigma x_1y_1\Sigma|x_1y_2z_3|^2 , \end{aligned}\right\} \qquad \text{(xxxv.)}$$

if, for shortness, we denote the quantities of the third triad of rows by

$$\begin{array}{lll} X_1, & X_2, & \ldots\ldots \\ Y_1, & Y_2, & \ldots\ldots \\ Z_1, & Z_2, & \ldots\ldots \end{array}$$

Following these, and deduced by means of them, is an equally noteworthy theorem regarding the sums of squares of all the resultants of the third order, which can be formed from the quantities of the second triad of rows. Denoting these quantities temporarily by

$$\begin{array}{lll} \xi_1, & \xi_2, & \ldots\ldots\ldots \\ \eta_1, & \eta_2, & \ldots\ldots\ldots \\ \zeta_1, & \zeta_2, & \ldots\ldots\ldots \end{array}$$

we know (XXXII.) that

$$3\Sigma|\xi\eta_2\zeta_3|^2 = \Sigma X_1{}^2\Sigma\xi_1{}^2 + \Sigma Y_1{}^2\Sigma\eta_1{}^2 + \Sigma Z_1{}^2\Sigma\zeta_1{}^2 + 2\Sigma Y_1Z_1 \,.\, \Sigma\eta_1\zeta_1 + 2\Sigma Z_1X_1 \,.\, \Sigma\zeta_1\xi_1 + 2\Sigma X_1Y_1 \,.\, \Sigma\xi_1\eta_1\,;$$

whence, by using the set of six results just obtained, we have

$$3\Sigma|\xi_1\eta_2\zeta_3|^2 = \Sigma|x_1y_2z_3|^2\left\{\begin{array}{l}\Sigma\xi_1{}^2\Sigma x_1{}^2 + \Sigma\eta_1{}^2\Sigma y_1{}^2 + \Sigma\zeta_1{}^2\Sigma z_1{}^2 \\ + 2\Sigma\eta_1\zeta_1 . \Sigma y_1z_1 + 2\Sigma\zeta_1\xi_1 . \Sigma z_1x_1 + 2\Sigma\xi_1\eta_1 . \Sigma x_1y_1\end{array}\right\}$$

and therefore, again by (XXXII.)

$$\Sigma|\xi_1\eta_2\zeta_3|^2 = \{\Sigma|x_1y_2z_3|^2\}^2 . \qquad \text{(XXXVI.)}$$

It is finally pointed out that from the third triad of rows there might, in like manner, be formed a fourth triad, and analogous identities obtained; also that, instead of starting with three rows, we might start with *four*,

$$\begin{array}{l} t_1, \quad t_2, \quad t_3, \quad \ldots\ldots, \quad t_n \\ x_1, \quad x_2, \quad x_3, \quad \ldots\ldots, \quad x_n \\ y_1, \quad y_2, \quad y_3, \quad \ldots\ldots, \quad y_n \\ z_1, \quad z_2, \quad z_3, \quad \ldots\ldots, \quad z_n\,, \end{array}$$

form from them other four

$$\begin{array}{l} |x_1y_2z_3|, \ldots\ldots\ldots\ldots \\ |y_1z_2t_3|, \ldots\ldots\ldots\ldots \\ |z_1t_2x_3|, \ldots\ldots\ldots\ldots \\ |t_1x_2y_3|, \ldots\ldots\ldots\ldots, \end{array}$$

thence in the same way a third four, and in connection therewith establish the identity

$$\Sigma t_1\Sigma|x_1y_2z_3| - \Sigma x_1\Sigma|y_1z_2t_3| + \Sigma y_1\Sigma|z_1t_2x_3| - \Sigma z_1\Sigma|t_1x_2y_3| = 0 \quad \text{(XXXI. 2)}$$

and other analogues. (XXXII. 2 + XXXV. 2.)

The rest of the memoir, 52 pages, consists of geometrical applications of the series of theorems thus obtained.

CAUCHY (1812).

[Mémoire sur les fonctions qui ne peuvent obtenir que deux valeurs égales et de signes contraires par suite des transpositions

opérées entre les variables qu'elles renferment. *Journ. de l'Ec. Polyt.*, x. Cah. 17, pp. 29–112.]

This masterly memoir of 84 pages was read to the Institute on the same day (30th November) as Binet's memoir, of which we have just given an account. The coincidence of date has to be carefully noted, because the memoirs have in part a common ground, and because there is a presumption that the authors, knowing this beforehand, had, in a friendly way, arranged for simultaneous publicity. Binet's words on the matter are—

> "Ayant en dernièrement occasion de parler à M. Cauchy, ingénieur des ponts et chaussées, du théorème générale que j'ai énoncé ci-dessus, il me dit être parvenu, dans des recherches analogues à celles de M. Gauss, à des théorèmes d'analyse qui devaient avoir rapport aux miens. Je m'en suis assuré, en jetant les yeux sur ces formules : mais j'ignore si elles ont la même généralité que les miennes : nous y sommes arrivés, je crois, par des voies très-différentes."

And Cauchy's corroboration is (p. 111)—

> "J'avais rencontré l'été dernier, à Cherbourg, où j'étais fixé par les travaux de mon état, ce théorème et quelques autres du même genre, en cherchant à généraliser les formules de M. Gauss. M. Binet, dont je me félicite d'être l'ami, avait été conduit aux mêmes résultats par des recherches différentes. De retour à Paris, j'étais occupé de poursuivre mon travail, lorsque j'allai le voir. Il me montra son théorème qui était semblable au mien. Seulement il désignait sous le nom de *résultante* ce que j'avais appelé *déterminant*."

Cauchy prefaces his memoir by another, entitled

Sur le nombre des valeurs qu'une fonction peut acquérir lorsqu'on y permute de toutes les manières possibles les quantités qu'elle renferme.

This latter must to a certain extent be taken into account, because it serves to show the point of view which he considered most natural for examining the subject, and also the exact position held by the functions now called determinants, when functions in

general come to be classified according to the number of values they are able to assume in certain circumstances.

At the outset of it the writings of Lagrange, Vandermonde, and Ruffini are referred to; the fact is recalled that the maximum number of values which a function can acquire by interchanges among its n variables is $1.2.3 \ldots . n$; also that when the maximum is not obtained, the actual number must be a factor of the maximum; and then proof is given of the very notable theorem that *the number of values cannot be less than the greatest prime contained in* n *without being equal to* 2. It is pointed out likewise that functions capable of having only two values are known from Vandermonde to be constructible for any number of variables. For example, the number of variables being three, a_1, a_2, a_3, all that is needed is to form their difference-product

$$(a_3 - a_2)\,(a_3 - a_1)\,(a_2 - a_1)$$

or

$$a_3{}^2a_2 + a_2{}^2a_1 + a_1{}^2a_3 - (a_3{}^2a_1 + a_2{}^2a_3 + a_1{}^2a_2)\,,$$

when it is found that either of the parts

$$a_3{}^2a_2 + a_2{}^2a_1 + a_1{}^2a_3\,,$$

or

$$a_3{}^2a_1 + a_2{}^2a_3 + a_1{}^2a_2\,,$$

is an instance of a function capable of only two values by permutation of the variables; the result indeed of any permutation being merely that the one function passes into the other. Further, the whole expression

$$a_3{}^2a_2 + a_2{}^2a_1 + a_1{}^2a_3 - (a_3{}^2a_1 + a_2{}^2a_3 + a_1{}^2a_2)$$

is another example, the difference between the two values which it can assume being however a difference of sign merely. As a reference to the title of the memoir of November 1812 will show, it is functions of this latter class which Cauchy there considers.

At the commencement he contrasts them with functions which suffer no change whatever by permutation of variables, that is to say, *symmetric* functions: and, noting the fact, afterwards ascertained, that the new functions consist of terms alternately + and −, and that were it not for this alternation of sign they would be symmetric functions, he decides to extend the term "symmetric" to them, and having done so, seeks to distinguish them from ordi-

nary symmetric functions by calling them "fonctions symétriques alternées," and calling the other "fonctions symétriques permanentes." Cauchy's view of determinants may therefore now be described by saying that he considered them as *a special class of alternating symmetric functions.*

To include them, however, either the adoption of a convention is necessary, or an extension of the definition must be made. For example, $a_1b_2 - a_2b_1$ is not an alternating function, unless the elements be so related that the interchange of a_1 and a_2 necessitates the interchange of b_1 and b_2 at the same time; or unless the definition be so worded that interchange shall refer to *suffixes,* not to letters. Cauchy selects the former course, his words being (p. 30)

". . . . . concevons les diverses suites de quantités

$$\begin{array}{cccc} a_1, & a_2, & \ldots\ldots, & a_n \\ b_1, & b_2, & \ldots\ldots, & b_n \\ c_1, & c_2, & \ldots\ldots, & c_n \\ \ldots & \ldots & \ldots\ldots & \ldots \end{array}$$

tellement liées entre elles, que la transposition de deux indices pris dans l'une des suites, nécessite la même transposition dans toutes les autres ; alors, les quantités

$$b_1, c_1, \ldots, b_2, c_2, \ldots, b_3, c_3, \ldots\ldots$$

pourront être considérées comme des fonctions semblables de

$$a_1, a_2, a_3, \ldots\ldots;$$

et par suite, les fonctions de

$$a_1, b_1, c_1, \ldots, a_2, b_2, c_2, \ldots, a_n, b_n, c_n, \ldots\ldots$$

qui ne changeront pas de valeur, mais tout au plus de signe, en vertu de transpositions operées entre les indices 1, 2, 3, ... n, devront être rangées parmi les fonctions symétriques de $a_1, a_2, \ldots, a_n$, ou, ce que revient au même, des indices 1, 2, 3, . . . , n. Ainsi

$$a_1^2 + a_2^2 + 4a_1a_2,$$

$$a_1b_1 + a_2b_2 + a_3b_3 + 2c_1c_2c_3,$$

$$a_1b_2 + a_2b_3 + a_3b_1 + a_2b_1 + a_3b_2 + a_1b_3,$$

$$\cos(a_1 - a_2)\cos(a_1 - a_3)\cos(a_2 - a_3),$$

seront des fonctions symétriques permanentes, la première du second ordre et les autres du troisième ; et au contraire,

$$a_1b_2 + a_2b_3 + a_3b_1 - a_2b_1 - a_1b_3 - a_3b_2,$$
$$\sin(a_1 - a_2)\sin(a_1 - a_3)\sin(a_2 - a_3)$$

seront des fonctions symétriques alternées du troisième ordre."

The question of nomenclature being settled there next arises the question of notation. This also is decided on the ground of the resemblance of the functions to symmetric functions. It being known that any symmetric function is representable by a typical term preceded by a symbol indicating permutation of the variables, *e.g.*

$$S(a_1b_2) \text{ or } S^2(a_1b_2) \text{ standing for } a_1b_2 + a_2b_1$$

and $S^3(a_1b_2)$ standing for $a_1b_2 + a_2b_3 + a_3b_1 + a_2b_1 + a_3b_2 + a_1b_3$;

also, that any non-symmetric function may be taken as the typical term of a symmetric function, the question arises whether the like may not be true of alternating functions. A lengthy examination of the latter point leads to the conclusion that any non-symmetric function K *cannot* be the originating or typical term of an alternating function unless it satisfies a certain condition, viz., that it be such that any value of it obtained by an even number of transpositions of indices will be different from any other value obtained by an odd number of transpositions. Should, however, this condition be satisfied, and K_α, K_β, K_γ, ... be all the values of the former kind, and K_λ, K_μ, K_ν, ... all the values of the latter kind, then

$$(K_\alpha + K_\beta + K_\gamma + \ldots\ldots) - (K_\lambda + K_\mu + K_\nu + \ldots\ldots)$$

is an alternating function and is appropriately representable by

$$S(\pm K)$$

if the indices appearing in K alone are to be permuted, and by

$$S^n(\pm K)$$

if the indices to be permuted be 1, 2, 3, . . . , n. For example, taking the typical term a_1b_2 we have

$$S(\pm a_1b_2) = a_1b_2 - a_2b_1,$$

and
$$S^3(\pm a_1b_2) = a_1b_2 + a_2b_3 + a_3b_1 - a_2b_1 - a_3b_2 - a_1b_3,$$
$$= S^3(\mp a_2b_1) = S^3(\mp a_1b_3) = \ldots\ldots$$

$S_4(\pm a_1 b_2)$ is an impossibility, as when there are four indices $a_1 b_2$ does not satisfy the condition required of a typical term; indeed, Cauchy notes that the number of indices in any term must either be the total number or 1 less.

The number of permutations being even, it is clear that *the number of* + *terms* K_α, K_β, ... *is the same as the number of negative terms* K_λ, K_μ, (x. 2)
a generalisation of a remark of Vandermonde's.

Further, since K_α, K_β, . . . are all the terms that arise from an even number of transpositions, and K_λ, K_μ, ... all those that arise from an odd number of transpositions, it is plain that any single transposition performed upon each of the terms of the function

$$(K_\alpha + K_\beta + K_\gamma + \ldots\ldots) - (K_\lambda + K_\mu + K_\nu + \ldots\ldots)$$

must change it into

$$(K_\lambda + K_\mu + K_\nu + \ldots\ldots) - (K_\alpha + K_\beta + K_\gamma + \ldots\ldots)$$

—this is, in fact, the proof that it is an alternating function—consequently each of the parts

$$K_\alpha + K_\beta + K_\gamma + \ldots\ldots,$$
$$K_\lambda + K_\mu + K_\nu + \ldots\ldots,$$

belongs to the class of functions which have only two different values.

Also it is evident that *if throughout the function any particular index be changed into another and no further alteration made, the resulting expression must be equal to zero,* (XII. 5)
a theorem regarding alternating functions which is the generalisation of a theorem of Vandermonde's.

We have lastly to note, that the criterion which determines whether a particular K belongs to the class K_α, K_β, ... or to the class K_λ, K_μ, ... is incidentally shown to be reducible to a more practical form. For example, if the term be K_θ, and it be derivable from K , say, by the change of the suffixes 1, 2, 3, 4, 5, 6, 7 into 3, 2, 6, 5, 4, 1, 7, that is to say, in Cauchy's language by means of the substitution

$$\begin{pmatrix} 1, & 2, & 3, & 4, & 5, & 6, & 7 \\ 3, & 2, & 6, & 5, & 4, & 1, & 7 \end{pmatrix},$$

we transform this substitution into a "product" of "circular" substitutions, viz., into

$$\begin{pmatrix}1, 3, 6\\3, 6, 1\end{pmatrix}\cdot\begin{pmatrix}4 & 5\\5 & 4\end{pmatrix}\cdot\begin{pmatrix}2\\2\end{pmatrix}\cdot\begin{pmatrix}7\\7\end{pmatrix}$$

and subtracting the number of "factors," 4, from the total number of suffixes 7, make the sign + or – according as this difference is even or odd.

Here the subject of general alternating functions may be left for the present. What remains of the first part of the memoir, refers to special cases, which naturally fall to be considered in another chapter of our history. At the close of the part, Cauchy says (p. 51)—

> "Je vais maintenant examiner particulièrement une certaine espèce de fonctions symétriques alternées qui s'offrent d'elles-mêmes dans un grand nombre de recherches analytiques. C'est au moyen de ces fonctions qu'on exprime les valeurs générales des inconnues que renferment plusieurs équations du premier degré. Elles se représentent toutes les fois qu'on a des équations à former, ainsi que dans la théorie générale de l'élimination."

The writings of Laplace, Vandermonde, Bézout, and Gauss are referred to, and from the latter the name "déterminant" is adopted.

The second part bears the title—

Des fonctions symétriques alternées désignées sous le nom de déterminans.

and opens with the following explanatory definition (p. 51)—

> "Soient $a_1, a_2, \ldots, a_n$ plusieurs quantités différentes en nombre égal à n. On a fait voir ci-dessus qu'en multipliant le produit de ces quantités, ou
>
> $$a_1a_2a_3 \ldots a_n$$
>
> par le produit de leurs différences respectives, ou par
>
> $$(a_2-a_1)(a_3-a_1)\ldots(a_n-a_1)(a_3-a_2)\ldots(a_n-a_2)\ldots(a_n-a_{n-1})$$
>
> on obtenait pour résultat la fonction symétrique alternée
>
> $$S(\pm a_1a_2^2a_3^3 \ldots a_n^n),$$

qui par conséquent se trouve toujours égale au produit

$$a_1a_2a_3 \ . \ . \ . \ . a_n$$
$$\times (a_2-a_1)(a_3-a_1) \ldots (a_n-a_1)(a_3-a_2) \ldots (a_n-a_2) \ldots (a_n-a_{n-1}) \ .$$

Supposons maintenant que l'on développe ce dernier produit, et que dans chaque terme du développement on remplace l'exposant de chaque lettre par un second indice égale à l'exposant dont il s'agit, en écrivant par exemple $a_{r,s}$ au lieu de $a_r^{\ s}$, et $a_{s,r}$ au lieu de $a_s^{\ r}$, on obtiendra pour résultat une nouvelle fonction symétrique alternée, qui, au lieu d'être représentée par

$$S(\pm\ a_1^{\ 1}a_2^{\ 2}a_3^{\ 3} \ . \ . \ . \ . \ a_n^{\ n})$$

sera représentée par

$$S(\pm\ a_{1\cdot 1}a_{2\cdot 2}a_{3\cdot 3} \ . \ . \ . \ . \ a_{n\cdot n}) \ ,$$

le signe S étant relatif aux premiers indices de chaque lettre. Telle est la forme la plus générale des fonctions que je désignerai dans la suite sous le nom de *déterminans.* Si l'on suppose successivement *

$$n=1, \ n=2, \ \&c. \ . \ . \ . \ . \ .$$

on trouvera

$$\begin{aligned} S(\pm a_{1\cdot 1}a_{2\cdot 2}) &= a_{1\cdot 1}a_{2\cdot 2} - a_{2\cdot 1}a_{1\cdot 2} \ , \\ S(\pm a_{1\cdot 1}a_{2\cdot 2}a_{3\cdot 3}) &= a_{1\cdot 1}a_{2\cdot 2}a_{3\cdot 3} + a_{2\cdot 1}a_{3\cdot 2}a_{1\cdot 3} + a_{3\cdot 1}a_{1\cdot 2}a_{2\cdot 3} \\ &\quad - a_{1\cdot 1}a_{3\cdot 2}a_{2\cdot 3} - a_{3\cdot 1}a_{2\cdot 2}a_{1\cdot 3} - a_{2\cdot 1}a_{1\cdot 2}a_{3\cdot 3} \ . \\ \&c. \ . \ . \ . \ . \ . \ . \ . \end{aligned}$$

pour les déterminans du second, du troisième ordre, &c. . . . . "

In regard to this it is important to notice that there are really two definitions given us. The latter, viz., that involved in the symbolism of alternating functions,

$$S(\pm a_{1\cdot 1}a_{2\cdot 2}a_{3\cdot 3} \ . \ . \ . \ . \ a_{n\cdot n})$$

contains nothing more than Leibnitz's rule of formation and an improved rule of signs. The former is new and may be paraphrased as follows :—

If the multiplications indicated in the expression

$$a_1a_2a_3 \ldots a_n$$
$$\times (a_2-a_1)(a_3-a_1) \ldots (a_n-a_1)(a_3-a_2) \ldots (a_n-a_2) \ldots (a_n-a_{n-1})$$

* $n=2$, $n=3$, &c. is meant.

be performed, and in the result every index of a power be changed into a second suffix, e.g., a_r^s into $a_{r,s}$, the expression so obtained is called a determinant (VIII. 2),
and is denoted by

$$S(\pm a_{1\cdot 1}a_{2\cdot 2}a_{3\cdot 3}\ \ldots\ldots\ a_{n\cdot n}) \qquad \text{(VII. 5)}.$$

In this definition the rule of signs and the rule of term-formation are inseparable—a peculiarity already observed in the case of Bézout's rule of 1764.

After the definitions various technical terms are introduced. The n^2 different quantities involved in

$$S(\pm a_{1\cdot 1}a_{2\cdot 2}a_{3\cdot 3}\ \ldots\ldots\ a_{n\cdot n})$$

are arranged thus

$$\begin{cases} a_{1\cdot 1}, & a_{1\cdot 2}, & a_{1\cdot 3}, & \ldots\ldots & a_{1\cdot n} \\ a_{2\cdot 1}, & a_{2\cdot 2}, & a_{2\cdot 3}, & \ldots\ldots & a_{2\cdot n} \\ a_{3\cdot 1}, & a_{3\cdot 2}, & a_{3\cdot 3}, & \ldots\ldots & a_{3\cdot n} \\ \&c. \ldots\ldots & & & & \\ a_{n\cdot 1}, & a_{n\cdot 2}, & a_{n\cdot 3}, & \ldots\ldots & a_{n\cdot n} \end{cases}$$

"sur un nombre égal à n de lignes horizontales et sur autant de colonnes verticales," and as thus arranged are said to form a *symmetric system* of order n. The individual quantities $a_{1\cdot 1}$, &c. are called the *terms* of the system, and the letter a when free of suffixes the *characteristic.* The "terms" in a horizontal line are said to form a *suite horizontale*, in a vertical column a *suite verticale. Conjugate* terms are defined as those whose suffixes ("indices") differ in order, *e.g.*, $a_{2\cdot 3}$ and $a_{3\cdot 2}$; and terms which are self-conjugate, *e.g.*, $a_{1\cdot 1}$, $a_{2\cdot 2}$, . . . are called *principal* terms. The determinant is said to *belong* to the system, or to be the determinant *of* the system. The parts of the expanded determinant which are connected by the signs + and − are called *symmetric products*, and the product

$$a_{1\cdot 1}a_{2\cdot 2}a_{3\cdot 3}\ \ldots\ldots\ a_{n\cdot n}$$

of the principal "terms" is called the *principal* product. The "principal product," however, is also called the *terme indicatif* of the determinant, and thus an awkward double use of the word "terme" is brought into prominence. The system

$$\begin{cases} a_{1\cdot1} & a_{2\cdot1} & a_{3\cdot1} & \dots\dots & a_{n\cdot1} \\ a_{1\cdot2} & a_{2\cdot2} & a_{3\cdot2} & \dots\dots & a_{n\cdot2} \\ a_{1\cdot3} & a_{2\cdot3} & a_{3\cdot3} & \dots\dots & a_{n\cdot3} \\ \&c. \dots\dots & & & & \\ a_{1\cdot n} & a_{2\cdot n} & a_{3\cdot n} & \dots\dots & a_{n\cdot n} \end{cases}$$

derived from the previous system by interchanging the suffixes of each "terme" is said to be *conjugate* to the previous system. A symbol for each of these systems is got by taking the last "terme" of its first "suite horizontale," and enclosing the "terme" in brackets: in this way we are enabled to say that $(a_{1\cdot n})$ *and* $(a_{n\cdot1})$ *are conjugate systems.*

In the course of these explanations a modification of the rule of term-formation is incidentally noted, the form taken being specially applicable when the quantities of the system have been disposed in a square. Cauchy's wording of this now familiar rule is (p. 55)—

> . . . . . "pour former chacun des termes dont il s'agit, il suffira de multiplier entre elles n quantités différentes prises respectivement dans les différentes colonnes verticales du système, et situées en même temps dans les diverses lignes horizontales de ce système."

Here we may note in passing that the disposal of the "termes" in a square might have enabled Cauchy to point out (which he did not do) the difference between Gauss' use of the word "determinant" and his own, by saying that the "determinant of a form" had its conjugate "termes" equal.

The rule of signs applicable to alternating functions in general is modified for the special case of determinants, and takes the following form (p. 56):—

> "Étant donné un produit symétrique quelconque, pour obtenir le signe dont il est affecté dans le déterminant
>
> $$S(\pm a_{1\cdot1}a_{2\cdot2}a_{3\cdot3} \dots\dots a_{n\cdot n})$$
>
> il suffira d'appliquer la règle qui sert à déterminer le signe d'un terme pris à volonté dans une fonction symétrique alternée. Soit
>
> $$a_{\alpha\cdot1}\, a_{\beta\cdot2} \dots\dots a_{\zeta\cdot n}$$
>
> le produit symétrique dont il s'agit, et désignons par g le

nombre des substitutions circulaires équivalentes à la substitution

$$\begin{pmatrix} 1 & 2 & 3 & \ldots\ldots & n \\ \alpha & \beta & \gamma & \ldots\ldots & \zeta \end{pmatrix}.$$

Ce produit devra être affecté du signe $+$, si $n-g$ est un nombre pair, et du signe $-$ dans le cas contraire." (III. 18).

Thus if the sign of the term

$$a_{6\cdot1}\,a_{8\cdot2}\,a_{3\cdot3}\,a_{1\cdot4}\,a_{9\cdot5}\,a_{2\cdot6}\,a_{5\cdot7}\,a_{4\cdot8}\,a_{7\cdot9}$$

in the determinant

$$S(\pm a_{1\cdot1}\,a_{2\cdot2}\,a_{3\cdot3}\;\ldots\ldots\;a_{9\cdot9}),$$

be wanted, we write the series of first suffixes 6, 8, . . . under the corresponding suffixes of the "principal product," that is to say, under the series 1, 2, 3 . . . 9, obtaining the interchange

$$\begin{pmatrix} 1 & 2 & 3 & 4 & 5 & 6 & 7 & 8 & 9 \\ 6 & 8 & 3 & 1 & 9 & 2 & 5 & 4 & 7 \end{pmatrix};$$

this we separate into circular interchanges, finding them three in number, viz.,

$$\begin{pmatrix} 3 \\ 3 \end{pmatrix},\ \begin{pmatrix} 5 & 7 & 9 \\ 9 & 5 & 7 \end{pmatrix},\ \begin{pmatrix} 1 & 2 & 4 & 6 & 8 \\ 6 & 8 & 1 & 2 & 4 \end{pmatrix};$$

and the determinant being of the 9th order, we thence conclude that the desired sign is $(-)^{9-3}$, *i.e.*, $+$. In connection with this subject a modification of Cramer's rule is given, no reference being made to "dérangements" at all. Put into the fewest possible words it is—*The sign of the term $a_{\alpha\cdot1}\,a_{\beta\cdot2}\ldots\ldots a_{\zeta\cdot n}$ is the same as the sign of the difference-product of the first suffixes, that is, the sign of*

$$(\beta-\alpha)(\gamma-\alpha)\ldots\ldots(\zeta-\alpha)(\gamma-\beta)\ldots\ldots \qquad \text{(III. 19)}.$$

For example, the sign of

$$a_{6\cdot1}\,a_{8\cdot2}\,a_{3\cdot3}\,a_{1\cdot4}\,a_{9\cdot5}\,a_{2\cdot6}\,a_{5\cdot7}\,a_{4\cdot8}\,a_{7\cdot9},$$

above sought, is the sign of the difference-product of

$$6,\ 8,\ 3,\ 1,\ 9,\ 2,\ 5,\ 4,\ 7$$

i.e., the sign of

$$\begin{aligned} &(7-4)(7-5)(7-2)(7-9)(7-1)(7-3)(7-8)(7-6) \\ &\quad\times(4-5)(4-2)\;\ldots\ldots\ldots\ldots\;(4-6) \\ &\qquad\times(5-2)\;\ldots\ldots\ldots\ldots\;(5-6) \\ &\qquad\qquad\ldots\ldots\ldots\ldots\ldots\ldots \\ &\qquad\qquad\qquad\times(8-6) \end{aligned}$$

The object which Cauchy had in view in stating the rule in this unnecessarily complex form was doubtless to show its essential identity with the rule implied in his new definition. He says (p. 58)—

> "On démontre facilement cette règle par ce qui précéde, attendu qu'une transposition operée entre deux indices change toujours, comme on l'a fait voir, le signe du produit
>
> $$(a_\beta - a_\alpha)\,(a_\gamma - a_\alpha)\,\ldots\,(a_\zeta - a_\alpha)\,(a_\gamma - a_\beta)\,\ldots\,,$$
>
> et par conséquent celui du produit
>
> $$(\beta-\alpha)\,(\gamma-\alpha)\,\ldots\,(\zeta-\alpha)\,(\gamma-\beta)\,\ldots\,"$$

The way having thus been prepared, the propositions of determinants are entered on. Those known to his predecessors we may dispose of rapidly, giving little, if anything, more than the enunciation of them, in order that the new garb in which they appear may be seen.

> .... "le déterminant du système $(a_{n\cdot 1})$ est égal à celui du système $(a_{1\cdot n})$ ..... En conséquence, dans l'expression
>
> $$S(\pm a_{1\cdot 1}a_{2\cdot 2}\,\ldots\,a_{n\cdot n})$$
>
> on peut supposer indifféremment, ou que le signe S se rapporte aux premiers indices, ou qu'il se rapporte aux seconds: (IX. 2).
>
> Si l'on échange entre elles deux suites horizontales ou deux suites verticales du système $(a_{1\cdot n})$ de manière à faire passer dans une des suites tous les termes de l'autre et réciproquement on obtiendra un nouveau système symétrique, dont le déterminant sera évidemment égal mais de signe contraire à celui du système $(a_{1\cdot n})$. Si l'on répète la même opération plusieurs fois de suite, on obtiendra divers systèmes symétriques dont les déterminans seront égaux entre eux, mais alternativement positifs et négatifs. On peut faire la même remarque à l'égard du systéme $(a_{n\cdot 1})$ (XI. 3).
>
> ..... si l'on développe la fonction symétrique alternée
>
> $$S[\pm a_{n\cdot n}S(\pm a_{1\cdot 1}\,a_{2\cdot 2}\,\ldots\,a_{n-1\cdot n-1})]$$
>
> tous les termes du développement seront des produits symétriques de l'ordre n, qui auront l'unité pour coefficient. Ces

termes seront donc respectivement égaux à ceux qu'on obtient en développant le déterminant

$$D_n = S(\pm a_{1\cdot 1}\, a_{2\cdot 2} \;.\;.\;.\;.\; a_{n\cdot n})\,;$$

et comme le produit principal $a_{1\cdot 1}\, a_{2\cdot 2} \;.\;.\;.\; a_{n\cdot n}$ est positif de part et d'autre, on aura nécessairement

$$\begin{aligned} D_n &= S[\pm a_{n\cdot n} S(\pm a_{1\cdot 1}\, a_{2\cdot 2} \;.\;.\;.\;.\; a_{n-1\cdot n-1})] \qquad \text{(VI. 3)} \\ &= a_{n\cdot n} b_{n\cdot n} + a_{n-1\cdot n} b_{n-1\cdot n} + \;.\;.\;.\;.\; + a_{1\cdot n} b_{1\cdot n}\,. \end{aligned}$$

En général, si l'on désigne par μ l'un des indices 1, 2, 3, . . . , n on trouvera de la même manière

$$D_n = S[\pm a_{\mu\cdot\mu} S(\pm a_{1\cdot 1}\, a_{2\cdot 2} \;.\;.\;.\;.\; a_{\mu-1\cdot\mu-1} a_{\mu+1\cdot\mu+1} \;.\;.\;.\; a_{n\cdot n})] \qquad \text{(VI. 4).}$$

. . . . . Cette dernière équation

$$0 = a_{1\cdot\nu} b_{1\cdot\mu} + a_{2\cdot\nu} b_{2\cdot\mu} + \;.\;.\;.\; + a_{n\cdot\nu} b_{n\cdot\mu} \qquad \text{(XII. 6)}$$

sera satisfacte toutes les fois que ν et μ seront deux nombres différens l'un de l'autre.

. . . . on aura donc aussi

$$\begin{aligned} D_n &= a_{\mu\cdot 1} b_{\mu\cdot 1} + a_{\mu\cdot 2} b_{\mu\cdot 2} + \;.\;.\;.\;.\; + a_{\mu\cdot n} b_{\mu\cdot n} \qquad \text{(VI. 4)} \\ 0 &= a_{\nu\cdot 1} b_{\mu\cdot 1} + a_{\nu\cdot 2} b_{\mu\cdot 2} + \;.\;.\;.\;.\; + a_{\nu\cdot n} b_{\mu\cdot n} \qquad \text{(XII. 6)} \end{aligned}$$

les indices μ et ν étant censés inégaux."

The expressions here denoted by $b_{1\cdot 1}$, $b_{1\cdot 2}$, ... are spoken of as *adjugate* (" adjointes ") to $a_{1\cdot 1}$, $a_{1\cdot 2}$, . . . ; and the system

$$\left\{ \begin{array}{llll} b_{1\cdot 1} & b_{1\cdot 2} & \cdots\cdots & b_{1\cdot n} \\ b_{2\cdot 1} & b_{2\cdot 2} & \cdots\cdots & b_{2\cdot n} \\ \&c. & \cdots & \cdots\cdots & \\ b_{n\cdot 1} & b_{n\cdot 2} & \cdots\cdots & b_{n\cdot n} \end{array} \right.$$

as adjugate to the system $(a_{1\cdot n})$. Similarly the system $(b_{n\cdot 1})$ is said to be adjugate to the system $(a_{n\cdot 1})$; and, on the other hand, it is said to be *adjugate and conjugate* to the system $(a_{1\cdot n})$.

Up to this point no new property has been brought forward. The following paragraph (p. 68), however, opens new ground, the formula given in it being of some considerable importance in the after development of the theory.

" Si dans le système de quantités $(a_{1\cdot n})$ on supprime la dernière

suite horizontale et la dernière suite verticale, on aura le système suivant,

$$\begin{cases} a_{1\cdot 1}, & a_{2\cdot 1} \;.\;.\;.\;.\;.\;.\; & a_{1\cdot n-1}, \\ a_{2\cdot 1}, & a_{2\cdot 2} & a_{2\cdot n-1}, \\ \&c. \;.\;.\;.\;. & & \\ a_{n-1\cdot 1}, & a_{n-1\cdot 2} & a_{n-1\cdot n-1}, \end{cases}$$

que je désignerai à l'ordinaire par $(a_{1\cdot n-1})$.

"Soit maintenant $(e_{1\cdot n-1})$ le système adjoint au précédent. Si dans l'équation (13) on change b en e et n en $n-1$, ou aura en général

$$D_{n-1} = b_{n\cdot n} = a_{\mu\cdot 1}e_{\mu\cdot 1} + a_{\mu\cdot 2}e_{\mu\cdot 2} + \ldots + a_{\mu\cdot n-1}e_{\mu\cdot n-1}\,.$$

Pour déduire de cette dernière équation la valeur de $b_{\mu\cdot n}$, il suffira en vertu des règles établies, de changer $a_{\mu\cdot\nu}$ en $a_{n\cdot\nu}$ dans l'expression précédente de $b_{n\cdot n}$, et de changer en outre le signe du second membre : ou aura donc généralement

$$b_{\mu\cdot n} = -\left(a_{n\cdot 1}e_{\mu\cdot 1} + a_{n\cdot 2}e_{\mu\cdot 2} + \;.\;.\;.\;.\;.\; + a_{n\cdot n-1}e_{\mu\cdot n-1}\right).$$

Si dans cette équation on donne successivement à μ toutes les valeurs entières depuis 1 jusqu'à $n-1$, et que l'on substitue les valeurs qui en résulteront pour $b_{1\cdot n}$, $b_{2,n}, \ldots b_{n-1\cdot n}$ dans l'équation

$$D_n = a_{1\cdot n}b_{1\cdot n} + a_{2\cdot n}b_{2\cdot n} + \;.\;.\;.\;.\; + a_{n\cdot n}b_{n\cdot n}\,,$$

on obtiendra la formule suivante,

$$D_n = a_{n\cdot n}b_{n\cdot n} - \begin{cases} a_{1\cdot n}a_{n\cdot 1}e_{1\cdot 1} & + a_{2\cdot n}a_{n\cdot 2}e_{2\cdot 2} + \ldots + a_{n-1\cdot n}a_{n\cdot n-1}e_{n-1\cdot n-1} \\ + a_{1\cdot n}(a_{n\cdot 2}e_{1\cdot 2} & + a_{n\cdot 3}e_{1\cdot 3} \quad + \ldots + a_{n\cdot n-1}e_{1\cdot n-1}) \\ + a_{2\cdot n}(a_{n\cdot 1}e_{2\cdot 1} & + a_{n\cdot 3}e_{2\cdot 3} \quad + \ldots + a_{n\cdot n-1}e_{2\cdot n-1}) \\ + \&c. \;.\;.\;.\;.\;.\;.\;.\; & \\ + a_{n-1\cdot n}(a_{n\cdot 1}e_{n-1\cdot 1} & + a_{n\cdot 2}e_{n-1\cdot 2} + \ldots + a_{n\cdot n-2}e_{n-1\cdot n-2}). \end{cases}$$

Cette équation peut être mise sous la forme

$$D_n = a_{n\cdot n}D^*_{n\cdot 1} - S^{n-1}S^{n-1}(a_{\nu\cdot n}a_{n\cdot\mu}\epsilon_{\nu\cdot\mu}), \qquad \text{(XXXVII.)}$$

les deux signes S étant relatifs le premier à l'indice μ et le second à l'indice ν."

This is the well-known formula nowadays described as giving

* Misprint in original, for D_{n-1}.

the development of a determinant according to binary products of a row and column. The special row here used is the n^{th} and the special column the n^{th} likewise.

The four pages regarding the application of determinants to the solution of a set of simultaneous equations may be passed over with the remark that they give evidence of the importance attached by Cauchy to his new definition of determinants, the solution in the case of the example

$$\left.\begin{array}{l} a_1x_1 + b_1x_1 = m_1 \\ a_2x_1 + b_2x_2 = m_2 \end{array}\right\}$$

being first put in the form

$$x = \frac{mb(b-m)}{ab(b-a)}, \quad y = \frac{am(m-a)}{ab(b-a)};$$

and similarly in the case of the example

$$a_rx_1 + b_rx_2 + c_rx_3 = m_r \qquad (r = 1, 2, 3).$$

The determinant solution of a set of simultaneous equations is put to good use by Cauchy to obtain new properties of the functions. Taking the set of equations

$$(20)\left\{\begin{array}{lllllll} a_{1\cdot1}x_1 & + & a_{1\cdot2}x_2 & + \;.\;.\;.\;.\;.\; + & a_{1\cdot n}x_n & = & m_1 \\ a_{2\cdot1}x_1 & + & a_{2\cdot2}x_2 & + \;.\;.\;.\;.\;.\; + & a_{2\cdot n}x_n & = & m_2 \\ \&\text{c.} \;.\;.\;.\;. & & & & & & \\ a_{n\cdot1}x_1 & + & a_{n\cdot2}x_2 & + \;.\;.\;.\;.\;.\; + & a_{n\cdot n}x_n & = & m_n \end{array}\right.$$

and solving for $x_1, x_2, \ldots$ he obtains of course the set

$$\left.\begin{array}{lllllll} m_1b_{1\cdot1} & + & m_2b_{2\cdot1} & + \;.\;.\;.\;.\;.\; + & m_nb_{n\cdot1} & = & \mathrm{D}_nx_1, \\ m_1b_{1\cdot2} & + & m_2b_{2\cdot2} & + \;.\;.\;.\;.\;.\; + & m_nb_{n\cdot2} & = & \mathrm{D}_nx_2, \\ \&\text{c.} \;.\;.\;.\;.\;.\;.\;. & & & & & & \\ m_1b_{1\cdot n} & + & m_2b_{2\cdot n} & + \;.\;.\;.\;.\;.\; + & m_nb_{n\cdot n} & = & \mathrm{D}_nx_n, \end{array}\right\}$$

where $b_{1\cdot1}, b_{2\cdot1}, \ldots\ldots$ have the signification above indicated, and D_n stands for $\mathrm{S}(\pm a_{1\cdot1}a_{2\cdot2} \ldots a_{n\cdot n})$. This second set may be treated in the same way as the first set, the quantities $m_1, m_2, \ldots, m_n$ being viewed as the unknowns. To express the result the system of quantities adjugate to $(b_{1\cdot n})$ is denoted by $(c_{1\cdot n})$, and the determinant of the system $(b_{1\cdot n})$ is denoted by B_n, the new set thus being

$$(27)\quad \begin{cases} c_{1\cdot 1}D_n x_1 + c_{1\cdot 2}D_n x_2 + \ldots\ldots + c_{1\cdot n}D_n x_n = B_n m_1 , \\ c_{2\cdot 1}D_n x_1 + c_{2\cdot 2}D_n x_2 + \ldots\ldots + c_{2\cdot n}D_n x_n = B_n m_2 , \\ \&c. \ldots\ldots\ldots \\ c_{n\cdot 1}D_n x_1 + c_{n\cdot 2}D_n x_2 + \ldots\ldots + c_{n\cdot n}D_n x_n = B_n m_n , \end{cases}$$

Cauchy then proceeds (p. 77)—

"Les équations (27) peuvent encore être mises sous la forme suivante,

$$\begin{cases} c_{1\cdot 1}\frac{D_n}{B_n}x_1 + c_{1\cdot 2}\frac{D_n}{B_n}x_2 + \ldots\ldots + c_{1\cdot n}\frac{D_n}{B_n}x_n = m_1 , \\ c_{2\cdot 1}\frac{D_n}{B_n}x_1 + c_{2\cdot 2}\frac{D_n}{B_n}x_2 + \ldots\ldots + c_{2\cdot n}\frac{D_n}{B_n}x_n = m_2 , \\ \&c. \ldots\ldots\ldots \\ c_{n\cdot 1}\frac{D_n}{B_n}x_1 + c_{n\cdot 2}\frac{D_n}{B_n}x_2 + \ldots\ldots + c_{n\cdot n}\frac{D_n}{B_n}x_n = m_n ; \end{cases}$$

et commes celles-ci doivent avoir lieu en même temps que les équations (20), sans que l'on suppose d'ailleurs entre les termes de la suite $x_1, x_2, \ldots\ldots, x_n$ et ceux du système $(a_{1\cdot n})$ aucune relation particulière, il faudra nécessairement que l'on ait quels que soient μ et ν,

$$c_{\mu\cdot\nu}\frac{D_n}{B_n} = a_{\mu\cdot\nu} ,$$

ou

$$c_{\mu\cdot\nu} = \frac{B_n}{D_n}a_{\mu\cdot\nu} . \qquad \text{(XXXVIII.)}$$

Cette équation établit un rapport constant entre les termes du système $(a_{1\cdot n})$ et les termes du système adjoint du second ordre $(c_{1\cdot n})$."

More definitely, and in more modern nomenclature, the theorem is

The ratio of any element of a determinant to the corresponding element of the second adjugate determinant is equal to the ratio of the determinant itself to its first adjugate. (XXXVIII.)

Attention is next directed to the group of equations—

$$\left.\begin{array}{llll}
\alpha_{1\cdot1}a_{1\cdot1}+\alpha_{1\cdot2}a_{1\cdot2}+\,.\,.+\alpha_{1\cdot n}a_{1\cdot n}=m_{1\cdot1} & \alpha_{2\cdot1}a_{1\cdot1}+\alpha_{2\cdot2}a_{1\cdot2}+\,.\,.+\alpha_{2\cdot n}a_{1\cdot n}=m_{1\cdot2}, & \ldots & \alpha_{n\cdot1}a_{1\cdot1}+\alpha_{n\cdot2}a_{1\cdot2}+\ldots+\alpha_{n\cdot n}a_{1\cdot n}=m_{1\cdot n}\\
\alpha_{1\cdot1}a_{2\cdot1}+\alpha_{1\cdot2}a_{2\cdot2}+\,.\,.+\alpha_{1\cdot n}a_{2\cdot n}=m_{2\cdot1} & \alpha_{2\cdot1}a_{2\cdot1}+\alpha_{2\cdot2}a_{2\cdot2}+\,.\,.+\alpha_{2\cdot n}a_{2\cdot n}=m_{2\cdot2}, & \ldots & \alpha_{n\cdot1}a_{2\cdot1}+\alpha_{n\cdot2}a_{2\cdot2}+\ldots+\alpha_{n\cdot n}a_{2\cdot n}=m_{2\cdot n}\\
\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot & \cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot & \cdot & \cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\\
\alpha_{1\cdot1}a_{n\cdot1}+\alpha_{1\cdot2}a_{n\cdot2}+\,.\,.+\alpha_{1\cdot n}a_{n\cdot n}=m_{n\cdot1} & \alpha_{2\cdot1}a_{n\cdot1}+\alpha_{2\cdot2}a_{n\cdot2}+\,.\,.+\alpha_{2\cdot n}a_{n\cdot n}=m_{n\cdot2}, & \ldots & \alpha_{n\cdot1}a_{n\cdot1}+\alpha_{n\cdot2}a_{n\cdot2}+\ldots+\alpha_{n\cdot n}a_{n\cdot n}=m_{n\cdot n}
\end{array}\right\}$$

Here there are three symmetric systems of quantities

$$(a_{1\cdot n}), \ (\alpha_{1\cdot n}), \ (m_{1\cdot n}),$$

the first appearing in every column of equations, the second in every row, and the third only once. The determinants of these systems are denoted by

$$D_n, \ \delta_n, \ M_n,$$

respectively: that is to say

$$D_n = S(\pm a_{1\cdot 1} a_{2\cdot 2} \ . \ . \ . \ a_{n\cdot n})$$
$$\delta_n = S(\pm \alpha_{1\cdot 1} \alpha_{2\cdot 2} \ . \ . \ . \ \alpha_{n\cdot n})$$
$$M_n = S(\pm m_{1\cdot 1} m_{2\cdot 2} \ . \ . \ . \ m_{n\cdot n}).$$

If now in

$$S(\pm a_{1\cdot 1} a_{2\cdot 2} \ . \ . \ . \ a_{n\cdot n})$$

there be substituted for $m_{1\cdot 1}$, $m_{1\cdot 2}$, ... their values as given by the group of equations, there will be obtained a function of all the a's and α's, which must be an alternating function with respect to the first indices of the a's and also with respect to the first indices of the α's. Further, since each of the m's is of the first degree in the a's and of the first degree also in the α's, each term of the development of $S(\pm m_{1\cdot 1} m_{2\cdot 2} \ . \ . \ . \ . \ m_{n\cdot n})$ must evidently be of the form

$$\pm a_{1\cdot \mu} a_{2\cdot \nu} \ . \ . \ . \ . \ a_{n\cdot \pi} \alpha_{1\cdot \mu_{\prime}} \alpha_{2\cdot \nu_{\prime}} \ . \ . \ . \ . \ \alpha_{n\cdot \pi}.$$

But the development by reason of its double alternating character cannot contain such a term without containing all the terms of the product

$$\pm S(\pm a_{1\cdot \mu} a_{2\cdot \nu} \ . \ . \ . \ a_{n\cdot \pi}) S(\pm \alpha_{1\cdot \mu_{\prime}} \alpha_{2\cdot \nu_{\prime}} \ . \ . \ . \ \alpha_{n\cdot \pi_{\prime}}).$$

Consequently it must equal one or more products of this kind. But again the indices μ, ν, . . ., π are either all different or not. If they be different, we have

$$S(\pm a_{1\cdot \mu} a_{2\cdot \nu} \ . \ . \ . \ a_{n\cdot \pi}) = \pm S(\pm a_{1\cdot 1} a_{2\cdot 2} \ . \ . \ . \ a_{n\cdot n}) = \pm \delta_n;$$

and if any two of them be equal

$$S(\pm a_{1\cdot \mu} a_{2\cdot \nu} \ . \ . \ . \ a_{n\cdot \pi}) = 0.$$

The like is true in regard to $S \pm (\alpha_{1\cdot \mu_{\prime}} \alpha_{2\cdot \nu_{\prime}} \ . \ . \ . \ \alpha_{n\cdot \pi_{\prime}})$. This enables us to conclude that the development of M_n is equal to one or more products of the form

$$\pm D_n \delta_n:$$

in other words, that

$$M_n = c D_n \delta_n,$$

where c is a constant. But if we take the very special case where

$$a_{\mu\cdot\mu}=1,\quad \alpha_{\mu\cdot\mu}=1,\quad a_{\mu\cdot\nu}=0,\quad \alpha_{\mu\cdot\nu}=0,$$

and where consequently

$$m_{\mu\cdot\mu}=1,\quad m_{\mu\cdot\nu}=0\,,$$

we see that

$$\mathrm{M}_n=1,\quad \mathrm{D}_n=1,\quad \delta_n=1\,,$$

and that therefore

$$c=1\,.$$

Hence the final result is

$$\mathrm{M}_n=\mathrm{D}_n\delta_n\,. \qquad \text{(XVII. 5).}$$

This, the now well-known multiplication-theorem of determinants, Cauchy puts in words as follows (p. 82) :—

> *Lorsqu'un système de quantités est déterminé symétriquement au moyen de deux autres systèmes, le déterminant du système résultant est toujours égal au produit des déterminans des deux systèmes composans.* (XVII. 5).

It is quite clear, from what has been said above, that it was discovered independently, and about the same time, by Binet and Cauchy, and ought to bear the names of both. Binet has the further merit of having reached a theorem of which Cauchy's is a special case, and then made an additional generalisation in a different direction; and Cauchy has the advantage over Binet of having produced, along with his special case, a satisfactory proof of it.

From the theorem Cauchy goes on to deduce several results equally important. Substituting for the system $(a_{1\cdot n})$ the system $(b_{1\cdot n})$ adjugate to $(a_{1\cdot n})$ so that

$$\delta_n=\mathrm{S}(\pm b_{1\cdot 1}b_{2\cdot 2}\;.\;.\;.\;b_{n\cdot n})=\mathrm{B}_n\,,$$

we know that then

$$m_{\mu\cdot\mu}=\mathrm{D}_n \text{ and } m_{\mu\cdot\nu}=0\,;$$

that consequently M_n consists of but a single term, viz.

$$m_{1\cdot 1}m_{2\cdot 2}\;.\;.\;.\;m_{n\cdot n},\quad \textit{i.e.}\ \mathrm{D}_n^n\;:$$

and that therefore by the theorem

$$\mathrm{D}_n^n=\mathrm{B}_n\mathrm{D}_n\,,$$

whence

$$\mathrm{B}_n=\mathrm{D}_n^{n-1}\,. \qquad \text{(XXI. 2).}$$

This result, afterwards so well known, Cauchy translates into words as follows (p. 82) :—

> . . . . *le déterminant du système* ($b_{1\cdot n}$) *adjoint au système* ($a_{1\cdot n}$) *est égal à la* $(n-1)^{me}$ *puissance du déterminant de ce dernier système.* (XXI. 2).

Again, by returning to the identity,

$$c_{\mu\cdot\nu} = \frac{B_n}{D_n} a_{\mu\cdot\nu}$$

and substituting the value of B_n just obtained, there is deduced the result

$$c_{\mu\cdot\nu} = D_n^{n-2} a_{\mu\cdot\nu} ; \qquad \text{(XXXIX.)}$$

or, in words,

> . . . *étant donné un terme quelconque* $a_{\mu\cdot\nu}$ *du système* ($a_{1\cdot n}$), *pour obtenir le terme correspondant du système adjoint du second ordre* ($c_{1\cdot n}$) *il suffira de multiplier le terme donné par la* $(n-2)^{me}$ *puissance du déterminant du premier système.* (XXXIX.)

A considerable amount of space (pp. 82–92) is devoted to the consideration of the adjugate systems of

$$(a_{1\cdot n}) \ , \ (\alpha_{1\cdot n}) \ , \ (m_{1\cdot n}) \ ,$$

and the adjugates of these adjugates; but nothing new is elicited. The section closes with the manifest identity

$$\begin{aligned} & (\alpha_{1\cdot 1} + \alpha_{2\cdot 1} + \ . \ . \ . \ + \alpha_{n\cdot 1}) \ (a_{1\cdot 1} + a_{2\cdot 1} + \ . \ . \ . \ + a_{n\cdot 1}) \\ & + (\alpha_{1\cdot 2} + \alpha_{2\cdot 2} + \ . \ . \ . \ + \alpha_{n\cdot 2}) \ (a_{1\cdot 2} + a_{2\cdot 2} + \ . \ . \ . \ + a_{n\cdot 2}) \\ & + \&c. \ . \ . \ . \ . \ . \ . \ . \\ & + (\alpha_{1\cdot n} + \alpha_{2\cdot n} + \ . \ . \ . \ + \alpha_{n\cdot n}) \ (a_{1\cdot n} + a_{2\cdot n} + \ . \ . \ . \ + a_{n\cdot n}) \\ = \ & m_{1\cdot 1} + m_{2\cdot 1} + \ . \ . \ . \ + m_{n\cdot 1} \\ & + m_{1\cdot 2} + m_{2\cdot 2} + \ . \ . \ . \ + m_{n\cdot 2} \\ & + \ . \ . \ . \ . \ . \\ & + m_{1\cdot n} + m_{2\cdot n} + \ . \ . \ . \ + m_{n\cdot n} \ , \end{aligned}$$

which, using later technical terms, we may express as follows :—

If there be two determinants, and the sum of the elements of one first column be multiplied by the sum of the elements of the other first column, the sum of the elements of one second column by the sum of the elements of the other second column, and so on, then the sum of these products is equal to the sum of the elements of the product of the two determinants. (XL.)

The third section breaks entirely fresh ground, its heading being

Des Systèmes de Quantités dérivées et de leurs Déterminans.

Of the integers 1, 2, 3, . . ., n all the possible sets of p integers are supposed to be taken, and arranged in order on the principle that any one has precedence of any other if the product of the members of the former be less than the product of the members of the latter. The number $n(n-1) \ldots . (n-p+1) / 1.2.3 \ldots . p$ of the said sets being denoted by P, the Pth and last set would thus be

$$n-p+1, n-p+2, \ldots \ldots, n-1, n.$$

Now, any two of the sets being fixed upon, say the μ^{th} and ν^{th}, the system of quantities $(a_{1\cdot n})$ is returned to, and from it are deleted (1) all the "termes" whose first index is not found in the μ^{th} set, and (2) all the "termes" whose second index is not found in the ν^{th} set. What is left after this action is clearly "un système de quantités symétriques de l'ordre p," the determinant of which may be denoted by $\overset{(p)}{a}_{\mu,\nu}$. For example, if $\mu=\nu=1$, all the a's would be deleted whose first or second index was not included in the set 1, 2, 3, . . ., p, and there would be left the system

$$\left\{\begin{array}{llll} a_{1\cdot 1} & a_{1\cdot 2} & \ldots . & a_{1\cdot p} \\ a_{2\cdot 1} & a_{2\cdot 2} & \ldots . & a_{2\cdot p} \\ \&c. \ldots \ldots & & & \\ a_{p\cdot 1} & a_{p\cdot 2} & \ldots . & a_{p\cdot p} \end{array}\right.$$

of which the determinant would be denoted by

$$\overset{(p)}{a}_{1\cdot 1}.$$

As any one of the P sets could be taken along with any other, preparatory to forming such a determinant, there would necessarily be in all $\mathrm{P}\times\mathrm{P}$ possible determinants. Arranged in a square as follows:—

$$\left\{\begin{array}{llll} \overset{(p)}{a}_{1\cdot 1} & \overset{(p)}{a}_{1\cdot 2} & \ldots . & \overset{(p)}{a}_{1\cdot \mathrm{P}} \\ \overset{(p)}{a}_{2\cdot 1} & \overset{(p)}{a}_{2\cdot 2} & \ldots . & \overset{(p)}{a}_{2\cdot \mathrm{P}} \\ \&c. \ldots \ldots & & & \\ \overset{(p)}{a}_{\mathrm{P}\cdot 1} & \overset{(p)}{a}_{\mathrm{P}\cdot 2} & \ldots . & \overset{(p)}{a}_{\mathrm{P}\cdot \mathrm{P}} \end{array}\right.$$

they manifestly form "un système symétrique de l'ordre P," the determinant of which, in strict accordance with previous convention, is denoted by

$$\left(\overset{p}{a}_{1\cdot\mathrm{P}}\right).$$

Cauchy then proceeds (p. 96)—

Si l'on donne successivement à p toutes les valeurs

$$1, 2, 3, \ldots, n-3, n-2, n-1$$

P prendra les valeurs suivantes,

$$n, \frac{n(n-1)}{1\,.\,2}, \frac{n(n-1)(n-2)}{1\,.\,2\,.\,3}, \ldots, \frac{n(n-1)}{1\,.\,2}, n,$$

et l'on obtiendra par suite un nombre égal à n − 1 de systèmes symétriques différens les uns des autres, dont le premier sera le système donné $(a_{1\cdot n})$. Ces différens systèmes seront désignés respectivement par

$$(a_{1\cdot n}), \left[\overset{(2)}{a}_{1\cdot\frac{n(n-1)}{1\,.\,2}}\right], \left[\overset{(3)}{a}_{1\cdot\frac{n(n-)(n-2)}{1\,.\,2\,.\,3}}\right], \ldots\ldots$$
$$\left[\overset{(n-3)}{a}_{1\cdot\frac{n(n-1)(n-2)}{1\,.\,2\,.\,3}}\right], \left[\overset{(n-2)}{a}_{1\cdot\frac{n(n-1)}{1\,.\,2}}\right], \left(\overset{(n-1)}{a}_{1\cdot n}\right);$$

je les appellerai *systèmes dérivés* de $(a_{1\cdot n})$. Parmi ces systèmes, ceux qui correspondent à des valeurs de p dont la somme est égale à n sont toujours de même ordre; je les appellerai *systèmes dérivés complémentaires.* Ainsi en général

$$\left(\overset{(p)}{a}_{1\cdot\mathrm{P}}\right) \text{ et } \left(\overset{(n-p)}{a}_{1\cdot\mathrm{P}}\right)$$

sont deux systèmes dérivés complémentaires l'un de l'autre, dont l'ordre est égal a

$$\mathrm{P} = \frac{n(n-1)\ldots\ldots(n-p+1)}{1\,.\,2\,.\,3\ldots p}.$$

PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. XIV. 1886-87. No. 125.

On the Conducting Paths between the Cortex of the Brain and the Lower Centres in relation to Physiology and Pathology. By D. J. Hamilton, M.B., F.R.C.S. Edin., F.R.S.E., Professor of Pathology, University of Aberdeen. (Plates XIV., XV.)

(Read 31st January 1887.)

Methods.—The great difficulties heretofore encountered in investigating the course of nerve fibres in the brain have been, firstly, the want of a method of preparation by which their gross anatomy could be thoroughly exposed, and, secondly, the failure of any previously known process of staining to satisfactorily indicate their direction on microscopic examination. In endeavouring to collect reliable data from the records of lesions of the human brain, it becomes only too evident that until more efficient methods of localising lesions be adopted than those generally in use at the present day, little can possibly be added to the knowledge we already possess.

The methods of preparation I now employ for demonstrating the connections of the brain are chiefly the *gelatine-potash* process I formerly described in the *Journal of Anatomy and Physiology* (vol. xix., 1885, p. 385), along with a modification of Weigert's hæmatoxylene-copper stain for medullated nerve fibres lately published by me in the same periodical (vol. xxi., 1887, p. 444).

The former method is suited solely for naked eye observation. The main objection to Weigert's stain is, as mentioned by its author, that it cannot be adapted to cutting the tissue in the freezing microtome. The modification above referred to has been introduced with the view of overcoming this. The reaction of the nerve-medulla is quite as intense if not more so than that obtainable by the original procedure.

Could one unite the aniline-black stain of Sankey and Lewis for nerve cells with this modification of Weigert's stain for nerve fibres, little would remain to be desired for microscopic demonstration. The difficulty, however, is that the aniline-black dye will give its proper reaction only when the brain is perfectly fresh, whereas the hæmatoxylene will act upon the nerve-medulla only when it has been hardened in a chrome salt. I have, however, already managed to partially combine the two, and see no insuperable barrier to complete success.

The Callosal Fibres.—It is, I think, almost universally believed at the present day that the corpus callosum is a commissure; that anatomically it unites equivalent areas in the two cerebral hemispheres, and that physiologically it serves to bring these into functional harmony. Some years ago, when working at the pathology of the brain, I came upon certain appearances which tended to shake my belief in the commissural theory, and which led to an inquiry, part of the results of which is embodied in this paper. The appearances alluded to are to be seen in the brain of any mammal when it has been hardened in Müller's fluid, but best in those in which the organ is of large size, as in Man. It was in Man that I first noticed the appearance, but I have since then found that it exists in all the mammalian brains I have examined. The Müller's fluid, in the case of a large brain such as that of Man, must be injected from the main vessels at the base in order to insure that it will penetrate deeply and in sufficient quantity.

If such a brain, when completely hardened, be simply cut into a series of perpendicular transverse segments, each of about half an inch in thickness, the following can be readily seen with the naked eye or with a simple lens:—

Coming out of the corpus callosum at each side is *a large arched*

mass of fibres (see Pl. XIV. fig. 1), which leaving this body and continuous with it turns upwards, outwards, and downwards in the centrum ovale. The arcuate mass varies somewhat in shape at different parts of the brain. Thus, anteriorly it represents an almost complete semicircle, while posteriorly it becomes more pointed. The fibres entering into the composition of the arched mass subsequently pass into the inner and outer capsules. The greater bulk of them, however, enters the inner capsule, and in its anterior limb the capsule is almost entirely composed of them; while a considerable portion also seems to run into the outer capsule, constituting the inner of the two layers of which it consists. Their further course and attachments to underlying parts will be subsequently considered.

In a former paper (*Journal of Anatomy and Physiology*, vol. xix., 1885, p. 385) I have named this mass of fibres the "crossed callosal tract"; and as all my work since then has tended to fully bear out the view I at that time entertained of its significance, I propose still to adhere to this nomenclature.

In order to get at once to the gist of the arguments I intend using to explain the nature of this crossed callosal tract, I shall start with the postulate that it is mainly composed of callosal fibres which have arisen from the cortex, which have crossed in the corpus callosum, and which, instead of turning upwards to become attached to points in the opposite cortex corresponding with those from which they sprang, are now turning downwards into the two capsules to become subsequently united to the basal or other ganglia presently to be enumerated.

If it be true that the crossed callosal tract represents the fibres derived from the opposite cortex, which have passed over in the corpus callosum, and which are now turning down to the two capsules, the following data ought to admit of verification:—

1. The crossed callosal tract ought to be capable of being dissected out;
2. It ought to be co-extensive with the corpus callosum; and
3. It should be possible to trace the fibres microscopically as they turn downwards.

I shall consider each of these in order.

1. Foville long ago ("Traité complet de l'Anatomie, de la Physiologie, et de la Pathologie du Système Nerveux Cérébro-spinal," *Atlas*) showed that an arched ridge of fibres could be exposed by simple dissection turning downwards at each side of the corpus callosum, and figured appearances which, allowing for a certain amount of artistic embellishment, substantially represent what actually exists. (The author here exhibited a brain, previously hardened in Müller's fluid, in which this dissection had been made, and in which the arcuate mass of fibres was distinctly displayed. He further showed this arcuate mass in horizontal sections prepared by his gelatine-potash method, in which it was quite clearly mapped out. Its fibres had a more or less transverse direction, so that they contrasted with those coming from the cortex, and the outer border of the mass where they turn downwards was quite sharply differentiated. The fact that the arcuate mass is seen on horizontal section completely does away with the notion that Foville's dissection was artificial. In a series of horizontal sections the crest of the ridge was found to correspond in position with that in the dissection, the site of it being at a point considerably below the level of the cortex at the vertex.)

2. That the crossed callosal tract is co-extensive with the corpus callosum can be proved by dissecting it out, or by examining it in a horizontal gelatine-potash preparation.

3. To trace microscopically the fibres curving downwards from the corpus callosum is not such an easy matter as might be supposed, owing in great part to the fibres running in different planes between their points of origin and insertion.

Meynert has alleged that he could trace single fibres from the cortex of one side through the corpus callosum into the cortex of the opposite. For my own part I can hardly credit this statement, for, after having spent an immense deal of labour upon the subject, and working with methods far more refined than those employed by Meynert, it has never been my good fortune to follow a single axis cylinder from the one side to the other even in the smallest mammals. The fibres diverge and run so obliquely after crossing that I question if a section made in any one plane would suffice to expose their entire course.

If the brain be cut *perpendicularly in an oblique antero-posterior*

direction, however, the bundles of callosal nerve fibres can be traced from about the middle line continuously down to *the outer and inner capsules, instead of upwards to the cortex as generally asserted.* In Pl. XV. fig. 2 I have given an accurate drawing of a section of the human brain ($\times$10 diams.), stained and prepared by my modification of Weigert's method, and taken from a region corresponding to the front of the basal ganglia. The parts of the preparation included in the drawing are the tectorial part of the corpus callosum (*c.c.*), the crossed callosal tract (*c.c.t.*), the plexiform nucleus (*p.n.*),* the head of the caudate nucleus (*c.n.*), and the inner capsule (*i.c.*). The section from which the drawing was taken was made perpendicularly in the oblique antero-posterior direction just indicated. By so doing the continuity of a certain number of callosal fibres, as will be noticed, can be followed in a direction downwards, although it will be remarked that even here some of them (as at *s.c.f.*) have been obliquely divided.

From the drawing it is evident that the bulk of the fibres issuing from the side of the tectorial part (*c.c.*) of the corpus callosum sweep distinctly upwards, outwards, and downwards towards the inner capsule (*i.c.*). They are united in coarse bundles, and thus can be readily distinguished from those entering it (*v.c.f.* and *p.n.f.*), which, although in bundles, are less condensed, and which, moreover, spread out in a radiate or fan-shaped manner. When the brain is cut in a perpendicular *transverse* direction the continuity of these fibres cannot be seen, because they have been severed by lying at an angle to the plane of section. Hence it is, I believe, that they have remained so long unnoticed. In no case have I been able to see a single bundle of fibres run upwards after emerging. After having crossed, the whole mass seems to turn downwards to the capsules, and to form the greater part of their bulk.

In two late numbers of *Brain* (vols. viii. and ix.), Dr Beevor has taken exception to this view as originally enunciated by me in a communication to the Royal Society (*Proceedings*, No. 230, 1884), and in papers which I subsequently published in the *Journal of Anatomy and Physiology* (*loc. cit.*), in *Brain* (vol. viii., 1886, p. 145), and elsewhere. He says that in the marmoset he has been unable to see the fibres turning downwards in the manner I

* For description of this body see the sequel.

have described, and gives a drawing which he considers demonstrates that my view is wrong, and that the old idea of the fibres passing from cortex to cortex is correct.* He asserts that this drawing is not a diagrammatic scheme, but an actual representation of a preparation in his possession. He further states that he has made oblique sections as I had directed, but still has been unable to see what I described.

When I read this criticism, I felt certain of two things: first, that Dr Beevor had not examined preparations cut in the oblique direction I have recommended; and, secondly, that the drawing above referred to was not an actual representation of the preparation from which it was said to have been taken. I was convinced that what he had endeavoured to depict consisted in reality of the fibres *passing into* the corpus callosum, and that he had entirely failed to see, as had happened to others, those which were *issuing from it*, owing to his having cut the brain transversely instead of obliquely. In justice to Dr Beevor's statement, however, I resolved to see his preparations for myself, and to hear his explanation of them by word of mouth. I am constantly being reminded by so-called critics that they are still sceptical of my statements, and the most ardent are those who have never taken the trouble to examine my work, nor really to work at the subject for themselves. The matter is not one which can be settled in an offhand manner, but requires the most careful scrutiny. If it had been easy to demonstrate what I have recorded, it would long ago have been done.

My anticipations in regard to the basis on which Dr Beevor's criticism was founded were more than realised. I emphatically state that the drawing of the corpus callosum given in his critique in *Brain* (vol. ix.) is very far from being an actual representation of the preparation from which it was taken. The continuity of the fibres is not such as he depicts, for immediately at the outer margin of the corpus callosum there is a break in the preparation caused by a large number of fibres having been cut off abruptly, which is not represented in the drawing. The fibres so cut across constitute those I have described as turning downwards. They have been severed, because they do not lie in the same plane as those

* Ferrier, I find, has somewhat hastily reiterated this statement in the latest edition of his work on the *Functions of the Brain.*

entering the body. I further found that the oblique preparations he employed had been cut in an entirely wrong direction, in a direction which was calculated to divide the crossed callosal fibres, instead of rendering them more apparent. As I have elsewhere stated, he has not followed the directions I have so explicitly given in various of my published papers, and hence it is useless to argue the point. If he will harden the human brain, or, say, that of a sheep, by the method I have recommended, and cut this perpendicularly in an oblique antero-posterior direction, he will see what I have described. It is impossible, as I have already indicated, to trace individual axis-cylinders throughout their entire course, but the continuity of individual bundles between the corpus callosum and the capsules can be demonstrated with facility. The difficulty of tracing the course of the crossed callosal fibres rests in this, that *those which lie anteriorly after crossing run obliquely backwards, while those which lie posteriorly run obliquely forwards*, the point to which they all tend to converge being the knee of the inner capsule.

It consequently happens that in whatever plane the organ may be cut, the fibres will be divided at some point. In a completely transverse perpendicular section the crossed callosal fibres are usually divided, and being represented only by small fragments, are very apt to be overlooked in the dense mass of nerve-medulla lying in their neighbourhood.

The Cortical Plexuses.—Of late years a good deal has been written of the most interesting plexus of medullated fibres which exists in the cortex of the cerebellum and cerebrum, by Exner (*Sitzungsb. d. k. Akademie d. Wissensch.*, vol. lxxxiii. Ab. iii., Feb. 1881), Butzke (*Arch. f. Psychiatrie*, vol. iii., 1872), Gerlach (*Centralb. f. d. med. Wissensch.*, 1872, p. 273), Boll (*Arch. f. Psychiatrie*, vol. iv., 1874, p. 1), Rindfleisch (*M. Schultze's Arch. f. mik. Anat.*, vol. viii. p. 453), and others. It seems likely, as Hill suggests, that since the discovery of these fine cortical plexuses our whole notions of what are known as *nerve centres*, and of the communication that exists between nerve cells and fibres, will shortly be revolutionised. The plexuses to which I refer can be seen only when certain methods of staining are employed. Exner, who is generally regarded as having discovered the plexus in the cerebral cortex,

employed perosmic acid and ammonia, but since then the reagent used for the purpose of demonstrating it has almost exclusively been Weigert's hæmatoxylene dye previously referred to.* In the cortex of the cerebellum the plexus is probably densest, but it is present in all parts of the cerebral cortex as well.

Continuity of Cortical Plexus with that in White Matter.—What I would specially wish to direct attention to at present, however, is that this plexus not only prevails in the cortical grey matter, but that it appears to intertwine itself round the nerve fibres throughout a great part of the white. The large medullated nerve fibres from the cortex run into the white matter, but almost immediately become surrounded by a dense padding or casing of this nerve network. At first it might be supposed to be simply connective tissue, and it has in bygone times been always regarded, when indistinctly seen by less favourable means of demonstration, simply as the branching neuroglia. The plexus I refer to, however, as pervading the white matter of the brain is a true nerve structure, and that which is found in the cortex of the cerebrum and cerebellum is an extension or outcrop of this. The appearance presented by it a short way within the cortical grey matter, of the motor region, towards the vertex is shown in Pl. XV. fig. 3. The large medullated trunks (*a.*, *a.*) are seen coming down from the grey cortex, but shortly after penetrating into the white matter of the centrum ovale they become encased, as it were, in a dense and complex mass of medullated fibres (*d.*). Between its fibres is the granular neuroglia (*c.*), which seems to fill all the meshes formed by it.

The Plexiform Nucleus.—A similar medullated plexus also exists in certain of the ganglia, such as the thalamus and lenticular nucleus. In the former it is in a high state of development, but there is one part of the brain in which it reaches even a higher grade of complexity. I refer to a little comma-shaped body (fig. 2, *p.n.*) which lies in the angle constituted by the under aspect of the tectorial part of the corpus callosum and the upper surface of the caudate nucleus. This body, whose presence I do not remember having seen referred to, is one mass of a dense and complicated nerve plexus, and, so far as I am able to discover, is without nerve cells. It

* Since this paper was read various modifications of Exner's method have been introduced by Pal of Vienna and others.

is contiguous to the caudate nucleus below, but the tissue of the one is separated from that of the other by a sharp line of demarcation. It passes for a short way underneath the corpus callosum, and at its lower extremity posteriorly, seems to be united with the tænia semicircularis. Its fibres are directly continuous with the fibres of the plexus in the white matter just referred to. It is most developed anteriorly in the region of the head of the caudate nucleus.

In Pl. XIV. fig. 4 I have given a drawing of the plexus constituting this body as it appears when magnified about 350 diameters. The part from which the drawing was taken was immediately adjacent to the inner capsule at the point *x.* in fig. 2. The plexiform nucleus (*p.n.*, *p.n.*, *p.n.*,) is seen to the right; a few of the fibres of the inner capsule (*i.c.*) to the left. It will be noticed that the main bulk of the body is made up of an intertwining felt-work of nerve fibres. They stain deeply with Weigert's copper-hæmatoxylene dye, and between them, as in other regions of the brain, a quantity of granular neuroglia is interposed. It is only lately that I have made out the true nature of this body, and on account of its structure I propose to name it the *plexiform nucleus.*

Points of Origin of the Callosal Fibres.—The most of the callosal fibres which come down from the *vertex* appear to run *directly* into the corpus callosum. Their usual appearance and direction are represented in fig. 2 (*v.c.f.*, *v.c.f.*). In passing downwards they interlace with those leaving the corpus callosum, and which are turning downwards to the two capsules.

Those, however, which are derived from the lower third or half of the cortex between the Sylvian fossa and the great longitudinal fissure (fig. 2, *p.n.f.*, *p.n.f.*) do not appear to run directly into the corpus callosum, but pass first of all into the *plexiform nucleus* just described. Shortly after issuing from the grey matter they become united into loose bundles which penetrate through the fibres of the crossed callosal tract, and which seem to lose themselves within the *plexiform nucleus* by breaking up into its reticular network. From this reticular network fresh fibres appear to arise, and to enter the corpus callosum. In all probability, these turn downwards on the opposite side into the two capsules as fibres of the crossed callosal tract. This *plexiform nucleus* would thus possibly represent

a meeting-point for many of the callosal fibres before they proceed to cross, the individual fibres losing their identity within it by splitting into an anastomising common network, from which again fresh fibres appear to arise and travel across the corpus callosum to the opposite side.

The fibres which enter this body are chiefly derived from the motor centres which in Man have been found to preside over the muscles of the tongue and face, that is to say, the lower parts of the ascending frontal and parietal convolutions, and it is conceivable that the function of the plexus contained in it is to correlate and associate their action.

Destinations of the Callosal Fibres.—After passing into the inner and outer capsules, the arched callosal fibres just described become united into dense bundles. A very large proportion of them lose themselves in the *thalamus opticus.* The excessively fibrous appearance which the thalamus presents is due to these fibres passing into it. They probably break up into a network, in the meshes of which the nerve cells are intercalated.

Are these nerve cells directly connected with the nerve fibres entering the ganglion, or is the network referred to intermediate? It seems more likely that the union is not direct, but that a plexus intervenes between the two, and that this plexus simply surrounds the nerve cells. I am even not at all convinced that the processes of the nerve cells are in all cases directly connected with the plexus. May not nerve energy generated in cells exert its influence upon nerve fibres in ways other than by direct continuity? Is it not possible that it may be transferred to the coils of a dense plexus through the liquid and neuroglia which fill up the intervals in the tissue, and that, conversely, peripheral stimuli may thus be conveyed to a nerve cell? I think this is at least conceivable, and the idea has of late been entertained by several physiologists, both in this country and abroad.

Few, if any, callosal fibres end in the *caudate nucleus*, and, curiously, as if supporting this observation, the plexus in this ganglion seems to be very scanty. .The *lenticular nucleus* may receive through the striæ medullares a considerable number, and probably some of the fibres connected with the *red nucleus* may be also callosal. A large number appear to end in the *pons and medulla*

oblongata, while there is a probability of certain of them even extending down to the *spinal cord.*

The Direct Fibres.—This paper, however, is concerned with the connections between the cerebral cortex and the centres lower down, and as yet I have referred to only one set, namely, those which are callosal and which cross from the opposite side. There are others, of course, which run down directly, and of these the motor fibres are among the most important. These direct motor fibres lie to the *outside* of the crossed callosal tract (fig. 2, *d.f.*), and, like it, bend somewhat outwards in circumventing the ventricle. Those derived from the *marginal gyrus* seem, at least in the sheep, to lie in very close apposition with the fibres of the crossed callosal tract. In Man I calculate that about one-third of the fibres entering the anterior two-thirds of the posterior limb of the inner capsule are direct, while the remainder are crossed callosal.

From experiments made upon the cortex, it is evident that these direct fibres are derived from a wide area, one, indeed, so wide that it comes to be a question how it is that they are so few in number when they decussate in the medulla and become connected directly, or through the intermediation of the spinal cord, with the peripheral nerves. The notion at present held by most physiologists is that from the motor cells of the cortex fibres issue which are continuously prolonged downwards to the spinal cord. But if we consider the matter for a moment, it is evident that they must have been much reduced in bulk by the time they have reached the medulla, and that the pyramidal fibres of the medulla or cord cannot represent the whole of the motor fibres derived from the motor area. How, then, is this sudden falling off to be accounted for? My present conviction is that the direct continuity of the process of a ganglion motor cell in the cortex with the pyramidal tracts of the spinal cord is a myth. I am strongly inclined to believe that, just as in the case of many of the callosal fibres, the motor fibres break up into a plexus, from which again fresh fibres, those which enter the pyramidal tracts, take their origin. When the pyramidal tracts in the cord are affected by secondary degeneration, they are mapped out with the utmost precision, and the degeneration never overlaps them. Can the same be said of the degeneration further up in the centrum ovale? I do not think that

it can. There is always more or less diffusion of the tract immediately and for some distance below the point of lesion, if that be cortical, and the explanation I think is to be found in the interposition of this plexus. The plexus is a means of reduction and association, a means by which the action of the many fibres coming from a particular cortical area may be combined and correlated in the few.

But the direct fibres entering the inner capsule are not all motor in their function. There are many other bands which enter it and whose function varies. Thus there is a large contingent of fibres which passes into its posterior limb from the parieto-occipital region, and whose function, there cannot be much doubt, is sensory. It has been shown, over and over again, that when it is destroyed hemianæsthesia results. Then there is a large band of fibres which comes from the prefrontal region, and which enters the anterior limb on its way back to the anterior nucleus of the thalamus, to which it becomes attached. The geniculate bodies and the pulvinar finally are connected by direct bands with the occipital region.

Plexus most abundant in Man.—One of the main differences which exist between the brain of Man and that of the lower mammalian types consists in the disproportional size of the white and grey matters. In Man the white matter is relatively more abundant than in the brain of any other mammal I have examined, and the lower we go in the scale the greater the disproportion appears to be.

Now, the cause of this seems mainly to reside in the fact that this intertwining plexus which ramifies through the whole centrum ovale is vastly more abundant in Man than in the lower animals, and hence, probably, the superiority of the human brain as an instrument of association may be accounted for.

Connections of Thalamus Opticus.—I have said that a large number of callosal fibres pass into the thalamus opticus. They lose themselves in it, apparently by becoming connected with a dense plexus. In conclusion, let me ask the question whether there are any fibres which leave the thalamus, and, if so, where they go to? Do fibres descend from the thalamus into the cerebral peduncle, ultimately to enter the spinal cord? I am becoming daily more and more convinced that, if such do exist, they must be small in

number. Certain bands of fibres which are not callosal, no doubt enter the thalamus, but these seem to connect it with parts of the *cortex* which experiment has shown are associated with definite and well-located functions. Thus there are the three so-called *peduncles* of the thalamus, uniting it respectively with the prefrontal region, with the nucleus amygdalaris in the temporo-sphenoidal lobe, and with the hippocampus major. But these differ entirely in their nature from the fibres which are supposed to leave the thalamus and to pass downwards with the other descending cerebral tracts, and it seems to me that the latter, if they do exist, must be in small quantity.

How, then, is the thalamus connected peripherally, and what is its use? As yet any statement on this subject must necessarily be largely conjectural, but, all things considered, there is reason for at least supposing that this ganglion is largely concerned with the *education* of the brain *through the optic nerves and corpus callosum.* Of all the nerves in the body the optics are those by which the brain is mainly educated. They are in constant use, imperceptibly opening up the cortical grey matter to impressions made upon the periphery by light vibrations. What is the connecting link between the peripheral retina and the central cortex? The visual centre is said to be located in the occipital lobe, but the optic must subserve a far wider function in educating other parts of the cortex as well as this small area? How is it that the motor centres, for instance, are educated to a particular complicated act, purely through the sense of sight? What is the mechanism by which a sudden visual impression, accompanied by a sense of danger, will serve to throw the body instantaneously into a complicated attitude of defence? This introduces far too wide a subject of discussion to take up at present; but it seems to me likely that the callosal fibres entering the thalamus constitute the substratum by which these acts are accomplished; that they are, in fact, the means by which the opposite side of the brain is educated through vision.

Where the corpus callosum has been destroyed in infancy, imbecility seems to have been the invariable result. There are certain records of congenital deficiencies of this body which have been unaccompanied by any symptoms of note, more especially one

described by Eichler (*Arch. f. Psychiat.*, vol. viii. p. 355). It seems, however, that in these cases we have to do not with a deficiency in the actual callosal fibres, but with a malformation by which they have failed to decussate in the middle line, just as so frequently happens in the anterior pyramids.

On the supposition that the thalamus subserves the purpose of concentrating the fibres which educate the higher centres through the optic, it can readily be understood how, if it were destroyed in adult life, no very evident symptoms might follow. It has already to a great extent subserved its purpose. The higher centres have been educated, and are capable of discharging their functions apart from the channels through which that education has been imparted. It has played its part, so to speak, in infancy and youth, and may now in a manner be considered as functionally inert. The impressions made upon the cerebral cortex through it are quite possibly recalled by a perfectly different set of channels; for I do not see why in the internal economy of the brain there may not be paths for *educating* the higher centres through vision, hearing, touch, and so on, and a whole set of other paths by which the results of this education may be brought into action. If such be the case—and I advance this with all due caution—the callosal system of fibres might be regarded as the great *educating system;* while the direct bundles to which I have adverted would constitute the means of adapting this education to a utilitarian purpose.

In the case of those born blind, the education of the cortex would of course be carried on through other channels, namely, through those of the remaining special senses. The nuclei of the nerves connected with these are situated in great part in the pons, medulla oblongata, and spinal cord, and these again, as already mentioned, appear to be extensively united to crossed callosal fibres which have descended in the inner capsule. By the agency of these callosal fibres they are placed in communication with the cerebral cortex on the opposite side of the body. The same mechanism for educating the cortex in fact prevails here as in the case of the optic; that is to say, there is, firstly, the peripheral nerve to receive the impression; secondly, an intermediate nucleus to which the nerve is bound; and, thirdly, a system of fibres (callosal) by means of

which this nucleus is placed in continuity with the opposite cerebral cortex.

The arrangement seems a probable one; all the most important motor and sensory channels seem to cross the middle line at some point. We see this exemplified in its most simple form in the ordinary ascending and descending paths in the spinal cord; and there seems good reason for believing that the same type of construction prevails higher up. The callosal fibres would thus represent the decussation of the multiform tracts which do not cross in any of the commissures or decussations lower down. The greater number of them are probably not motor nor purely sensory in their function, but in great part *educational.* They are, in fact, the means of impressing the opposite cortical centres with the stimuli that have been made upon the intermediate centres lower down.

Explanation of Plates XIV., XV.

Fig. 1. Perpendicular opaque transverse section through the region of the infundibulum of human brain hardened by injecting Müller's fluid. Natural size. In the centre is the corpus callosum, and at each side of it, turning upwards, outwards, and downwards in the centrum ovale to the two capsules, is the arched mass of fibres which I have named the *crossed callosal tract.* The fibres *coming in* to the corpus callosum from the cortex of the vertex and elsewhere are seen interlacing with the fibres of this arched mass. The drawing was made with the greatest care, and may be taken as being as nearly as possible a *facsimile* of the preparation from which it was copied. The brain, after being thoroughly hardened, was simply cut into segments about half an inch thick, whose surfaces were polished in the freezing microtome. Nothing further was done to it. The drawing represents the surface of one of these segments.

Fig. 2. Perpendicular oblique antero-posterior section of human brain through the corpus callosum and *crossed callosal tract.* $\times$ 10–20 diams. Stained by the author's modification of Weigert's copper-hæmatoxylene process. *c.c.*, corpus callosum; *c.c.t.*, crossed callosal tract; *v.c.f.*, *v.c.f.*, callosal fibres from the vertex; *p.n.f.*, *p.n.f.*, same, from cortex lower down, running towards the plexiform nucleus (*p.n.*); *d.f.*, direct cortical fibres lying outside the crossed callosal tract, and running down to the inner capsule (*i.c.*); *s.c.f.*, severed callosal fibres of the crossed callosal tract; *c.n.*, caudate nucleus; *x.*, part of preparation from which figure 3 was drawn.

Fig. 3. Portion of white matter immediately under the grey mantle of the cortex at the vertex in the motor region. Stained as in figure 2. $\times$ 300 diams. *a.*, *a.*, bundles of large medullated fibres passing downwards from the grey matter; *b.*, a clot in a blood-vessel; *c.*, the granular neuroglia; *d.*, the felt-like plexus of nerve fibres surrounding the straight bundles coming from the cortex.

Fig. 4. Part of plexiform nucleus and adjacent inner capsule, taken from the preparation depicted in figure 2 at point *x*. × 350 diams. *p.n.*, *p.n.*, *p.n.*, outer border of plexiform nucleus ; *i.c.f.*, *i.c.f.*, descending fibres of inner capsule ; *u.c.f.*, *u.c.f.*, cortical fibres derived from the grey matter shortly above the Sylvian fossa, and which apparently end in the nerve network of the plexiform nucleus.

Fig. 4.

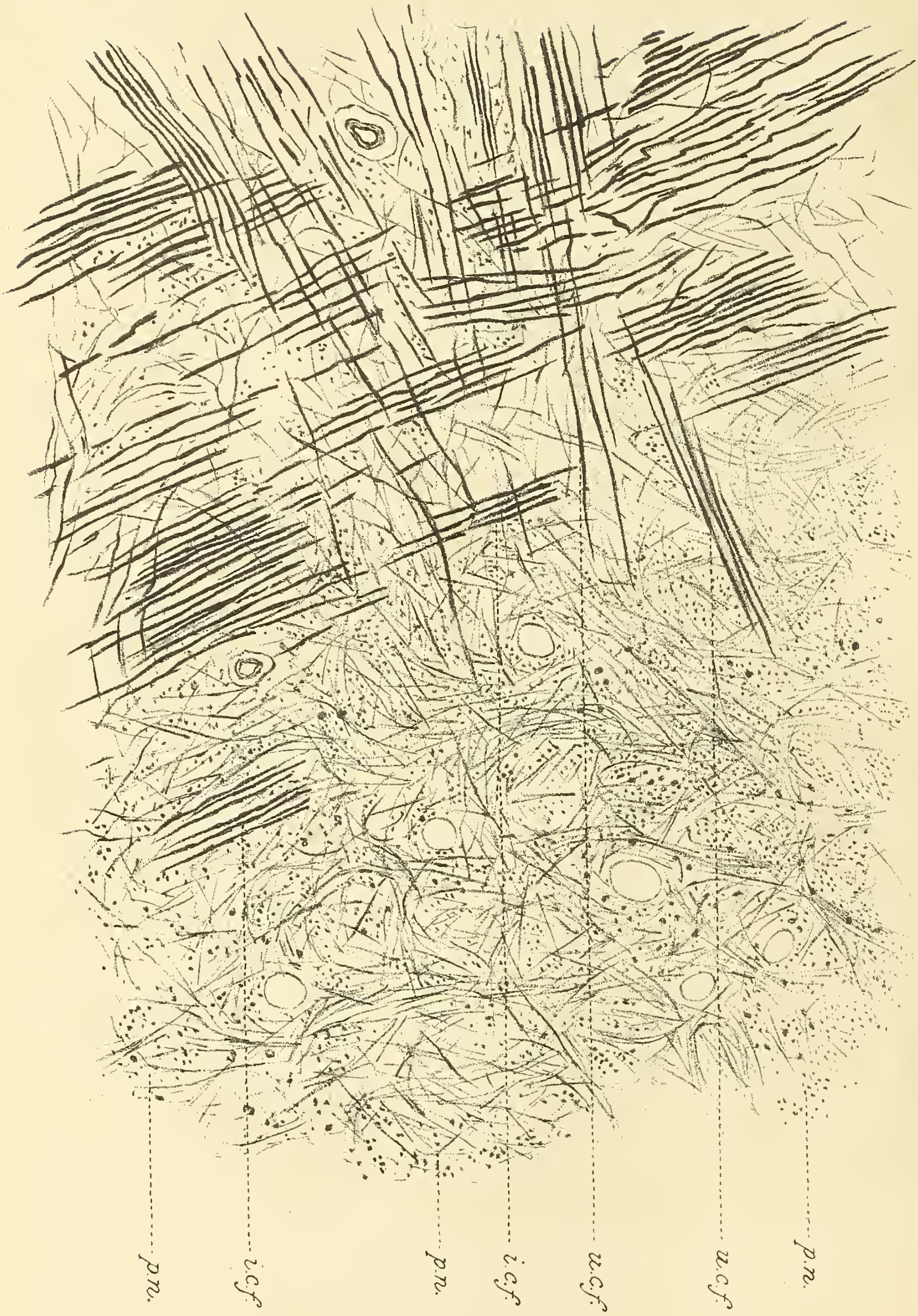
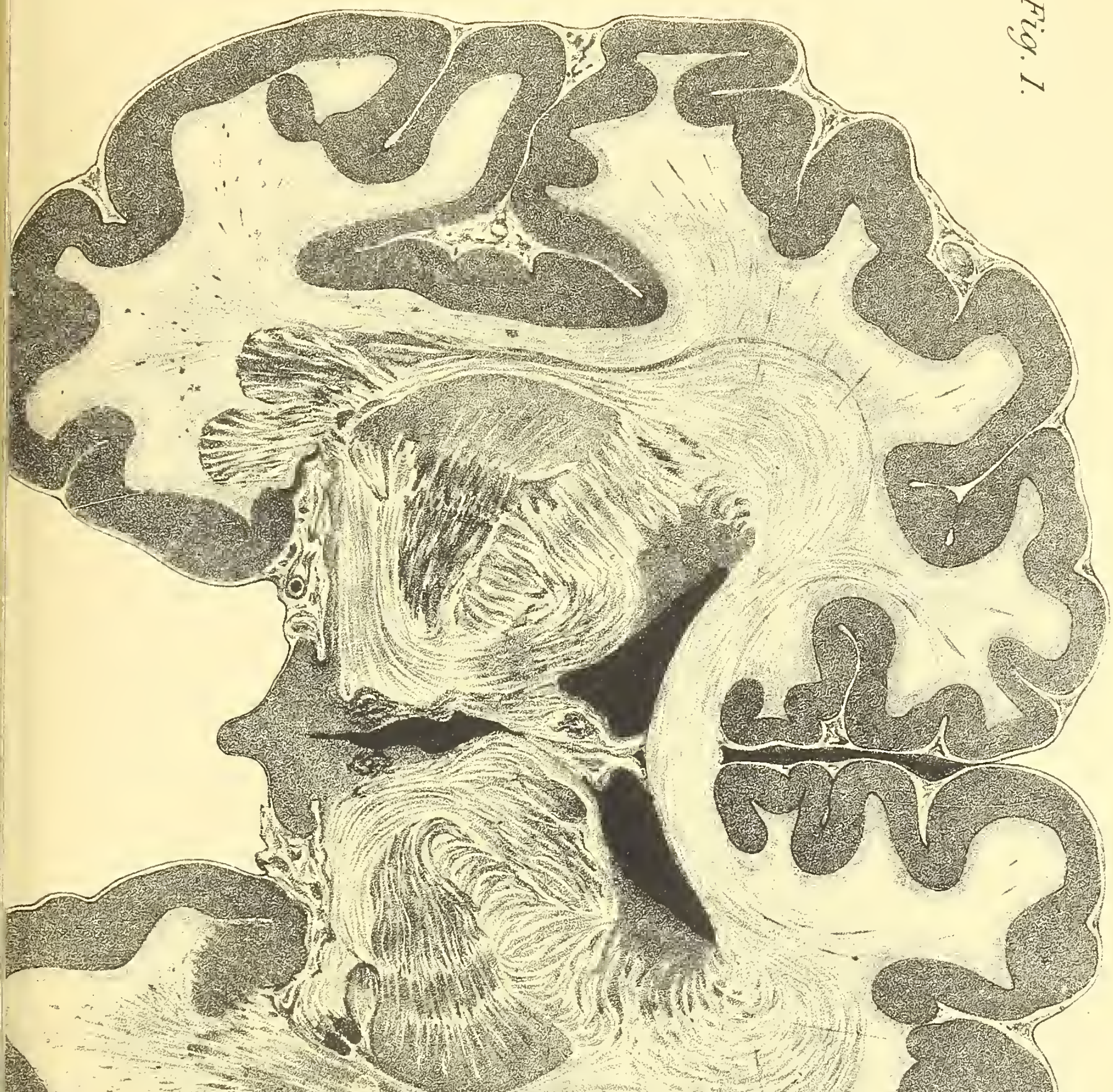


Fig. I.



F. Huth, Lith^r Edin^r

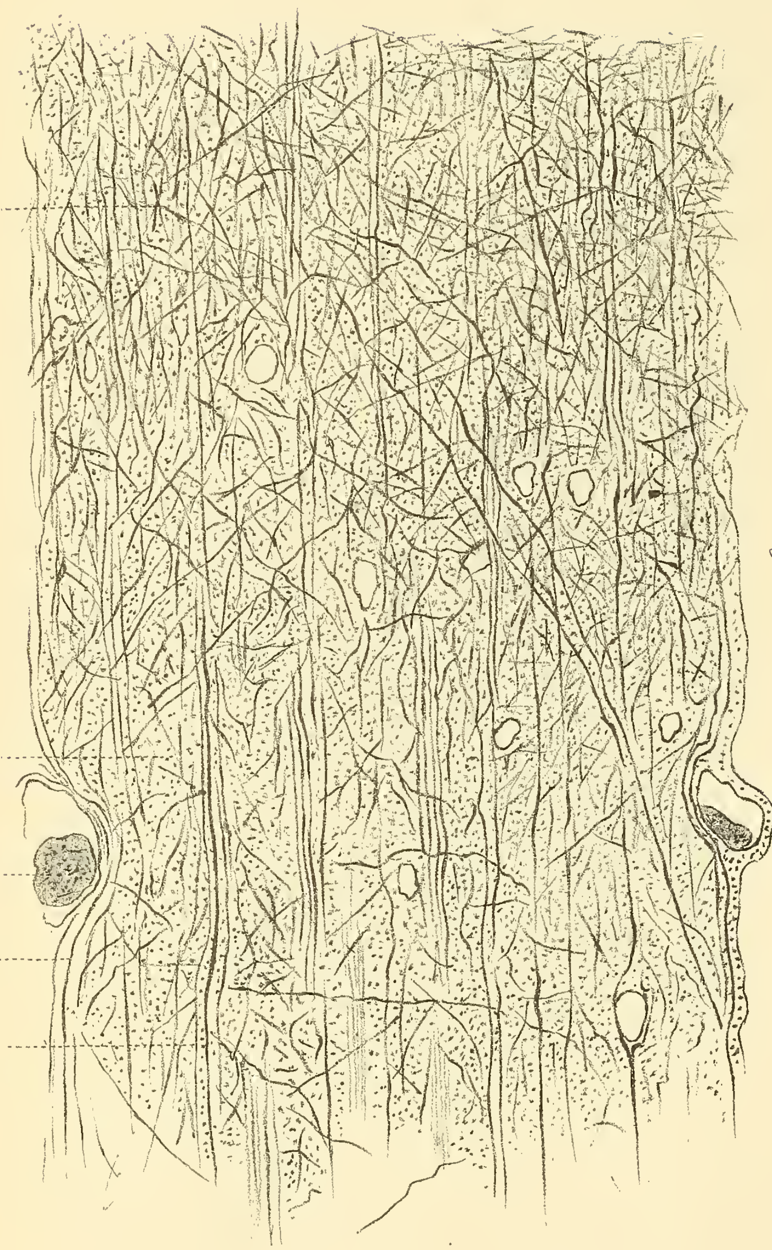


Fig. 3.

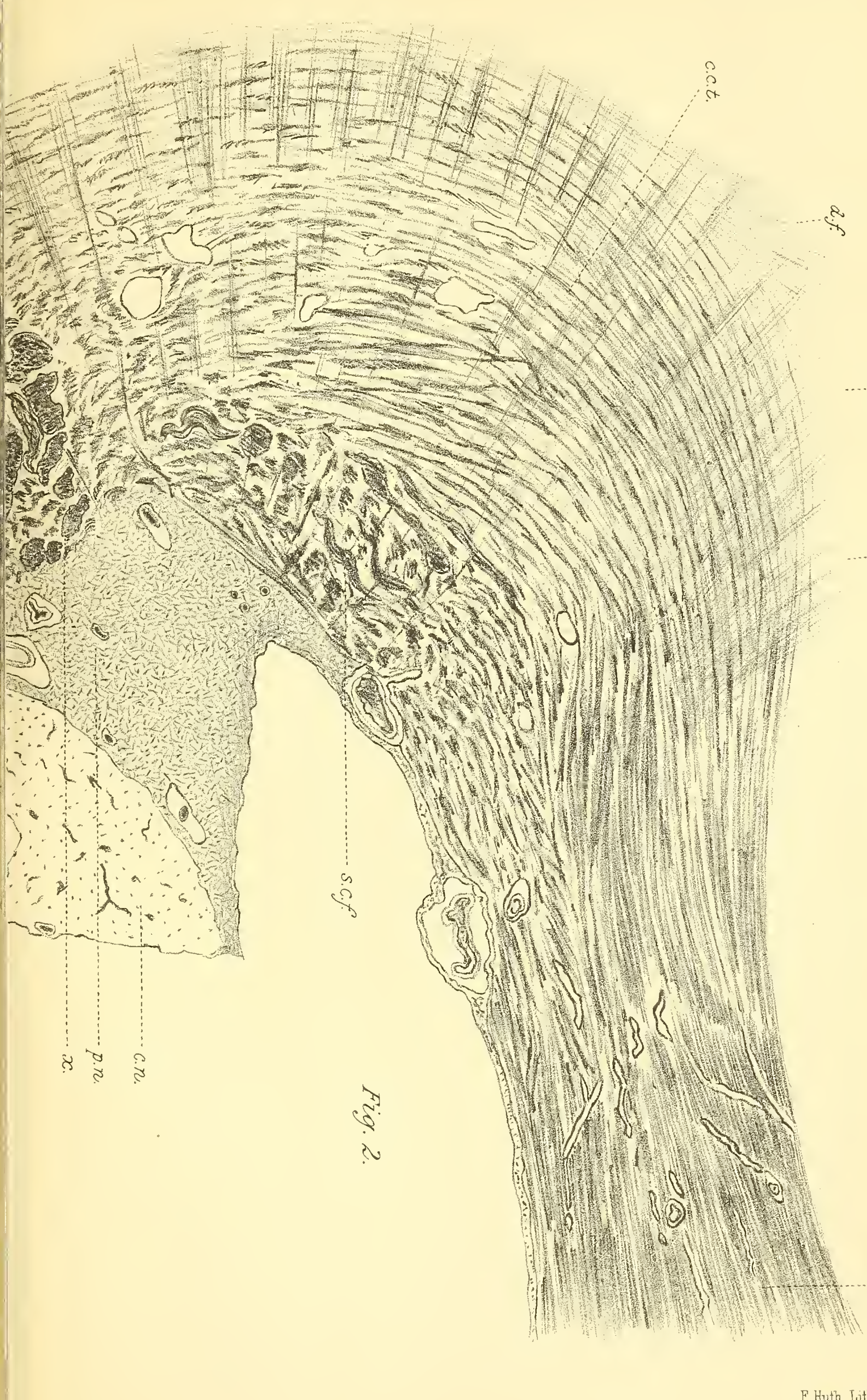


Fig. 2.

F. Huth, Lith^r Edin^r

Vol. XIV, Pl. XV.

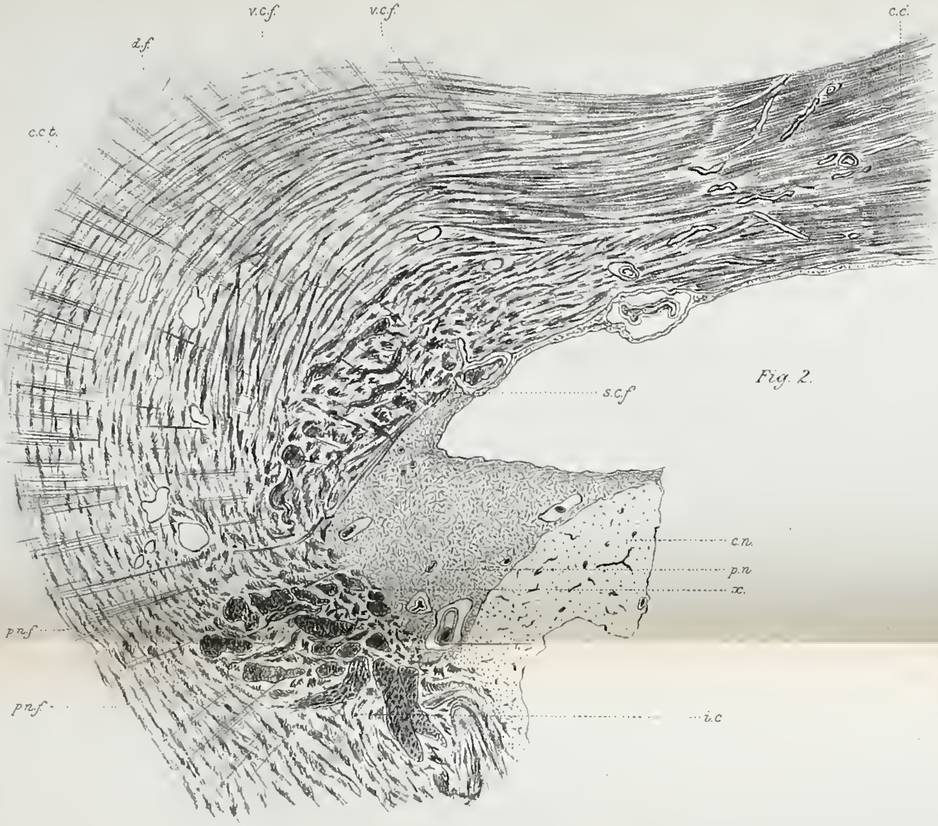
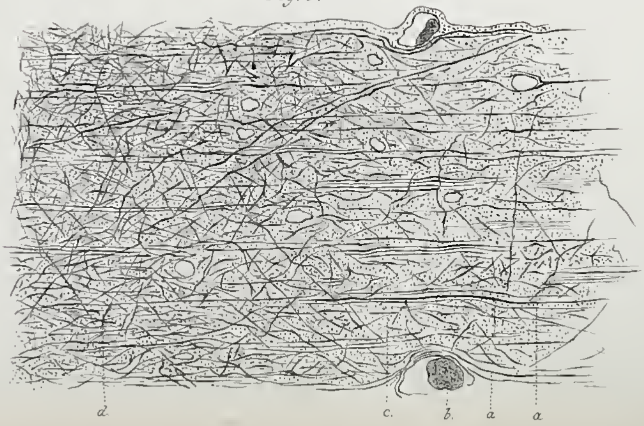


Fig. 2.

Fig. 3.



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OBITUARY NOTICES OF FELLOWS DECEASED.

(*Separate paging at end of Volume.*)

OBITUARY NOTICES.

OBITUARY NOTICES.

Charles Darwin. By Professor Cossar Ewart.

Charles Robert Darwin, who was the son of Dr. R. W. Darwin, and grandson of the distinguished Dr. Erasmus Darwin, was born at Shrewsbury on February 12, 1809. His mother was a daughter of Josiah Wedgwood. Of his early life little is at present known. For a time he attended the school at Shrewsbury, of which Dr. Butler, afterwards Bishop of Lichfield, was master. It having been decided that he should study medicine, he was at the age of sixteen (1825) sent to the University of Edinburgh. After two sessions at Edinburgh, he gave up the study of medicine, and entered Christ's College, Cambridge, to study for the Church. While in Edinburgh Mr. Darwin seems to have directed his attention chiefly to botany and natural history. During his second session (1826–27) he became a member of the University Plinean Society, and, as the MS. records testify, took part in its discussions, and read before it at least two papers. One of these papers referred to the ova of *Flustra*, the other pointed out *that the small black globular body hitherto mistaken for the Fucus lor was in reality the ovum of the Pontobdella muricata.* These papers probably contained the results of Mr. Darwin's earliest scientific observations. At a subsequent meeting of the Society he presented "specimens of *Pontobdella muricata* ova and young."

After the usual course at Cambridge, Mr. Darwin obtained the B.A. degree in 1831, and in 1837 he was promoted to the degree of M.A. Already an entomologist, on entering Cambridge he soon became acquainted with the distinguished naturalist Professor Henslow. Judging from letters published, Professor Henslow seems

more than any other to have been instrumental in leading Mr. Darwin to take a deep interest in natural science; and not only to have ably assisted and advised him in his pursuits, but to have gained his life-long admiration and esteem. Further, we are indebted to Professor Henslow for urging Mr. Darwin (notwithstanding the objections offered that it might unsettle him for the Church) to accompany Captain Fitzroy in the "Beagle,"—a voyage in which we cannot but feel great interest, not only because of the enormous work Mr. Darwin accomplished single-handed, but more especially because it was during this voyage that the great generalisations occurred to him which will ever be associated with his name, and which mark a new epoch in biology, and have had a more profound influence on science than any other doctrines ever published.

Three years after returning from his voyage round the world, Mr. Darwin married, and in 1842 settled at Down, in Kent, where he remained living the quiet life of a country gentleman until his death on the 19th of April last.

Mr. Darwin was elected an Honorary Fellow of the Society in 1865.

Of Mr. Darwin's work, the influence it has already had, and the influence it is likely to have in time to come, it is almost impossible to form any estimate, and still more difficult is it for us to realise his personal character, and the loss we have sustained in his death; for however great he was as a worker, he was still greater as a man. We have only to be reminded of the wonderful manifestations of reverence and regard which followed the announcement of his death, to understand how universal has been his influence, and how keenly his work has been everywhere appreciated. As has been well said, in the "memorial notices," his wholly irreparable loss is "not merely because of his wonderfully genial, simple, and generous nature, his cheerful and animated conversation, and the infinite variety and accuracy of his information, but because the more one knew of him, the more he seemed the incorporated ideal of a man of science;" and that it was not his great reasoning powers, vast knowledge, and tenacious industry "which impressed those who were admitted to his intimacy with involuntary veneration, but a certain intense almost passionate honesty by which all his thoughts and actions were irradiated as by a central fire;"

and again, that his "character was chiefly marked by a certain grand and cheerful simplicity, strangely and beautifully united with a deep and thoughtful wisdom, which, together with his illimitable kindness to others, and complete forgetfulness of himself, made a combination as loveable as it was venerable."

When we consider Mr. Darwin's work, we are led to regard him as one of the most fortunate and successful observers of natural phenomena, and as the greatest generaliser in the whole history of biology; and further, we are impressed with the great influence his generalisations have had on all other sciences.

What, in a few words, may be said to be Mr. Darwin's great work? It is not that he first propounded the theory of *evolution*, nor so much that, taking into consideration heredity, the struggle for existence, and the survival of the fittest, he hit upon the idea of *natural selection*, as that by undertaking elaborate investigations, by collecting facts from every possible source, and by pondering over and testing his conclusions again and again, he was able, after many years of patient industry, to publish an all but complete *proof* of evolution. He has thus not only increased our knowledge, but, by establishing a new principle, has completely revolutionised biology, introduced order where there was confusion, and laid new foundations on which naturalists are raising a fair and comely edifice, which will form the best and most lasting monument of the great philosopher of the nineteenth century.

So familiar are we all with Mr. Darwin's writings, that it is scarcely necessary to do more than mention some of the more important ones. First of all, one naturally thinks of that mine of wealth to the naturalist, the *Origin of Species*, in which we have condensed into an exceedingly small compass facts enough for a dozen volumes; yet notwithstanding the great condensation manifested throughout this book, the reasoning is evident from beginning to end, and the conclusions stand unassailable. It reads as if it were the epitome of a whole series of works which the author had intended to write, and for which material had been collected, rather than as an introduction; an epitome, however, so complete and suggestive in itself that, like a picked army, it was able to fight its way so effectively, that it was found to be practically unnecessary to fall back upon the vast

reserves which had been accumulated in order to support by detailed evidence the new doctrines. Hence, after publishing *The Variations of Plants and Animals under Domestication*, Mr. Darwin was again able to turn to Nature, not so much now for evidence of his theory, as by applying the principle of natural selection to point out how hitherto obscure problems might be explained.

In the *Variation of Animals and Plants*, and in the *Expression of the Emotions in Man and Animals*, we have further evidence of Mr. Darwin's enormous power of work, his faculty for collecting and arranging facts, and of the remarkable ability he possessed of drawing from them conclusions which indicated a wonderful insight into the secrets of nature. Further, in all of these works, as also in the *Origin of Species*, we have numerous observations of great importance and interest, which mark out Mr. Darwin at once as an able and careful investigator; but his fitness for pure zoological work is still more evident when we turn to the *Naturalist's Voyage Round the World*, and to the *Monographs on the Cirripedia.* Those familiar with the elaborate memoirs on the *Cirripedia*, must feel that Mr. Darwin was as capable of prosecuting purely morphological work as he was in performing physiological experiments, or of working out philosophical problems, and that although his zoological investigations are thrown into the background by his profound generalisations, they are of themselves of sufficient importance to entitle him to rank with the greatest biologists of any age.

What has been said of Mr. Darwin as a zoologist, may almost with equal propriety be said of him as a botanist and geologist. To quote again from the "memorial notices:"—"It is not too much to say that each of his botanical investigations, taken on its own merits, would alone have made the reputation of any ordinary botanist." Most of his investigations on plants were communicated to the Linnean Society, and then published in a collected form. A volume on *The Effects of Cross and Self-Fertilisation in the Vegetable Kingdom*, was published in 1876, and in the following year appeared the results of his work *On the Different Forms of Flowers on Plants of the same Species;* and in addition to these we have the memoirs *On the Various Contrivances by which Orchids are Fertilised by Insects; The Movements and Habits of Climbing*

Plants; and also the well-known treatise on *Insectivorous Plants.* We perhaps learn best the influence of Mr. Darwin's work on botanical science when we compare the ideas held as to the distribution of plants before and after the publication of the *Origin of Species.* Previously, it was generally believed that the different species and genera were special creations, and that the regions in which the same forms occurred being similar, had led to the creation of similar plants. This theory entirely failed to account for the appearance of similar plants in regions which had nothing in common in their physical conditions, and for their absence from places where the conditions were similar; whereas, as pointed out by Sir Joseph Hooker, by adopting Mr. Darwin's theory, "The theory of the modification of species after migration and isolation, their appearance in distant localities is only a question of time and changed physical conditions."

Mr. Darwin's geological work was chiefly the outcome of his voyage in the "Beagle." The most important of these is the masterly treatise *On the Structure and Distribution of Coral Reefs.* As with zoology and botany, however, his generalisations have had more influence than his special investigations. About the time when advanced geologists were beginning to feel that the old notions about fossils utterly failed to account for the distribution of organisms in the rocks, they were startled with the announcement of the theory of natural selection, and soon deeply impressed with the fact insisted on by Mr. Darwin, that the geological record was still very imperfect. Just as this theory has hurried on by leaps and bounds the study of embryology, so it has given a mighty impulse to palæontology. Having no longer to battle over what is, or what is not, a species, palæontologists are now vieing with embryologists in working out the ancestral history of organisms. The work of Professor Marsh alone amply testifies as to the success of these investigations. Not the least important of Mr. Darwin's works, from a geological point of view, is his treatise on *Vegetable Mould and Earthworms.* A paper "On the Formation of Mould" was read at the Geological Society in 1840. After more than forty years, during which period he made numerous additional observations and experiments, his book on *Earthworms* made its appearance—this, with the exception of two papers, read

before the Linnean Society shortly before his death, being his last work.

We might now indicate what influence Mr. Darwin has had on mental and other sciences: how that, through his general nobility of character, and his moral attributes rising pre-eminently above his intellect, he has been able to effect the greatest revolution of modern times without creating more than a passing show of strife and bitterness: and how all his work was accomplished under physical difficulties which an ordinary man would have considered excuse enough to regard himself as a confirmed and helpless invalid; but feeling intensely how difficult it is to express in words what one feels regarding Mr. Darwin, we shall refrain from saying more. Those who knew the chaotic condition to which Biology had been reduced before the appearance of the *Origin of Species* in the memorable year of 1859, and who have had the opportunity of observing order take the place of confusion, and light that of darkness, can best testify to the mighty influence of Mr. Darwin and to the loss the cause of science has sustained in his death. As we lament our loss, let us however remember that, in one sense, the hero so many of us worshipped is still with us, and that he lived to see his great life-work completed and justly appreciated in all parts of the civilised world. J. Cossar Ewart.

EMILE PLANTAMOUR. By the Astronomer Royal for Scotland.

On September 7th of the present year, at the age of 67, died our Foreign Associate, Emile Plantamour, director of the Observatory of Geneva, and professor of both astronomy and physical geography in the university of the same city.

Victim at last to a sudden accession of consumptive disease, he died in full possession of his admirable mental faculties, and as universally regretted as he had lived generally respected, not only in his own, but in every other country where science is known and civilisation appreciated; for well had he exhibited throughout his whole career how much of kindly goodness, as well as intellectual ability, does so often characterise those who are snatched out of this world immaturely by that insatiable malady of the lungs.

Born on the 14th of May 1815 in Geneva—a year after his little father-state had escaped from its temporary subjection to the first Bonapartist empire of France, and had joined the Helvetic Confederacy—Emile Plantamour's commencing epoch was that of young Switzerland, and he ultimately became as excellent a representative as could be found anywhere of that peculiar yet admirable microcosm of a republic, whose strict observance of law and order the red agitators of Paris can by no means understand; and even the United States of North America, republicans and democrats alike, do not altogether comprehend how it can continue to exist so anomalously to them—"a republic without a president!" And yet it not only exists, but lasts and grows, produces wealthy families too, capable, as with the Plantamours, of educating themselves up to the highest pitch of usefulness to their State, without seeking any help from others beyond the use of the self-supporting institutions in that case already made and provided.

But just as the school, or "the old college," wherein the young Plantamour spent the earliest of his hard-working years of learning as a boy, was of that staid and solid character that might be expected in an institution founded by Calvin, soon after the Reformation, so the comfortable private means of the older Plantamour's, like that of so many other Genevan families, had been attained in a manner worthily corresponding thereto. For not by manufactures nor by commerce, still less by speculations or bubble companies, were those tidy little Alpine accumulations obtained, but by the magnificent moral control of the progenitors of the family and their successors, one and all determining to live, though put to any straits for a time, on half only of their yearly income, leaving the other half to grow at compound interest.

Emile Plantamour himself was still too young to think much of these things while at Calvin's school; but by the time he had passed through that institution, also through the higher academy of Hofroyl, and then the classes of the Genevan University, he was called on to choose his future walk in life as a working member of the busy republican mountain hive. So he elected to be an astronomer—a Helvetian astronomer of course. But what is there peculiar in that prefix? This mainly, that while the Helvetian confederation forms so small a patch of country, surrounded by

great empires, it yet possesses more diversity of populations than any of them; so that but for the mysterious cord of Helvetic unity, its French cantons would be ever fighting against the German, and the Italian against both. Wherefore, all the great and good citizens of that up and down mountainous land seem ever to have a most difficult problem of their own to work at, viz., how to keep up the vigour and elasticity, the frictional polish and emulating fervour of those several competing nationalities, while inducing them, nevertheless, to do all peacefully, and voluntarily to contribute each their best characteristics, so as to raise the united name of a Swiss republican for virtue and education, valour, prudence, and understanding, above that of all collections of men, if possible, ruled in any other manner. Wherefore, thus did M. Plantamour proceed on attaining to virile growth and privileges.

From the university of his native city he went to Paris, and studied for two years under Arago, that grand specimen of the Celtic Gaul; a man of superb genius, of commanding presence, of daring flights into the connection and bearings of hitherto untrod branches of physical science; and with whom occasional researches into the curiosities of magnetism, or the uncultivated jungles of meteorology, combined with public displays of fervid eloquence—took the place of regular observatory work, and was thought everything of, almost up to adoration, in the midst of a Latino-Celtic population.

After highly approving himself and his mountain-born abilities among that class of men, descendants of warriors and native chiefs of long, long ago,—Emile Plantamour went north to Konigsberg, and there, under the grandest soul in all Germany for philosophical breadth, instrumental skill, and mathematical power in gravitational astronomy—though originally only a grocer's apprentice, the illustrious Bessel—he learned by what kind of steady work and calm devotion in a quiet home the Teutonic mind obtains some of its highest triumphs. While thus truly a student studying under Bessel, young Plantamour produced, as a thesis, a most creditable essay "On the Determination of the Orbit of a Comet according to Olbers' Method from three Observations." Next he went into Berlin, where, under the celebrated Encke, he learned the still more rigid work of meridian astronomy, besides enjoying the improving

society of the great traveller Humboldt, the magnetical mathematician Gauss, and the astronomical analyst Hansen.

Returning to Geneva in 1839, the venerable Alfred Gautier retired, and Plantamour, able now to look on astronomy from every side, or as a Switzer of each and every diversely tongued canton, was installed as director of the observatory, with powers to choose and direct accordingly. Wherefore thus he proceeded. Not with any of the two or three great observatories of the three or four gigantic countries, powerful governments, and populous nations around him, would he contend in their ancient and still prescriptive work of procuring new expressions for the oldest fundamentals of the grand classic astronomy of sun, moon, and principal planets; no fresh and always minutely differing values would he attempt for the exact quantities of precession and nutation, for the aberration of light, for refraction, and sun-distance; on each of which inquiries such myriads of pounds sterling have been, and still are, being spent, and libraries of books written in the great centres of civilisation; but, while fully appreciating both the grandeur and difficulty of those problems as much as any of the savants working at them, he chose more especially "the orbits of Comets" as the future distinctive subject of his observatory labours.

Comets, however, will not always come just when they are wanted; and so, for a time, we find the disciple of the German Bessel, remembering anew the Gallic Arago, and to such purpose, that the very earliest of his published memoirs in his new directorship, was on "Atmospheric Electricity." Then came two years observations of terrestrial magnetism. And next, duly considering the wants of those of his countrymen engaged in the staple industry of Geneva, watch-making, he organised a department in the observatory where watches and chronometers sent in by the local makers are submitted to a variety of scientific tests, the results published, and prizes awarded for the best time-keepers; with the happiest effects too in promoting improvements in that most delicate branch of all the mechanical arts.

But in 1843, Plantamour's own faithful waiting was at length rewarded by the apparition of one of the most splendid, and in every way remarkable, comets of modern times. Seen first in broad daylight close to the sun, and afterwards hurrying away into the

depths of space with a longer train than any known comet since Newton's day, this chief of comets, in 1843, opened a new epoch of activity among all the observatories; while Plantamour was the first of the computers to announce that in its perihelian passage this comet must have almost grazed the very surface of the sun. That it must have seen for two hours the sun's incandescent disc under an angle of practically 180°, and have been exposed for that length of time to a radiation sufficient to vapourise iron, platinum, and every known metal; yet had it lived, preserved its movement, and gone off at last apparently uninfluenced on its regular orbit. And what kind of orbit was that?

Ah! That indeed is the question; never more abundantly discussed too than at this moment in connection with a comet of the present year. Plantamour had been strong enough in his first theoretical university essay on the beauty of determining a comet's orbit from three observations; but he soon learned in practice that no three observations ever taken by man, much less the first three that are usually secured of such a sudden and unexpected intruder as the great comet of 1843, can give more than a very wide approximation to that one of all the orbital elements which the public most cares for, viz., its period; and thence the date when it will next be seen, as well as that when it was last visible. He showed indeed without controversy, that it was a closed orbit, and of no very great duration; but whether of 165 years, or 22, or even less—and why, in that case it had not been seen oftener, subsequent to its supposed record in 1868, he left for the future to determine.

And now comets followed one another quickly; the next with which Plantamour occupied himself being the second of 1844, called the Comet of Mauvais; and a most opposite one it was to that of 1843 in almost every particular. For this of 1844, though little more at any time than barely visible to the naked eye, remained within telescopic range for nearly nine months; was well and numerously observed during the whole of the time, and gave to M. Plantamour's calculations, perfected as they were in this instance by his careful introduction of corrections for planetary perturbations, a perihelion distance of so much as 78 millions of miles; and a period, reaching the hitherto unheard of extent, of 102,000 years, subject to an uncertainty of not more than $\frac{1}{30}$th of the whole.

Again in 1846 came the separation of Biela's comet into two. These were long followed up by Plantamour, both by observation and calculation; until he at length proved them to be each proceeding on its own independent course through space, quite uninfluenced by the other. It was but a small telescopic comet at any time, until that startling telegraphic announcement of Herr Klinkerfues to Mr. Pogson at the Madras Observatory, on December 2 (1872), thus concentrating the results of his long and difficult orbital calculations :—"Biela touched earth on November 27, search near θ centauri." Pogson accordingly turned his telescope in that southern direction, and found a retreating, and already far-off patch of cometary matter in that quarter. But what had the inhabitants of the northern side of the earth witnessed on the 27th of November? A brilliant display of shooting stars so-called, or isolated meteoric stones, darting through the upper rarefied air at the rate of more than 1000 miles a minute, and taking the regular meteoric observers quite by surprise, as being an altogether abnormal and unexpected vision to them.

Here, accordingly, was admirable authority for Plantamour adding to the Besselian astronomy of cometary orbits, the physical studies of the Aragonian school. But his observatory was ill supplied with instruments of size and quality adapted to such researches, and neither the University of Geneva, nor the politicians thereof, were inclined to spend anything to improve them. So, by noble self-denial, and out of the economies of his ancestors, Plantamour supplied a fine equatorial, with objective of 10 inches aperture, with tower and revolving dome, to the establishment; and kept it thenceforward at excellent work for the credit of the community and the promotion of astronomy.

The situation, too, was deserving of being so powerfully instrumentalised. No less than 10 degrees of latitude further south than Edinburgh, raised on a plateau 1200 feet above the sea-level, in a drier and generally warmer air, and with far less of dreadful coal smoke belching around from blackened and blackening chimneys; a telescope could there be used to its full advantage; and the climate itself would afford, especially in the land, and to a countryman, of Saussure, a most deeply interesting study.

From his very first appointment to the observatory, Plantamour

had continued in the *Bibliotheque Universelle Journal*, the publication of the comparative meteorological observations begun to be taken long before, both at Geneva and at the Hospice on Mount St. Bernard; the earliest example it is said of a systematic study of the climate of elevated regions; and the results, discussed as they were by the hand of a master, were eventually published in his work entitled *The Climate of Geneva from* 50 *years of Observation.* To these again he added his brilliant studies of the physical geography of the region, chiefly from the astronomical and accurate point of view; conducting extensive levellings, both instrumental and barometric, over the highest ridges, and through the deepest valleys of Europe; determining also the force of gravity by pendulum observations in numerous localities, and their longitudes both by telegraphic signals and geodesic measures of the most exact kind.

In short, this admirable man, as our own learned and most devoted librarian, Mr. James Gordon—to whom I owe much of these materials, has kindly informed me—produced in his time no less than 83 distinct memoirs, varying in size from pamphlet to book, with a distribution of their subjects, something as follows,—

Cometary, observations and calculations . .	27
Astronomy, general	6
Eclipses, Solar transits, new planets or planetoids	8
Magnetic	2
Atmospheric Electricity	1
Meteorologic	28
Hypsometric and Geodesic	11

besides several other memoirs in conjunction with M. Hirsch and M. Birner, chiefly geodetic.

No wonder then that his local biographers have described, that even in his later years, when though over-shadowed by the threatenings of his eventually fatal disease, he yet worked a full eight hours a day; and at a kind of astronomical labour which does not exactly repose the mind. Yet through it all, they record that he was ever the gentleman, the man useful to the community, and always ready to give his services wherever they were asked. Oh! what abnegation of self, for who amongst us, living happily under an ancient, long consolidated, and much loved constitutional monarchy, can

presume to know the many republican calls that were made on Citizen Plantamour's time and attention.

"All the scions of our richer families," said a fine old specimen of a New York State country gentleman to me not long ago, "find it expedient to enter into politics, in order to understand the mind of the people, and endeavour to lead public opinion." So too did Plantamour, and with success. For he was listened to with deference whenever he spoke, even in the most radical assemblies. And if not personally present, the mere statement by any orator there that "Plantamour thinks so," would often suffice to carry the day.

And in Emile Plantamour there was something more and beyond mere political wisdom to admire. For when I had, on one occasion, the honour of receiving a letter from him; and which, beginning with a discussion of the Meteorology of the xiiith vol. of the *Edinburgh Astronomical Observations*, went on to speak of various little local institutions he was interested in, as his Bible Society, his Scripture reading Society among the poor, and other similar institutions, a lady listening to my reading started to her feet and demanded "What French savant was capable of such ideas."

So then I had to explain, that although the letter might be written in the French language, the man himself was a Switzer; born in Geneva; and educated in a school founded by Calvin, and approved of by John Knox. Whence immediately she understood the possibility, or even recognised the origin, of that wide philanthrophy joined to the highest science. And this meeting will doubtless similarly appreciate how much our Society has lost in every way by Emile Plantamour's recent demise; so very soon too, or within 18 months, after the Council had selected his name to occupy that honoured place amongst our Foreign Associates which the Royal Society of Edinburgh has always desired to see filled by some representative of that noble, though circumscribed, community dwelling amongst the higher Alps.

C. P. S.

Charles Davidson Bell. By the Astronomer-Royal for Scotland.

Late and for long the Surveyor-General of our vast colony of the Cape of Good Hope, elected an Ordinary Fellow of this Society on March 4, 1878. Died here on the 7th of April 1882, aged 69.

Born at Newhall, in the parish of Crail, in the East Neuk of Fife, in 1813, in a family of three, and of so long lived a race that his mother lived to 85, his father attained to 90, his father's elder brother, General Sir John Bell, a leading officer of the Staff Corps in the Peninsular War, and highly approved of by the Duke of Wellington, reached the still nobler age of 95, and a grand-aunt lived to be 101, while his own brother and sister are still living, hale and hearty;—it might therefore have been hoped, that when Charles D. Bell retired from southern official life to this country, in 1874, there were yet many years before him, wherein to exercise at leisure the many fine talents wherewith he was gifted, and in a manner to show forth something of the fervid love and even ecstatic devotion he always bore to his native land, notwithstanding his long separation from it.

But that was not to be. The trials and the stresses he had gone through in the South African climate and country were too many and too severe, however successfully they seemed to be overcome at the time. Success, indeed, usually crowned almost everything he undertook; and he would have had a far more notable name among us had his career been confined to Old Scotland, instead of being spent so entirely as it was from his sixteenth to his sixty-first year in that new and greater Scotland which stretches now all across the globe, from Canada in the north-west to Australia and New Zealand in the south-east, with the Cape as a very central stronghold. Originally, no doubt, the Cape was a Dutch colony, but one wherein it was long ago remarked to me, that among the many British residents that had come flocking in, every one who got on best, whether in the higher or lower walks of life, was always a Scotchman. And one of the most noteworthy of those, because mainly by such original efforts and innate qualities of his own, was the subject of this notice.

Though leaving his country at so early an age (in 1829), Charles

D. Bell's preparations and prospects were good; for he had attended classes at St. Andrews University, in fellow-studentship with John Goodsir (afterwards Professor of Anatomy in Edinburgh) and his brother Joseph, the minister. While at the Cape, his uncle, then Colonel Bell, private secretary to the Governor, and afterwards and for many years the sage and steady Colonial Secretary, was greatly interested in him.

After a period of service in the Secretary's office, C. D. Bell was transferred to that of the Master of the Supreme Court, and then to the Audit Office of the Colonial Government, becoming a favourite everywhere; until it seemed as if his friends intended the young man for a future of nothing but quiet, resident, jog-trot official life in Cape Town itself, and no further. But in 1833, Sir (then plain Dr.) Andrew Smith (of the Army Medical Service) succeeded in organising an exploring expedition for penetrating into the interior of South Africa on a grander scale than anything hitherto attempted. Whereupon the internal fires of C. D. Bell's own spirit broke forth; and, against the advice and even strenuous opposition of his legal guardians (for he was still under age), he would give up all his other prospects in order to seize this opportunity of penetrating into the great unknown. And though he *was* allowed to join the party at the last moment, it was only on condition of taking the lowest place in it.

An old friend, who well remembered meeting him just after he received his leave, has some years since described in print the wild enthusiasm with which C. D. Bell galloped through the streets of Cape Town to dash off preparations for the immediate start. He was a handsome-looking young fellow too; not very tall, but broad-built and muscular, with a rather brown complexion, but regular features of refined and sculpturesque character, piercing black eyes, and dark lank hair.

The expedition, in spite of its numbers, was generally looked on as one of imminent danger. The latest previous expedition had been cut off to a man; and no previous parties had ever returned without having undergone more or less perils of thirst, of hunger, of wild beasts, of lurking Bushmen with poisoned arrows, and whole tribes of more openly slaughtering foes. But away went this new expedition, striking straight across those stony tablelands of blinding

sunshine, roasting heat, and terrestrial drought, which fence in the Cape Colony along its northern and north-western frontiers almost as imperviously to all ordinary travellers as though they were walls of iron rising up to skies of brass, and intended to prevent inquirers from entering into the mysterious interior beyond, stretching away, as it was then supposed, uninterruptedly to the Equator itself.

A Kaffir war breaking out soon afterwards on the north-east frontier of the colony, the expedition was lost for a time to sight and hearing; but after three years it returned safe, successfully too, on the whole, and with C. D. Bell raised by shere merit and proved capacity to be its second in command. But he had done far more than merely rule over others, and order their services; for when the Association which had defrayed the expenses of the expedition held a public meeting in 1836 to exhibit its results (a meeting at which Sir John Herschel, then in the colony on astronomical research, presided, and gave a splendid address), every one was astonished, delighted, and instructed at finding the walls of the room decorated by nearly three hundred of C. D. Bell's drawings. He had been the artist of the expedition, and such an artist as showed him to possess a soul of true genius, if there be any one in the world of whom that can be properly said.

There, in those matchless drawings, was the peculiar country the expedition had passed through, in its minuter as well as larger features; unadulterated, moreover, artistically by any methods of drawing taught at home on English trees and hedges and shady lanes; for C. D. Bell had taught himself in South Africa on exactly what nature presented to him there. Hence was the great interior's physical geography, geology, and vegetation, too, where there was any, depicted again and again, either in brilliant colour, or *chiaro-scuro* force of black and white, and almost perfect truth of outline; with the very atmosphere also before one to look into, as it shimmered and boiled in the vividness of solar light, and over stony surfaces heated up to 140° or 150° Fahr.; but yet garnished with episodes of the wild animals of the region—generally gigantic mammals, of South Africa to-day, but of other parts of the earth only in some past geological age; and with lifelike examples of the natives of every tribe whose lands the expedition had traversed, depicted in their most characteristic avocations. From little Bushmen securing

once in a way a mountain of meat, in a desert without water, by taking a two-horned rhinoceros in a pit-fall, to the Zulu regiments of King Moselikatse going forth with cow-hide shield and stabbing assegai to exterminate some neighbouring tribe; or the poor of his kingdom, with famine girdles braced tightly round their loins, following in the track of lions, in hopes of partaking of some of their leavings: they were all on those faithful sheets of paper. While, so keen was C. D. Bell's appreciation of the ridiculous, that if there was any young fop among the nearly naked Bechuana or Malokolo, who wore his dress of a few jackals' tails and *some* glass beads with a particular twist of his own invention, and thought he looked so handsome in it that all the women must be falling in love with him, this native-born dandy was sure to figure in some one or other of Mr. Bell's drawings; for he drew as much, or more, from memory in the silent watches of the night, as by sketching direct from nature through the day.

That that brilliant collection of pieces of graphical information never saw the light again until, after twenty years, a few of them straggled out to illustrate later travellers' books,—was no fault of Mr. Bell's. For he had necessarily to give them up to his chief; and he, a very learned naturalist, and taken up far more with curiosities in the way of undescribed snakes,—was allowed by the Association to carry out a scheme of his own for obtaining renewed funds for more expeditions, by exhibiting all his natural history treasures in that focus of wealth and Government patronage, London; but with a result which totally failed to pay its own expenses.

Meanwhile Mr. Bell quietly re-entered the Audit Office of the Colonial Government, where he was raised from his former junior position to be chief clerk; and not long after that received the acting clerkship to the Legislative Council, holding that honourable position during a two years' absence of the proper officer.

But mere pen-work within four walls was not enough to satisfy C. D. Bell's aspirations, or assure his conscience that he was thereby turning to the utmost account all the varied talents committed to him by his Creator. In 1838, therefore, he began to turn his attention to surveying, and became soon after, one of the sworn land surveyors of the colony;—a colony twice the size of Great Britain, but with a population of half the city of Edinburgh. A colony of

immense, craggy, rocky, mountain ranges (Le Vaillant termed one of them "the backbone of the earth"), and extensive desert plains, with a nice variation in the quality of their barrenness accordingly as they were of deep sand or ferruginous clay, mixed or unmixed with salt and gypsum. A colony, too, where the British element of population was still in utmost minority; and where the surveying system hitherto in vogue in the back, or over-berg, country had been—to let any Dutch boer, wanting land, choose some possible central water-hole in Government or unoccupied ground, and then ride round it on horseback for three hours, or drive round it on an ox-waggon for three days, according to its degree of want of vegetation; such interested boer undertaking to remember against all future comers what particular isolated bushes, or great rocks in the weary land, he had seen during such circumferential ride or drive, and had then and there chosen to be his baakens, or landmarks for ever afterwards.

As population increased, such a system was of course most fruitful in land disputes, and the then Surveyor-General, Colonel Mitchell, formerly of the Portuguese Legion in the Peninsular War, found his attempts to introduce accurate surveying into town lots grievously swamped by having presently the legal business of certain Land-drosts, Heem-raden, and up-country Dutch Civil Commissioners, further thrust upon him, and his then sole assistant, Mr. Hertzog. He applied, therefore, in 1840, and obtained Mr. Bell's appointment as Second Assistant Surveyor-General; when he (C. D. Bell) was immediately sent off on a long and solitary travel, occupying several months (quite a geographical exploration in itself, in his little ox-cart, and attended only by a Malay driver and a Hottentot leader of the oxen), to the north-western corner of the colony, to settle disputed claims there; some of them on the cool Khamiesberg granite mountains 5000 and 6000 feet high, others in the hot and arid plains below, or the sandstone ranges and steppe formations further eastward and northward, even as far as to the Orange River below its falls. And he settled them so satisfactorily, and with so much calmness and wisdom, that the Dutch boers ever after that always addressed him, though still only twenty-seven years of age, by their title of highest honour, viz., "Old Mynheer Bell."

He was next appointed to organise a survey office in the eastern

district of the colony, where so many Scotch settlers were established under Pringle and others many years before, in what the Dutch had stigmatised as the "Zuureveld," or field where the grass was sour, and in their eyes, and to their means and resources, good for nothing, except to stick Englishmen in, to serve as buffer between them and the Kaffirs. From thence he was taken for a time by the new Governor of the colony, Sir Peregrine Maitland, to inspect the Kaffir frontier and interior districts. And again, in 1845, across the upper Orange River to Zwartkopjies, "where," says a local print, "the information he had acquired in the expedition of 1833 was found extremely useful." No doubt it was, too, and yet not sufficiently utilised by the Government at home; for had it been, several calamitous disasters of recent years, and excitations of international hatred in the north-east of that country, would have been avoided. The distribution, too, of the British settlers would have been arranged in a manner to profit far more by the physical geography of South Africa; which offers a Brazilian climate on one side against an Arabia Deserta on the other, if you only go far enough in either direction from the intermediate point of Table Mountain and Table Bay, where the Dutch first landed, and for so long looked upon it as their only and commanding centre for their agriculture, commerce, and government in all the southern hemisphere.

In 1848 Mr. Bell became, on the demise of Colonel Mitchell (a demise much lamented, and hastened in no small degree by the extra anxieties of his office), full Surveyor-General of the Cape of Good Hope. Thenceforward, for twenty-six years, he ruled in that department with a suavity, firmness, and knowledge that obtained the best possible results for Government, out of an otherwise almost hopeless, tangled collection of conflicting interests, old legislations, and changed ideas. His thorough knowledge of the colony in which he had grown up to man's estate, his mature development of a judicial breadth of view, combined with his scientific skill and mechanical abilities, were evidently becoming more and more appreciated; for, spite of the over-work of his own office, he was so frequently requested to join divers Government committees, and prepare special reports, such as that on the establishment for convicts, lepers, and lunatics on Robben Island, the newly-found copper fields of Little Namaqua land: and various lines of road to open up

hitherto untrodden districts: until at last, such extra demands of the upper colonial officials culminated in this,—that whereas they had quarrelled with and dismissed their recently imported English engineer-in-chief for a proposed line of Metropolitan Railway, extending from Cape Town to the Berg River Valley, and some one must be found to take his place, they unanimously agreed that C. D. Bell was that man; because he was the only one amongst them who could lay out railway curves, build bridges, raise embankments, bore tunnels, inspect locomotives, and, in a word, save them, the great officials of the new "responsible Government," so called, in the eyes of the people, and before public opinion.

And he did help them through that great difficulty by extraordinary exertions of his own, and which he could hardly have accomplished at his then advanced age, but that he had never spared himself. He had lived a triple life all along; first, his official life, whose duties were always paramount with him; second, his private social life, where he was always a favourite; and lastly, his artistic life, which occupied almost every other moment of his existence.

In 1846, while still in Cape Town, by shere dint of his knowledge of the eastern country and people, he produced a long series of drawings in black and white, representing events in the Kaffir war then raging under Sir Peregrine Maitland,—drawings which astonished and delighted the soldiers who had been engaged in the operations,—and, being sent home, were taken on one occasion by the Duke of Wellington into his private study, to con over alone, before giving his opinion on the conduct of that war to the House of Lords.

In 1847, having come home on a short leave of absence, Mr. Bell procured from Messrs. Schenck & Co. of this city, a lithographic press and stones, learned to work it himself, threw off at once a number of South African subjects, varying from the Rev. Mr. Moffat preaching to Bechuanas, down to Amakosa Kaffirs torturing a wounded prisoner; and took the whole plant out with him to Cape Town, a novel and important accession at that time to its means of graphic multiplication.

There he further worked at oil painting; and when the community at last began to awake to the importance of art culture, and opened an exhibition of pictures, he carried off their gold medal for

a spirited historical painting, representing Van Riebeck founding the Cape Colony in 1650.

Wood carving next came in for attention during some of C. D. Bell's spare hours; also modelling in clay; such models, after baking in an oven, being then painted in natural colours. For the object of most of these artistic works "in the round" was to preserve the physiognomy, manners, customs, tastes, and traditions of the native races of South Africa. He had enjoyed the opportunity of seeing those races on his first arrival in their country, still numerous, distinctive, and true in their stationary savagedom to a long past antiquity; but before he left, they were rapidly losing, under new conditions, those characteristic features, as well as their ancient, imperfect mother tongues.

Such then was the man, Charles D. Bell, who, leaving most of those precious works behind him, or having given them away right and left too generously as soon as completed, returned to this country in 1874, with his second wife (a Cape lady), two sons, and a daughter. Giving vent immediately to his long pent-up passionate admiration for his native land, he soon joined the Antiquaries and the Meteorological Societies, as well as our own; wrote on the ancient harps of Scotland, and began to illustrate in painting some of the touching ballads of the country's former days. But his existence was saddened by the quickly following deaths of mother, father, and uncle. Then he suddenly lost the use of one eye. Without external change, or internal feel, the sight power was gone. He had always been very short of sight, however keen; and it was that eye, whose surrounding muscular contractions had enabled him to keep a strong concave lens always in place through fifty years of excellent work, which had now suddenly broken down.

But most of all was he affected by the sudden and totally unexpected demise at Crail, during a summer residence there last year, in his father's old house, of his beloved Wife. His faithful spirit never recovered that blow, and he but lingered on for some six months further, until he followed her himself.

I cannot expect that, with my imperfect knowledge of his most multifarious life, I have in any way succeeded in representing it as it so fully deserves to be represented, in all its noble character and just proportions. Nor is there, perhaps, the most pressing of all

necessities that I should do so before the present audience; for it is not we, but the Cape people who should erect a statue to him: to him, Charles D. Bell, who did, and accomplished, and suffered so much for them and amongst them, through all the best years of his long period of a most hard-working life and publicly useful career. C. P. S.

William Robertson, M.D. By George Seton, Esq., Advocate.

Dr. William Robertson was born in Edinburgh on the 8th of January 1818. He was the eldest son of Mr. George Robertson, Keeper of the Records in H.M. General Register House, by Eliza Brown, his wife, sister to General Sir George Brown, of Crimean fame, and Mr. Peter Brown, well known as an agriculturist and land valuator in the north of Scotland. He obtained his early education at the Edinburgh Academy, from which he passed to the University; and, after completing the medical curriculum, he continued his studies at Paris, Berlin, and Vienna. In 1839 he graduated M.D. of Edinburgh, his Thesis being on Enlargement of the Heart, which proved to be the disease from which he suffered prior to his death. Four years afterwards he was admitted a Fellow of the Royal College of Physicians. He acted for some time as a physician in the Royal Infirmary, the Fever and Cholera Hospitals, and the New Town Dispensary; and, holding the appointment of Inspector-Physician of the British Civil Hospital at Renkioi, in virtue of the recommendation of Sir Robert Christison, he served as a physician during the Crimean war. He was at one time editor of the *Edinburgh Monthly Journal of Medical Science*, to which he contributed several papers. On the resignation of Dr. Stark, in 1874, he was appointed to the post of Superintendent of the Statistical Department in the General Registry Office of Births, Deaths, and Marriages, having previously acted as Medical Registrar for Scotland. One of his latest official works was the preparation of the Report prefixed to the first volume relative to the Scottish Census of 1881. In 1876, on the death of Dr. Warburton Begbie, he became medical officer to the Scottish Widows' Fund, having by that time gained large experience in matters connected with Life

Insurance, in the capacity of medical referee to the Guardian and Scottish Equitable Societies.

Distinguished by his diagnostic skill and his thorough knowledge of therapeutics, but for his modest and retiring disposition Dr. Robertson might, in the opinion of competent judges, have taken a very distinguished place as a consulting physician ; and, owing to his high reputation as a mathematician and a statist, he was eminently fitted for the two appointments which he held at the time of his death. His capacity for figures was of a very high order. He did not hesitate, however, to facilitate his elaborate calculations by the use of the arithmometer, which he was able to turn to the best account, owing to his remarkable memory and his powers of numerical combination.

Nor were his acquirements confined to physical and mathematical science. While well versed in classical as well as modern literature, he was an excellent linguist, being familiar with French, German, Italian, and Turkish, and possessing a fair acquaintance with Spanish and Dutch.

In social life he was a universal favourite, in consequence of his kindly and genial disposition, his fund of anecdote, and his well-stored mind. One of the original members of the Edinburgh Evening Club, he seldom failed to appear at its bi-weekly meetings, where the blank which his lamented death has caused will not easily be supplied. His cordial sympathy with the young was an interesting feature in his character. He was a devoted member of the Church of Scotland, and his political tendencies were Conservative.

In connection with his official appointment in the Registrar-General's Department, it may be mentioned that the office of Joint Deputy Keepers of the Records was held, in the first instance, by Dr. Robertson's grandfather and granduncle, in succession to whom it was held, also jointly, by his father and uncle, and singly, at a later period, by his brother George Brown Robertson, Writer to the Signet, who died in 1873. Accordingly, the official connection of the family with the General Register House extended over a period of upwards of a hundred years.

Dr. Robertson became a Fellow of the Royal Society in 1860. His death occurred at his residence in Albany Street on the 25th of

August 1882, at the comparatively early age of sixty-four. He had been in failing health for about two years, but it was only a week before he died that he became seriously ill. His funeral took place in Warriston Cemetery on the 29th of August, and was attended by a large concourse of attached relatives and friends.

Dr. Robertson is survived by two sisters, with one of whom he resided, while the other is the wife of Mr. John Gillespie, Writer to the Signet, and Secretary to the Royal Company of Archers. His youngest brother, Alexander, a promising artillery officer, was one of the many victims of the Indian Mutiny of 1857.

Since the above was prepared, the writer has received a letter from one of Dr. Robertson's medical compeers (Dr. George Bell) in reply to an application on the subject of his chess-playing, in which he says :—" Dr. Robertson was no ordinary chess-player ; he *understood* the game, and practised it with judgment and skill. I know this, for the 'chequered field' was our favourite meeting-place during many years. Always pleasant there as elsewhere, Edinburgh does not know what a rare son she has lost. Though undemonstrative, the Royal Society had few such members as William Robertson."

Sir Daniel Macnee. By the Rev. Walter C. Smith, D.D.

Daniel Macnee's life, like that of most hard workers, was not a very eventful one. Its chief incidents were its productions, and these were nowise startling, but rather such as might have been looked for—fruits of patient labour, and proofs of quiet growth. Born at Fintry in 1806, he lost his father while yet a mere child ; but he was happy in having a mother who could understand and guide his youth. Very likely that youth puzzled her a little at first, for she would fain have trained him for merchandise and money-making, and his gifts did not lie at all in that line. The sleepy valley of the Endrick, among the green Campsie hills, had to produce its genius like other Scottish glens ; and probably his mother had her anxiety, as well as her pride, when it began to dawn upon her that she had given birth to one of that wayward race. I suppose

he did his school tasks fairly well for her sake; but after school hours, if he was not fishing the water, he was sketching his companions, or telling the drollest stories of things he had seen or heard, which were truly pictures of the vividest kind. So she concluded that he was born to be an artist; and that, no doubt, was his own opinion also. Yet, with all his well-merited success, it may be doubted whether they were not both of them mistaken. That he had genius was clear enough, and that he was fond of drawing pictures was plain to every one who knew him. But whether his genius would best find its true field in painting the outward or the inward man—faces or characters—that the gossips of Fintry could hardly be expected to determine. It showed some courage then, at that early stage of Scottish art, to devote a boy of thirteen to so precarious a means of living; but it would, no doubt, have looked like very madness to bring him up for the career of a man of letters. Yet, excellent as his portraits are—and some of them caught not the features only, but the very spirit of the sitter—those who knew him, and can remember the delicate shades and dramatic play of character in the stories with which he was wont to brighten our social intercourse, will hardly doubt that his real power lay rather in word-painting than in material pigments. The patient industry which he devoted to art would have made him a subtle dramatist—a writer of such comedies as Scotland has never yet produced, or a novelist to rival her very best. I am not sure, then, that in making an excellent painter of him, we did not lose something greater still, for which nature had specially endowed him.

There was a Glasgow artist, at this time, who bore the honourable name of John Knox, to which, however, he has not added any fresh lustre, for he will probably be known hereafter chiefly as the teacher of Daniel Macnee and Horatio Macculloch. Yet there must have been something in him to have trained two such men. These two formed their life-long friendship in Knox's studio; and many a trip, doubtless, the two lads had together to the lochs of Argyle and Dumbarton, and many a Highland story they picked up, and learned to interpret well the character both of its scenery and its people. Afterwards Macnee came to Edinburgh, and studied at the Academy there, along with Thomas Duncan, Scott Lauder, and David Scott, who all became his warm friends. For there was

no mean jealousy in his nature, but he gladly recognised the genius of his compeers, even when their views of art differed wholly from his own. In the end, having been admitted an Academician without passing through the humbler grade of Associate, he settled in Glasgow, till he was elected to the Presidency of the Royal Scottish Academy, on the death of Sir George Harvey in 1876.

It was in Glasgow, then, that his life-work was really done, and probably the necessities of "pot-boiling" dictated the path it was to follow. Now and then, for a season, indeed, he sent for exhibition some simple rural study—"A Peat Sledge," or "A Burnside," or "A Pretty Picture of Children"—which had a touch of pure poetic fancy. But these were short flights into a region which he could not afford to cultivate; and ere long he settled down to the steady business of portrait painting. Rembrandt and Reynolds, Vandyke and Raeburn, have shown that this may be a very noble branch of art; and Macnee's portraits of the late Dr. Wardlaw and Mr. Dalgleish prove that he had no mean idea of the work of his profession. But if painting was the right vehicle for his genius to express itself in, we should have expected to find him rather following in the wake of Wilkie than of Raeburn, and showing on his canvass that dramatic power, and insight into Scottish character, and rarely delicate humour, which were the richest gifts, and most real qualities of his mind. Nothing of this, however, can be found in all his work. Even in painting portraits, though among so many he must have come across some faces which had Scotch character and humour like his own, yet I am not aware of any of his pictures which suggest what a wealth of laughter lay in the man. It would almost seem as if his art was not his natural utterance, but a mere skill of hand, and that this successful painter, after all, had not "found his mission," and has left no real record of his brilliant genius, except in the short-lived memory of his friends. For assuredly, however excellent his likenesses are, and however ably some of them are painted, they give no adequate conception of that singularly rich and fertile and dramatic portrayer of national character, who could so nicely hit off not local dialect only, but local habits of thought, with strokes of finer insight that pierced far into the deepest heart of man. Of all this, however, nothing now remains. No other tongue could reproduce those tales,

which had no plot to speak of, which were at times even a mere thread of extravagance, but in which the characters were so felicitously sketched with touches of such kindly humour, that they themselves could only have joined heartily in the mirth which they evoked. For this was a marked feature of his genius; there was not a drop of gall in it. If he saw all the oddities of a Glasgow bailie, an Airdrie miner, a Paisley shopkeeper, a Highland gillie or drover, or minister, "he handled them as if he loved them," and, indeed, he did like them all the better for the flavour of individual character they had.

I would not be understood as anywise undervaluing his artistic powers, which were of no mean order, but they certainly would have attained a still higher rank had his canvas been covered with many figures representing the men who lived so vividly in his stories, and reflecting the dramatic lights in which he could have placed them. Most likely this was at first prevented by the *res angusta domi*, and when easy times came his *role* was already determined for him. So he went on painting portraits—many of them; and making warm friends—many of them, too. Art naturally drew to him Macculloch, Sam Bough, Brodie, and others; and his rare social qualities as naturally associated him with Outram, Glassford Bell, and Norman MacLeod. Brighter evenings there were not in all Scotland than those which brought together the authors of the Annuity and Billy Buttons and Daniel Macnee—and the brightest of them all was Macnee. He was the last of them, too, and perhaps this fact, that he had been left alone by these, his dearest friends, made him more willing at last to leave Glasgow, and take up the burden of the Presidency, even when his own health began to be uncertain.

How he discharged those duties, and commended himself to all his brother artists by hearty kindness and frank recognition of their several powers—how he also interested himself in the meetings and business of our Royal Society—how he soon became as valued a member of general society in Edinburgh as he had been in Glasgow; —all this is known to you all, and all the more is our sorrow that his stay among us was so brief.

David Anderson of Moredun. By A. Campbell Swinton of Kimmerghame.

Mr. David Anderson of Moredun, who died in Edinburgh in his ninetieth year, was the eldest son of Mr. Samuel Anderson of Moredun, banker in this city, the head of a family many members of which have been well known and highly respected. Mr. Anderson was long a leading partner in the banking house of Sir William Forbes & Company, and when that firm was, in 1838, merged in the establishment now known as the Union Bank of Scotland, he continued for many years an active director of the new body. There was no citizen of Edinburgh, whose generous aid was more readily given, to every object which commended itself to his approval as likely to benefit his countrymen of any class or condition; and consequently there is no one whose loss will be more seriously felt, when any measure of public utility is in contemplation. He was elected a Fellow of the Society in 1849, but never took an active part in its proceedings. He was, however, a man of cultivated taste and varied information, whose warm heart and genial disposition made him a universal favourite among a large circle of friends. And as one of the Trustees of Fettes College, he showed his interest in the higher education of the country by endowing two scholarships of the annual value of £100 each, with a view of enabling distinguished pupils of that school to prosecute their studies at one or other of the English universities.

John M'Culloch. By Francis Brown Douglas.

Mr. John M'Culloch was a native of Galloway, born in 1807. He died at Edinburgh 13th July 1882, in the 76th year of his age. After attending classes at the Glasgow University he came to this city, when he entered the British Linen Company Bank, and remained in connection with it for a period of fifty-five years, much esteemed for his probity and business habits. Many a journey he undertook to bring gold from London for the bank's purposes, and to take it back when no longer required.

He had a kindly feeling for the poor and helpless, and was

induced, shortly after coming to Edinburgh, to become a visitor of the Society for Relief of the Destitute Sick. He was appointed its treasurer, an office he filled for nearly forty years, ever seeking to promote the usefulness of that institution in his own way, and to increase its funds. He also latterly took an active part in the management of St. Cuthbert's Parochial Board, being the more interested in this from his connection with the West Kirk session, of which he was an elder for no less than fifty-five years.

Mr. M'Culloch was admitted a Fellow of the Royal Society on 2nd January 1866, and was a very regular attender at its meetings —generally, indeed, present unless prevented by illness. In his later years he was subject to sharp attacks of cold and rheumatism, which much impaired his strength and health, and from one of which, with other complications, arose his last illness and death.

Samuel Raleigh, C.A. By David Maclagan, F.R.S.E.

Mr. Samuel Raleigh was a native of Galloway, having been born on a farm near Castle Douglas held by his father. His early education he obtained in the parish school and high school there; where his brother, afterwards a distinguished Nonconformist divine in London, was being trained at the same time. After a brief apprenticeship to a local solicitor, Samuel Raleigh resolved to go to Edinburgh, and seek there some opening which might afford him an opportunity of securing a position of usefulness and success.

He entered the University as a student at the Law Classes, and at once made his mark by carrying off Professor Macvey Napier's first Conveyancing Prize.

There, as always, he was a man of unwearied industry, and used to say that his object in reading systematically the English Classics was to acquire a good style of composition. Those who remember his power of expression in writing, either on business or more general subjects, will recognise how successfully he achieved his purpose.

Very soon he became partner of Mr. William Campbell of Queenshill, Writer to the Signet, like himself a Galloway man.

It was very well known to Mr. Raleigh's friends that his tastes

lay in the direction of figures and finance more than of law alone, and that he possessed a singular proficiency in dealing with them. The offer therefore of a partnership with Mr. Archibald Borthwick —one of the ablest accountants in Edinburgh—was readily accepted by him; and the partnership so constituted, of two men of great powers, and in very many of their special qualities the complement of each other, became one of the outstanding firms of Edinburgh, recognised as such by all professions. The arrangement, however, only lasted until 1857, when the crash of the Western Bank failure led all interested in that calamity to cast about for suitable men to extricate matters, and Raleigh was selected along with other three, who are now each at the head of leading Scottish banks.

The changes in his professional life, which are not always a favourable experience, proved remarkably so with him. He got an insight in business of quite unwonted range and variety; and he was just the man to extract and utilise the best elements out of such a career.

It was not therefore surprising that when in 1859 the office of Manager of the Scottish Widows' Fund Life Assurance Society became vacant, the Directors sought to secure his services. The offer somewhat perplexed him. His professional prospects were so good that he felt it was a doubtful step to enter upon this new life, and he asked and obtained time to consider the proposal. After consultation with friends on whose judgment he relied, he closed with the offer, and became Manager of a Society which, even at that time, stood in the very highest rank among Scottish offices, and which, under his management, was to acquire the position of unapproached pre-eminence which it holds in Great Britain. It was not only, however, the large increase of its business which gave such universal public confidence, but the knowledge of his skill in manipulating and investing large sums of money, and in devising and working out the best ways of distributing the very large profits which accumulated during each septennial period.

Although never taking a public part in political or ecclesiastical affairs, for which he had little leisure and no taste, he took a deep interest in all such matters—held very decided views regarding them—and was often consulted, and always ready with his counsel.

In the year 1880 the labours and responsibilities of his business

life began to tell upon him, and he resigned his appointment. His retirement was of short duration; and he died 26th July 1882.

Professor James Spence. By Professor Chiene, M.D., F.R.C.S.E.

James Spence was born in Edinburgh on the 31st day of March 1812. His father sent him, in the first instance, to a boarding-school at Galashiels, and afterwards to the Edinburgh High School. He entered the University at the age of 13, attended the medical classes in the University and Extra-mural School, and obtained the diploma of the Royal College of Surgeons in 1832. His first ambition was to enter the army or navy, and for this purpose he studied in Paris, and passed the examination for a surgeon in the navy. After two voyages to India in troopships, he apparently abandoned the idea of public service, and settled in Edinburgh. It may with truth be said that he then (1835) commenced that career as a teacher and surgeon which paved the way for his appointment as Professor of Surgery in 1864. He first, for seven years, acted as Demonstrator of Anatomy to Professor Monro (*tertius*). He then taught Anatomy in the Extra-mural School until 1849, when, having obtained the Fellowship of the Royal College of Surgeons, he became a Lecturer on Surgery. He held this appointment until his election as Professor of Surgery in 1864. In 1865 he was made Surgeon in Ordinary to the Queen for Scotland. In 1866 he became a Fellow of the Royal Society of Edinburgh.

For nearly half a century James Spence was intimately associated with the teaching of Anatomy and Surgery in this city. From the very first he adopted a course of self-education, and under many difficulties he gradually but surely made his way to the front; and at the time of his death (June 1882) he had attained a position in which he was esteemed by all as the representative of Scottish Surgery. He possessed most marked manipulative skill, and was a very successful practitioner.

He has left, as a result of his long practical experience, a most valuable work on the Practice of Surgery. To tracheotomy, herniotomy, the ligature of vessels, urinary diseases, and methods of

amputation, he paid special attention, and has done much to advance our knowledge.

James Spence is an example of a man who slowly rose to eminence by earnest, honest work. He will be remembered as a teacher who had always something worth telling on every practical question, and who told it in a way easily remembered. His systematic lectures were essentially clinical.

Much loved by those who knew him best, his memory will long remain in the Edinburgh school as a faithful teacher, a good operator, and a kind friend.

Frederick Hallard. By Thomas M'Kie, Advocate.

Frederick Hallard, Advocate, senior Sheriff-Substitute of Mid-Lothian, died in this city on 12th January 1882, aged sixty-one. His father was a soldier in the French army, who, after the Revolution of 1793, emigrated to this country, and, along with other Royalist refugees, took up his abode in Edinburgh as a teacher of his native language. Here he married, lived, and died. His son Frederick was born in this city in May 1821. At the age of four, he was taken to Avranches, his paternal home in Normandy; and there, and at Paris, he received a sound and liberal education. At sixteen he returned to Edinburgh. The strong affection he always had for the city of his birth arose not more from admiration of its material beauty, than out of regard for its intellectual renown, and the friendly intercourse which existed between it and France in the olden time. Being destined for the Scotch bar, young Hallard attended the usual classes at the University of Edinburgh, and proved himself a diligent and distinguished student. He was admitted to the bar in 1844, joined the *Speculative Society*, and after acting for some years as a reporter on the *Jurist*, he was, in 1855, appointed by the late Sheriff Gordon junior Sheriff-Substitute for Mid-Lothian. From that time until his death, he discharged the duties of his office with a manly independence of spirit and judicial integrity of purpose, rarely equalled. The year before his judicial appointment, he married Mary Carr Robertson, a daughter of the late Mr. James Robertson of this city. The marriage was one of affection, and for

many years was a source of uninterrupted happiness. But of the nine children born of the marriage, death carried off three in as many months; shortly afterwards, the grave was again opened to receive his beloved wife, and in 1873 he had yet again to follow to the tomb his eldest son, Frederick, a youth of great brightness and promise. Against this overwhelming affliction Mr. Hallard bore up outwardly with manly fortitude; but those who knew him best knew too well how the sad ruin of his once happy home haunted his memory, and bowed to the earth a spirit shrinkingly sensitive and keenly affectionate. It was then that he truly felt the consolations of philosophy; for he had loved letters from his early youth with a devotion which grew with his growth, strengthened with his manhood, and continued with him to the end. His literary tastes had adorned and brightened his life in the times of prosperity, and when the sorrowful days came, these tastes weaned him from himself, and gave him comfort, if not consolation. One charm of his society was, that along with a love for all things lovely and of good report, he united in a singular manner, in his own person, two separate nationalities. For his intimate acquaintance with French literature, history, politics, and jurisprudence was happily combined with a wide knowledge of, and a lively interest in, everything pertaining to the literature and jurisprudence of our own country. To his other accomplishments he added a keen relish for classical studies, and particularly Greek.

Besides doing his judicial duties, Mr. Hallard for many years, and until his death, acted as a Director of the Philosophical Institution, and took an active part in the management of its affairs. The useful work he did there cannot be better summarised than in the words of its Vice-President, Dr. William Smith, who at a meeting of the Directors of that Institution thus spoke of Mr. Hallard:—"We can call to mind how much his fine tastes, his varied culture, and his active helpfulness, his ready aid, always willing and gracefully rendered, have contributed to our success. Associated with him as I have been for nearly thirty years, no one knows better than I do how much we have been indebted to him in these respects; and I had looked forward to the time when you might permit me to retire from this chair, which by your favour and indulgence I have occupied so long, and called on him to fill it with new and fresh efficiency."

Mr. Hallard became a Fellow of the Royal Society on twenty-first January 1867. He was proud of its diploma, pretty constant in his attendance at its meetings, but never read a paper, nor took part in the debates. This was partly owing to an inherent modesty of nature, and partly because his knowledge and the bent of his mind were much more literary and philosophical than scientific. He published several able pamphlets on legal topics, one of them being entitled "The Inferior Judge," and he took a prominent interest in all questions affecting a reform of the law. Apart from these he did not write much; yet what he did write showed such vivacity, grace, and culture, that, like the aroma of good wine, it served but to whet the appetite and to make one wish he had written more. But that was not to be; and so he has passed away from among us, still to be held in fond remembrance by a wide circle of friends.

Dr. John Muir. By Professor Eggeling.

Dr. John Muir, who died on the 7th of March last, was born at Glasgow on the 5th February 1810, being the eldest son of Mr. William Muir, at one time a magistrate of that city. After receiving his early education at the grammar school of Irvine and the University of Glasgow, he passed to Haileybury College, then the training institution for the civil servants of the East India Company. In 1828 he proceeded to India, and, having passed with distinction through the College of Fort William, and served for some years as assistant secretary to the Board of Revenue at Allahabad, and afterwards as a commissioner for investigating claims to hold land rent free in the Meerut Division, he was appointed magistrate and collector at Azimgurh. During his occupancy of these posts (a period of some fifteen years) he always devoted a large portion of his leisure to the study of Sanscrit literature; and so well did he succeed in mastering the language, that he himself composed several treatises in Sanskrit metre and prose, viz., a description of England, a sketch of the history of India, and two treatises setting forth the essentials of the Christian doctrines and ethics; and delivered to the students of Sanskrit at Benares lectures in that language on mental philosophy, and kindred subjects (1843).

In 1844 the combination of the hitherto separate Sanskrit and English colleges at Benares was resolved upon, and John Muir was appointed first principal of the institution. In an address delivered by the Hon. James Thomason, Lieut.-Governor of the N.W.P., at the opening of the Benares New College, on 11th January 1853, credit is given to Mr. Muir for having succeeded "in introducing into the college a stricter discipline and a better system of education." This post John Muir held for one year, when he was succeeded by Dr. Ballantyne, he himself reverting to the judicial branch of the service, as civil and session judge at Futtehpore. From his parting address to the students of the Benares College, on the 10th February 1843, I extract the following passages as characteristic of the man:—"Now, I am anxious that your reasonable ambition should be satisfied; I desire to see you all rise to wealth and honour; but I am more solicitous that high principles should now be implanted in your minds, which in after life may bear the precious fruits of integrity, wisdom, and piety. I wish that you should be devoted to study, not so much for the outward advantages it brings, as because you love that truth to which it ought to lead; because you appreciate the most valuable results of education, I mean intelligence, enlargement of mind, the cultivation of your judgment and other faculties; acquaintance with the wonderful works of God, and the laws by which He rules the universe;—above all, because you find that sound instruction is auxiliary to moral improvement. These are the motives which best deserve to be urged at length to stimulate you to the earnest pursuit of knowledge." After a brief outline of some of the chief departments of Sanskrit literature, he continues:—"There is, however, one subject which, more than any other, demands your earnest attention, both during the course of your education and after its close; I mean your moral improvement. If the instruction you have received in the college have not inspired you with the love of goodness, of truth, integrity, justice, purity, and piety, as well as with a desire to practise all these virtues which in theory you admire, it will have effected but little. Mere intellectual, unattended by moral improvement, may render you only more accomplished in wickedness. True wisdom cannot exist apart from goodness. However strengthened by discipline your powers may be, they will always be directed to

the attainment of ignoble or comparatively insignificant objects, if they are not guided and hallowed by virtuous principle. True self respect, real happiness, the blessing of God, and your everlasting welfare, all depend on you strictly regulating your lives according to the dictates of conscience and the Divine will."

In 1854, having completed his term of service, John Muir returned to England, and, after a brief residence in London, he settled permanently in Edinburgh. During his last few years in India his earlier literary attempts at religious subjects were followed up by a *Life of the Apostle Paul*, and an *Examination of Religion*, both of them in Sanscrit verse, with English (the former treatise also with Bengali and Hindi) translations. The deep interest which he always took, not only in the moral improvement of the Hindus, but in religious and theological matters generally, led him, in later years, to offer to the University of Cambridge a prize of £500 for the best exposition of the errors of Indian philosophy, and the principles of Christianity in a form suitable for learned Hindus; and to the University of Glasgow a prize of £100 for proficiency in Hebrew scholarship, open to all Scottish graduates in arts of not more than six years' standing. It also prompted him, some years since, to endow, for a period of five years, a lectureship on the Comparative Science of Religion in the University of Edinburgh. Moreover, it induced him to take up the systematic study of the religious literature of India, and the writings of modern European theologians. The results of many years of unwearied research were laid down in a number of papers, mostly contributed to the *Journal of the Royal Asiatic Society*, and ultimately collected in four volumes of *Original Sanskrit Texts on the Origin and History of the People of India, their Religion and Institutions.* This work, of which a revised and greatly enlarged edition, in five volumes, was published in 1868-70, forms by far the most complete and trustworthy digest of authentic texts bearing on the growth of the Brahmanical doctrines and institutions. The amount of patient, methodical research with which the various religious conceptions of the ancient Hindus are traced by him from their first germs through the various phases of development; and the impartial spirit with which he reproduces and examines the often conflicting views of European scholars on single points of Hindu

tradition, are beyond all praise. His English translation of the frequently obscure texts, as a German scholar has justly said, "betrays throughout a master's hand." To insure accuracy in his interpretation of difficult passages, Muir would save himself no trouble, but would write letters upon letters to Sanskrit scholars who he thought might be able to clear up his difficulties. I have sometimes heard it remarked that, in dealing with important questions, Muir too often contents himself with stating the conflicting views of others, without giving any decided opinion of his own one way or the other, when he was at least as competent as any other scholar to pronounce on these points. To a certain extent this is no doubt true; but it is only what might be expected from so cautious and conscientious an inquirer, whose sole aim was to get at the truth; and who, while ever anxious to allow every one a fair hearing, shrank instinctively from committing himself to a definite alternative where the available data appeared to him insufficient for forming a conviction. His mind, indeed, was singularly open to argument; it was as free from preconceived ideas as it was disinclined to hasty conclusions. As in his literary inquiries regarding the bygone ages of Indian belief, so in his own religious views, which, it would seem, were somewhat modified, in his latter years, by a close study of modern theological writings. Liberty of research and teaching, in whatsoever department of human science, was to him an article of faith, which neither his natural reserve, nor outside considerations of any kind, could keep him from vindicating. The powerful impetus imparted to the study of the Vedic texts, some thirty years ago, gave rise to an animated discussion as to the degree of authority to be assigned to the traditionary interpretation of the sacred lyrics, as handed down in the native commentaries. Into this literary warfare Muir threw himself with the full weight of his scholarship, in a manner showing how well he knew to fight for the principle of free research, so dear to him. A distinguished Sanskrit scholar had given his opinion to the effect that "in the present stage of Vaidik studies in Europe, it seemed to him the safer course to follow native tradition rather than to accept too readily the arbitrary conjectures which Continental scholars so often hazard." This remark drew forth, after a few weeks, Muir's excellent paper "On the Interpretation of the Veda" (*Jour. of the Roy.*

Asiatic Soc., 1866). Writing with more warmth than he usually displayed in his writings, he therein proved conclusively that it is a mistake to speak of an unbroken chain of Hindu tradition, the meaning of the Veda having already become largely obscure by the time a school of exegesis arose; and that, therefore, the scholars alluded to (viz., Roth and his school) were quite justified in emancipating themselves from the trammels of native tradition, and calling into requisition all the other available resources of philology, thereby laying the foundation of a true interpretation of the Veda.

After the completion of the second edition of the *Original Sanskrit Texts*, Dr. Muir was by no means satisfied to rest on his laurels. He continued his studies as assiduously as ever, though perhaps with a less definite object in view; printing from time to time, for private distribution, small collections of metrical translations of characteristic passages he met with in his reading, generally of a moral or religious tendency. These were ultimately published, in a collected form, in a volume of Trübner's Oriental series, with parallel passages from the Bible and classical authors. In his interesting introduction, he discusses the difficult question as to whether an acquaintance with the Christian Scriptures may have exercised some influence on the religious ideas of the Hindus in the earlier centuries of our era; an influence which has been asserted to be traceable more especially in the Bhagavad-gītā, the famous philosophical episode of the Mahābhārata. Although Muir does not arrive at any definite conclusion on this point, he seems, on the whole, to incline to the assumption of an independent origin of the work in question. The particular object he had in view in making this collection may best be stated in his own words:—"But however the question of the obligations of the Bhagavad-gītā, or of some other parts of the Mahābhārata, to Christianity may be decided, the decision can scarcely affect the determination of the farther and very different question of the originality or otherwise, as far as any foreign influences are concerned, of the great bulk of the moral and religious sentiments embraced in my collection. These sentiments and observations are the natural expression of the feelings and experiences of universal humanity; and the higher and nobler portion of them cannot be regarded as peculiar to Christianity. The

correctness of this view is placed beyond a doubt by the parallels which I have adduced from classical writers. It is my impression, however, that the sentiments of humanity, mercy, forgiveness, and unselfishness are more natural to the Indian than to the Greek and Roman authors, unless, perhaps, in the case of those of the latter who were influenced by philosophical speculation. This tenderness of Indian sentiment may possibly have been in part derived from Buddhism, which, however, itself was of purely Indian growth." The publication of this volume seems to have left a void in his mind which, deepened by the loss of his good and gentle sister, who had been for many years the faithful companion of his solitary life, had at times a depressing influence on his spirits. Still, however, he pursued his course of reading, and only a few months before his death he issued to his friends another small collection of metrical translations from the Mahābhārata, including the highly poetical episode of Sāvitrī.

While the literary researches of John Muir have gained for him a place in the foremost rank of Sanskritists, and have thus contributed in a remarkable degree to the credit of Scottish scholarship in an important branch of Oriental studies—as those of his distinguished brother, Sir William Muir, have done in another branch —John Muir deserves to be not less gratefully remembered by his countrymen for the eminent services he has rendered to the cause of education in Scotland. The want of a recognised medium of instruction on his favourite subjects of study in any of the Scottish universities induced him, in 1862, to offer to the Senatus of the Edinburgh University the sum of £4000 for the foundation of a chair of Sanskrit and Comparative Philology; on the condition that the interest of this capital should be supplemented by an annual grant from Government of the same amount. In 1876 this munificent gift was increased by a further sum yielding an addition to the emoluments of the chair of £50 a year. In one respect Dr. Muir's expectations in founding the chair were disappointed. It appears that, in drawing up the deed of endowment, he had intended to provide, beside the systematic courses of instruction, for annual courses of lectures of a more popular kind to be open to any non-matriculated persons that might wish to attend them. Unfortunately, however, the terms of the deed were not sufficiently definite

to exclude an interpretation more in harmony with the existing arrangements of university teaching; and, though the question was long and carefully considered by the Senatus—I myself deeming it my duty to support Dr. Muir's interpretation—they found it impossible to consent to what might have proved a somewhat inconvenient precedent. Nevertheless, Dr. Muir continued to the last to show the warmest interest in the objects of the chair, by giving annual prizes for distinguished attainment in the several classes. He also offered a (still available) prize of £100 to the first student that should take the degree of Doctor of Science in the department of Sanskrit and Comparative Philology. To his liberality the University Library owes a very considerable portion of its Oriental and Philological books. The connection of his name with our University, in this respect, has been further strengthened, since his death, through the presentation, by Sir William Muir, of the large collection of Oriental and Philological books left by his brother. In accepting this splendid gift, the Library Committee resolved that this collection, together with the books previously presented by Dr. Muir, should be kept separate from the general library, under the designation of the "Muir Collection." Dr. Muir also took a prominent part in the founding, in 1868, of the Shaw scholarship in mental philosophy (in honour of his uncle, Sir John Shaw, at one time Lord Mayor of London, and a director of the East India Company); and in originating and conducting the Association for Promoting the Better Endowment of Edinburgh University, having acted for ten years as honorary secretary of that most useful society. Dr. Muir's interest and liberality were not, however, confined to the University of Edinburgh; but the other Scottish universities also, I believe, received from him numerous donations of books; and to the Berlin University he presented, a few months before his death, the sum of £50, to form the nucleus for a scholarship in Sanskrit philology. In recognition of his services to higher education, Dr. Muir was appointed a member of the last Scottish Universities Commission. To the report of the commissioners Dr. Muir, in accordance with his principles, added a note urging the consideration of the advisability of the theological chairs in the universities being thrown open to members of all the churches.

John Muir's eminence as a scholar obtained for him the honorary degrees of D.C.L. from the Oxford University, of LL.D. from the Edinburgh University, and of Doctor of Philosophy from the University of Bonn; as well as the title of a corresponding member of the French Academy, the Royal Prussian Academy of Sciences, and a foreign member of the Leyden Society for the Cultivation of Dutch Literature. He joined the Royal Society of Edinburgh in 1861, and at their meeting on Feb. 16, 1863, he read, by request of the Council, a highly interesting paper "On the Recent Progress of Sanskrit Studies." This and several other papers contributed by him were published in the Society's Transactions.

John Muir was loved by all who knew him for his extreme kind-heartedness and truthfulness, his love of humanity, and the purity of his life. His memory ought to be dear to every Scotsman.

Dr. Charles Morehead. By James Sanderson, F.R.C.S.E., Deputy Inspector-General of Hospitals, Madras Army.

Dr. Charles Morehead, C.I.E., M.D. Edin., F.R.C.P. Lond., and Honorary Surgeon to Her Majesty, was born in Edinburgh in 1807, and died suddenly at Wilton Castle, Redcar, Yorkshire, on the 24th of August 1882, in the 75th year of his age. He was the second son of the Rev. Robert Morehead, D.D., Dean of Edinburgh, and afterwards rector of Easington, Yorkshire. His mother was Margaret, daughter of the Rev. Charles Wilson, Professor of Church History in the University of St. Andrews.

He was educated at the High School of Edinburgh, for which through life he cherished a strong affection, and at the time of his death was one of the very few remaining members of the Carson Club. He entered the medical classes in the University of Edinburgh about 1825, where he distinguished himself as a student more particularly in the science classes. In the early part of his studies he manifested great ardour in the study of clinical medicine, and soon attracted the attention of Professor Alison, whose clerk he became at the end of his course.

Dr. Morehead graduated as M.D. in 1828, and thereafter prosecuted his medical studies for upwards of a year in Paris, under the

famous physician Louis, with whom he kept up an intimate correspondence till his death.

At the age of 22, Dr. Morehead entered the Bombay Medical Service, and was soon placed on the personal staff of Sir Robert Grant, Governor of Bombay, and continued to serve in that capacity till Sir Robert's death in 1838. He was president of the Medical and Physical Society of Bombay from 1837 to 1859, and during that time contributed largely to the *Transactions* of the Society. He acted also as secretary to the Board of Native Education from its establishment in 1840 to 1845. In connection with this last subject he long ably advocated in various ways, and through various channels, the opinion that the instruction and education of the natives of India should be through the medium of the English language; and at last, in 1845, had the satisfaction of seeing his ideas carried into practical effect in the founding of the Grant Medical College, one of the chief features of which was the education of the natives by means of the English language. The practice has now for long been universally adopted, with the best results, both as regards the governors and governed of our Indian Empire.

About this time, the large and well-equipped native hospital, named after Sir Jamsetjee Jejeebhoy, was established at the joint expense of the Government and Sir Jamsetjee, for practical instruction in clinical teaching. To Dr. Morehead belongs the merit of introducing this branch of medical training, which at that time did not form a regular part of the curriculum even in the medical schools of the United Kingdom. It was but fitting that Dr. Morehead was appointed first Principal and first Professor of Medicine to the College, and first Physician to the Hospital.

A bust of Dr. Morehead has been placed in the hall of the College as a memorial of its eminent Principal and Professor, by the students and friends of the college.

During these years Dr. Morehead was patiently collecting in the course of his practice as a physician, and from other available sources, observations on the diseases of India, the results of which he published in his valuable work on *Indian Diseases*, a book which still holds its place as a standard authority in the treatment of the tropical diseases of Hindostan. His last service to the pro-

fession, before leaving India for England in 1859, was the characteristic one of the formation of a society composed of the old students of the Grant College, which has served not merely as a bond of union, but been also productive of no inconsiderable practical advantages to its members.

On his return from India he was offered the professorship of medicine in Netley Hospital, then just founded, which, however, the state of his health obliged him to decline.

In 1862, he retired from the service with the rank of Deputy Inspector-General of Hospitals; in 1857 he was appointed Honorary Surgeon to the Queen, and in 1881 was made a Companion of the Order of the Indian Empire.

Dr. Morehead will be long and best known by his important researches into the diseases of India, based on a truly scientific diagnosis, and so successfully set forth in his great work on the subject; and by the insight and strength of will by which he succeeded in making clinical medicine so prominent a feature of the medical education of natives in Western India.

It only remains to add that in 1875 Dr. Morehead published the Memorials of the Life and Writings of his Father, the Rev. Dr. Robert Morehead. He was elected a Fellow of this Society on the 15th January 1860.

Friedrich Wöhler. By Professor Dittmar.

On the 23rd of September 1882, this great man closed his eyes to go to rest after a noble and glorious career in the service of chemical science, extending over two generations. Some sixty years ago, when the elementary nature of chlorine had just been established and the isolation of cyanogen was still a novelty, young Wöhler already worked as an investigator,—the same Wöhler who rejoiced with us over the synthesis of indigo. Of the world of chemical discoveries that lie between he *magna pars fuit*.

To desire to know something of the mould of external circumstances into which such a great life was cast is no vulgar curiosity. The writer, accordingly, had no hesitation in availing himself of an opportunity which presented itself some time ago for obtaining

authentic information from Mr. A. Wöhler of Schönhof of Bockenheim, the great chemist's only son. From Mr. Wöhler's letter we extract the following biographical sketch :—

Friedrich Wöhler was born on the 31st July 1800, in Eschersheim, a village near Frankfort-on-the-Main. He received his first instruction from his father, a man well versed in economical and physical science, as also in philosophy and pedagogics ; and, besides, attended the village school in Rödelheim, where his father owned a small landed estate. In 1812 the family removed to Frankfurt, where he attended the gymnasium, and by the kindness of a scientific friend, Dr. Buch, who, besides a thorough knowledge of the subjects, possessed the necessary appliances, was introduced to the study of mineralogy, more especially, but also of chemistry and physics. [Conjointly with this Dr. Buch, Wöhler, as early as 1821, published an investigation on "Selenium in a Bohemian mineral,"—his *debut* as an investigator.] After having completed his curriculum at the gymnasium, Wöhler went to the University of Marburg as a student of medicine. In 1821 he left Marburg to continue his studies at Heidelberg, where he took his degree as doctor of medicine but, on the advice of Leopold Gmelin, decided upon devoting himself henceforth to chemistry. He completed his chemical education at Stockholm under Berzelius, in whose laboratory he worked for a considerable time, and with whom, during his subsequent life, he maintained the most friendly relations. While in Sweden he took part in a scientific expedition through Norway, which made him acquainted with the brothers Brogniart and Humphrey Davy.

After his return from Sweden, in 1825, he accepted a call to Berlin as teacher of chemistry in the then newly-erected Gewerbschule, and remained there until 1832, when family affairs caused him to take up his abode in Cassel. In 1836 Wöhler became Professor of Chemistry in the Medical Faculty of the University of Göttingen, which office, in his case, was combined with that of Inspector-General of Pharmacy for Hanover. He held his chair to the day of his death on the 23rd September 1882. After only three days illness he died, deeply mourned by his widow, children, grand children, and great-grand children, in the 83rd year of his life.

To pass now to what for us, as part of the republic of science, is Wöhler's real biography.

The superabundance of experimental genius in the chemical camp must account for the fact that the border-lands between chemistry proper and the collateral sciences of physics, physiology, &c., have been cultivated chiefly by men who called themselves chemists. It is there that Bunsen, Graham, Kopp, Liebig, Regnault, gathered part of their laurels. If it were possible to characterise Wöhler by one stroke of the pen, we should say that of such border-land work he did very little—all his work lies in the very core of the science; but on this only relatively narrow field he simply ranks with Scheele, no other name, except perhaps that of Berzelius, could fitly be placed alongside of these two.

To begin with Wöhler's minor contributions, and at the same time qualify what we have just said of him in a negative sense, let us state that Wöhler, while a student of medicine in Heidelberg, published a thesis on the excretion of substances by the kidneys, for which a prize was awarded to him by the Medical Faculty of that University in 1823. Many years later he resumed this subject conjointly with Frerichs; the memoir is in the *Annalen der Chemie* for 1848 (vol. lxv. p. 325). In this connection we may state that we owe to Wöhler the best method for the detection of arsenic and other mineral poisons in complex organic mixtures. It is described shortly in his *Mineral Analyse in Beispielen.* (The original memoir is in the *Annalen,* for 1849, vol. lxix. p. 364.)

We have not been able to find out exactly what Wöhler did while in Berzelius's laboratory, and presume that, as a sensible man, he there mainly confined himself to learning the great master's methods. Nothing but a short notice on "Improvements in the Preparation of Potassium," dates from the Stockholm period. It is significant, however, as forming the small beginning of a brilliant series of researches on the isolation of elementary substances and their properties, a subject for which he evidently had a great love, as he always comes back to it in the intervals of other work. In 1827 he, for the first time, succeeded in isolating aluminium, the metal of clay, by means of a method which was soon found to be more generally applicable. Alumina, like many other metallic oxides, is not reducible by electrolysis or by the action of charcoal at any temperature. But, when heated with charcoal in chlorine gas, it passes into the state of a volatile chloride. What Wöhler found was that this chloride when

heated with potassium or sodium, readily gives up its chlorine and assumes the elementary form. The aluminium which Wöhler thus obtained was a grey powder; but in 1845 he succeeded in producing the metal in the shape of well-fused, fully metallic globules. Wöhler, on this second occasion, correctly ascertained all the properties which everybody now knows to be characteristic of this metal, and it is as well to add that where Wöhler's aluminium differed from what now occurs in commerce under this name, it differed to its own advantage. That Wöhler should not have seen the practical importance of his discovery, is what we refuse to believe; if he never even suggested an attempt to manufacture the metal industrially, this is the natural consequence of the circumstances in which he was placed. For these *we* now should feel thankful; if, instead of quiet little Göttingen, a place like Birmingham had been his abode, he would, perhaps, have been lost to science for all the rest of his life.

The earlier aluminium research was followed, in 1828, by the isolation of beryllium and yttrium. These earlier metal reductions fall into the Berlin period. While in Cassel he worked out processes for the manufacture of nickel free from arsenic, and this laid the foundation for what is now a flourishing chemical industry in Germany. The several methods for the analysis of nickel and cobalt ores which he describes in his *Mineral-Analyse* are, we presume, an incidental outcome of this work. This subject was one of his favourite topics; as late as 1877 we see him coming back to it in the publication of a short cut for the separation of nickel and cobalt from arsenic and iron.

In 1849 metallic *titanium* arrested his attention. Since the days of Wollaston those beautiful copper-like cubes which are occasionally met with in blown-out blast furnaces, had been supposed to be metallic titanium pure and simple. Wöhler observed that the reputed metal, when fused with caustic alkali, emitted torrents of ammonia, and on further inquiry ascertained the crystals to be a ternary compound, containing the elements of a nitride and of a cyanide of the metal. In pursuance of this research Wöhler taught us how to prepare real titanium and really pure titanic acid.

In 1854 Deville's energetic attempts to produce aluminium industrially, caused Wöhler to turn his attention again to this early

and almost forgotten child of his genius. His first incentive, no doubt, was the natural and just desire to claim his right as the real discoverer of what Deville, in his ignorance of foreign scientific work, quite honestly thought he had been the first to find out. This priority dispute came to a very satisfactory issue. Deville, after a little pardonable hesitation, bravely acknowledged Wöhler's priority, and the two henceforth were friends and worked together.

The first fruit of this happy union was a memorable joint research (published in 1856 and 1857), which led to their discovery of an adamantine and of a graphitoidal—in addition to the long known amorphous—modification of *boron*. This graphitoidal species subsequently (in their own hands) proved a mistake; but the adamantine modification lives to this day as a true analogue of ordinary (carbon) diamond.

From boron to *silicon* is an easy transition, so we need not wonder at finding Wöhler, in 1857, engaged (conjointly with the physicist Buff) in a research on new compounds of silicon. On electrolysing a solution of common salt with silicon—containing aluminium, as a positive electrode, they obtained a self-inflammable gas which they recognised as hydrogen contaminated with the previously unknown hydride of silicon SiH_4, which body Wöhler subsequently (with the co-operation of Martius) obtained in a state of greater purity. Wöhler and Buff also obtained, though in an impure state, what were subsequently recognised by Friedel and Ladenburg as silicon-chloroform and as silicon-formic anhydride.

Within the limits of this notice we could not reasonably attempt anything like a *complete* account of Wöhler's numerous researches on inorganic subjects; but we must not omit to at least allude to his researches on metallic or semi-metallic *nitrides*. What we know of this as yet little understood class of bodies, with barely an exception, came out of his laboratory, if it was not done by himself in the strict sense of the word.

We also can only refer to the numerous processes which Wöhler, in the course of his long laboratory practice, has worked out for the preparation of pure chemicals, and for the execution of exact analytical separations. Wöhler had better things to do than to take up analytical problems for their own sake; but what he did in this

direction incidentally—with his left hand, so to say, while his right was engaged in greater work—amounts to a great deal. With the two exceptions of Heinrich Rose and Robert Bunsen, no man has done more than Wöhler has for the perfection of analytical methods. The analysis of *meteorites* was one of his favourite specialties, and one of his results in regard to these must not be withheld from a Scottish Society. We refer to his discovery of organic matter in a meteorite which he examined in 1864.

If Wöhler had done nothing more than what has been referred to explicitly or implicitly in the above, his work, even for the fifty years of unbroken health which Providence granted him for its execution, would have to be admitted to be both *multa* and *multum*; but far more important than even all that are his researches in *organic chemistry*.

Wöhler's first organic research dates from 1821, when (as a student in Heidelberg) he discovered *persulphocyanic* acid, a compound of sulphur with the sulphocyanic acid which, the year before, had been analysed by Berzelius. But fraught with greater consequences was his discovery of cyanic acid in 1822. Organic chemistry might be said to date from it in two senses. When, in 1828, Wöhler prepared the ammonic salt of his acid, he was astonished to find that the salt, although made by what appeared to be a straight-forward double decomposition, did not exhibit the character of an ammonia salt at all, but turned out to be identical with *urea*, a substance which heretofore had been known only as one of the *organic* components of urine. A momentous discovery for that time! A wide and impassable gulf then, in the minds of chemists, separated the mineral from the organic kingdom. In organic bodies all appeared to be derivable from their elements by a succession of acts of binary combination; the full analysis of such a body contained in itself the full instruction for its synthetical production in the laboratory. Organic substances, on the other hand, were supposed to be things of an entirely different order; in them the few elements which they all consist of, were assumed to be united with one another, each with each, in a mysterious manner, which could be brought about only by the agency of *vital force*. Vital force, it was now seen, had nothing to do with the formation of urea at any rate. The gulf was bridged over, and a

great and new morning full of the highest promise dawned over chemistry. If the promise was more than fulfilled, if organic chemistry from a mere possibility developed into a reality, we owe this chiefly to the great researches which were carried out conjointly by Wöhler and Liebig.

Two years after Wöhler had discovered cyanic acid, Liebig and Gay-Lussac inquired into the nature of that dangerously explosive compound known as fulminate of mercury (which had been discovered twenty-four years before by Howard), and proved it to be the mercuric salt of an acid which, although clearly a thing of its own kind, had precisely the same elementary composition as Wöhler's cyanic acid, a result which, at that time, appeared hardly credible. These doubts, however, were set to rest by a joint investigation on the oxygenated acids of cyanogen, which Liebig and Wöhler published in 1830. In their research they proved, both analytically and synthetically, that cyanic and cyanuric acid, although distinct bodies, have the same elementary composition, and that the former, when simply kept in a sealed-up tube, gradually passes *wholly* into a porcelain-like neutral solid, cyamelide, which is widely different from either. By these discoveries, and by Wöhler's synthesis of urea, the fact of isomerism was firmly established. Compared with this great conquest their joint work on mellitic acid (1830), and on sulphovinic acid (1831), appears small; it sinks into insignificance when viewed in the light of their immortal researches on bitter almond oil and on uric acid.

In 1832 bitter-almond oil was supposed to be to bitter almonds what a hundred and one other essential oils are to their vegetable sources. Of its chemistry nothing was known except the fact that it contains loosely combined prussic acid, and that, when kept for a long time, it is liable to deposit a crystalline solid, as various other essential oils do. Liebig and Wöhler, being struck by the absence from even powdered bitter almonds of the intense smell characteristic of the oil, set about tracing the latter to its origin, and soon solved the question. In 1830 Robiquet and Boutron-Charlard had succeeded in extracting from bitter almonds a crystalline nitrogenous solid, soluble without decomposition in alcohol and in water, which they called amygdaline. What Liebig and Wöhler found, was that when bitter-almond meal is mashed up with water, this amygdaline,

by the action of the water and a ferment (common to both sweet and bitter almonds), breaks up into sugar, prussic acid, and bitter-almond oil. They also succeeded in separating the prussic acid from the distilled oil, and found the thus purified oil to be a *non*-poisonous liquid of the composition C_7H_6O. This liquid, when exposed to the air, readily takes up oxygen and assumes the form of a solid which is identical, at the same time, with the quasi-stearoptene of the oil and with Scheele's benzoic acid $C_7H_6O_2$. When treated with chlorine the purified oil yields a chloride $C_7H_5O \cdot Cl$; the chlorine of which, by treatment with the respective potassium compounds, is displaced by its equivalent in bromine, iodine, sulphur, cyanogen, and, on treatment with ammonia, by the group NH_2. Water converts it into hydrochloric and benzoic acids. In all these reactions the group C_7H_5O holds together, it moves forwards and backwards as if it were a compound element. A common-place enough fact in the eyes of the chemical student of 1882, but a most wonderful revelation to the chemist of 1832. Berzelius, who certainly was not much given to dealing in superlatives, greeted the discovery in his *Jahresbericht* as opening up a new era in organic chemistry, and, rejecting the prosaic name of benzoyl which Wöhler and Liebig had given to their radical, proposed to name it proine or orthrine, from πρωΐ the beginning of the day, or orthrine, from ὄρθρος the dawn of the morning.

It is part of the glory of the two men that, in regard to none of their joint researches, the outer world ever had any hint given to it as to what was the one's and what was the other's share in the work although they rarely worked together in the same laboratory. Wöhler would work away in Göttingen and Liebig in Giessen; they only compared notes and slumped the whole into one memoir.

Going by what we know of the genius of the two great men, we should say that in the benzoyl research Liebig's hand is more distinctly visible, while the one on *uric acid* (published 1837) impresses one as having more of the Wöhler element in it. Uric acid was discovered by Scheele in 1776. It is a constant component of urine, but more readily prepared from the excrement of birds and serpents. Its general properties and its relations to bases are all that was known of it when Liebig and Wöhler took it in hand. Apart from an isolated observation of

Brugnatelli's, who as early as 1817 obtained from it, by oxidation, a crystalline product, which he called "erythric acid," Wöhler and Liebig, by, in a sense following in Brugnatelli's footsteps, but looking with sharper chemical eyes, discovered, instead of one, a whole host of derivatives, the disentanglement of which, even to them, must have been a tough problem. But they did not rest before each and every one of the bodies had given a clear account of itself. Liebig, somewhere in his Chemical Letters, *spricht ein grosses wort gelassen aus,* "of any scientific investigation worthy of the name, the main results can be summed up in a few words." It holds for his and Wöhler's case. Uric acid when oxidised behaves as if it were potential urea plus potential mesoxalic acid $C_3O_3.(OH)_2$. Part of the urea comes out as such; the rest unites with the mesoxalic acid into a "ureide" with elimination of water, formed from the two (HO)'s of the acid and two of the hydrogens in one molecule of the urea. This is alloxan (Brugnatelli's erythric acid in a pure state). But alloxan itself, when further oxydised, loses part of its carbon as carbonic acid and becomes *parabanic acid,* the ureide of oxalic acid $C_2O_2(OH)_2$. Either ureide, when treated with caustic alkali, takes up first one and then a second molecule of water to form, in the first instance, alloxanic and oxaluric (hydro-parabanic) acid, in the second, urea plus mesoxalic and oxalic acid respectively. Either ureide, when subjected to reducing agents, takes up one atom of hydrogen per molecule and is reduced, the one to alloxantine, the other to oxalantine. A more limited oxidation of uric acid leads to the formation of allantoine which, before Liebig and Wöhler, had been known only as a component of the allantois-liquid of the cow. These few notes do not pretend to do justice to the great research; but they will suffice to give to the general reader a notion of its importance. Liebig and Wöhler's work—apart from a few isolated though not inglorious attempts—was not continued until Baeyer took it up and rounded it off. Baeyer has enabled us to see clearly certain relations which had before been obscure; but it is worthy of notice that, while overhauling the whole of Liebig and Wöhler's work, he found nothing to rectify; it all proved solid masonry on which he was able to build without resetting a single stone.

After their uric acid research the ways of Wöhler and Liebig

diverged. The latter continued to prosecute organic research; the former turned his attention more to inorganic subjects, not exclusively though, as the well-known research on narcotine (which was carried out in his laboratory, part by himself, part by Blyth, and published in 1848) is alone sufficient to prove.

As a *teacher* Wöhler ranks with Liebig and Berzelius. In a sense he was the greatest of the three. Berzelius, we believe, never had the facilities afforded to him for teaching large numbers of students in his laboratory; and as to Liebig, even he lacked the many-sidedness which formed so characteristic a feature in the Gottingen laboratory as long as it really was under Wöhler's personal direction. One student might wish to work on organic chemistry, another on minerals, a third on metallurgy, a fourth on rare elements; let them all go to Wöhler and they all, like the fifth or sixth, would find themselves in the right place.

That Wöhler in these circumstances should have been able to do much of literary work would appear incredible if we did not know it to be so. His *Grundriss der Chemie*, which he published anonymously at first, has passed through many editions and been translated into various foreign languages; never, we are sorry to say, into English. A more valuable teaching book still, and more unique in its character, is his excellent *Practische Uebungen in der chemischen Analyse* (entitled in the second edition *Mineral-Analyse in Beispielen*), which has been translated twice into English, once in this country by Hofmann, and a second time (from the second German edition) in America. To a man like him the compilation of either book probably gave little trouble; what must have taken up a very large portion of his valuable time, are his translations of Berzelius's *Lehrbuch der Chemie*, and of all the many successive volumes of Berzelius's *Jahresbericht*, which works only thus became really available to the scientific world at large. We must not omit to state in this connection that since 1838 Wöhler has been one of the editors of Liebig's *Annalen*.

Wöhler's last publication dates from 1880. It treats of a new kind of galvanic element in which the one metal aluminium serves for either pole. We mention this as showing that he continued working to almost the edge of his grave.

Sir John Rose Cormack. By Professor Maclagan.

John Rose Cormack was born on 1st March 1815, on the classic banks of Gala Water, in the Manse of Stow, of which parish his father, the Rev. John Cormack, D.D., was minister. His mother belonged to the old northern clan of Rose, her brother, Sir John Rose of Holm, being a distinguished Indian officer.

Cormack's primary education, like that of so many Scotchmen who have risen to distinction, was got in the parish school; his secondary education at the High School of Edinburgh; and his professional education at the University of Edinburgh, in which he became a student of medicine. During his whole University career he was a hard-working student, and took the degree of M.D. on 1st August 1837, on which occasion he got a University gold medal for his thesis on the subject of Death from the Entry of Air into the Veins. On this subject subsequently, both in a surgical, obstetrical, and medico-legal aspect, he made some further observations in the years 1838 and 1850, and he again made it the subject of a thesis when he took the degree of M.D. of Paris in 1870.

This was not, however, his first attempt at authorship, for he had the year before his graduation gained the prize of the Harveian Society of Edinburgh for an essay on Creasote, which he subsequently published as a thin octavo. It is curious to note the affection which Cormack retained for his first scientific love, for Creasote figures not only in many of his prescriptions in future years, but we find that creasote water (cresylic acid) was used by him in his surgical experience during the siege of Paris in 1871, instead of the closely allied carbolic acid now so familiar to everybody.

Having taken his degree with gold medal honours in 1837, he went to Paris, where he followed out his professional studies, chiefly under Andral as regarded medicine, and Velpeau as regarded surgery. He then returned to Scotland, and determined to settle in practice in Edinburgh, and became a Fellow of the Royal College of Physicians of Edinburgh 2nd February 1841. Practice came scantily, but Cormack could not be idle. He became a lecturer on Medical Jurisprudence in the Extra Academical School, and then

he entered upon that course of medical journalism which was a leading characteristic of a great part of his subsequent life. In 1842, under the name of the *London and Edinburgh Medical Journal,* he started that monthly Journal of Medicine, which, under some changes of designation and varieties of editorship, continues to be an important vehicle of scientific and practical medicine; its 334th number being that for April 1883.

In 1842 he was appointed physician to the Fever Hospital in connection with the Royal Infirmary, and in this capacity he had a large experience of the remarkable epidemic of Relapsing Fever, which in 1843 occurred in Edinburgh and other towns in Scotland. The labour which he bestowed on his hospital work, and the accurate details which he preserved of his cases, are a striking character of the hard-working nature of the man. His observations were given to the profession in the form of a book on this epidemic, which had, up to that time, not been so fully and accurately described, and he subsequently published some additional remarks on the subject in the *London Medical Gazette* for April 1849.

Cormack's journalistic venture, and his work as a hospital physician, did not, however, bring him much in the way of practice, and accordingly he migrated to London, where he remained but a short while, settling in practice in its neighbourhood at Putney.

In the English metropolis his journalistic propensities again manifested themselves. Besides writing leaders and other unsigned articles in some of the London medical journals, he became editor of the *Association Medical Journal,* the organ of the Provincial Medical Association. But this he gave up in 1856. The journal was much improved under his management, and still exists as the *British Medical Journal,* the organ of that large and influential body the British Medical Association.

Cormack did not, however, succeed in practice at Putney. His journalism brought him much notoriety and some ill-will, but it was perhaps itself adverse to his success as a practitioner, and it was necessary for him to look to something which would add to the means of maintaining a rising family.

An elderly lady who resided at Tours in France required a British medical man to be always with her, and accordingly he went to France, with the life and language of which he was familiar.

This, however, was a source of income which could not be otherwise than temporary, and in the course of time his patient died, and he had once more to look for a field of practice. He went to Paris, and to enable him to practise there he took the degree of M.D. of Paris in 1870, using for the thesis which he was bound to present to the Faculty the old subject of the Entrance of Air into the Veins, with the addition of his further observations which have been already mentioned. The sun seemed at last to be shining on his side of the hedge. Sir Joseph Oliffe, then the leading English physician in Paris, was old, and soon died, and Cormack got into good practice among the English, and to some extent among the French community. He was appointed physician to the British Embassy, and all seemed to be getting on prosperously with him. But soon the Franco-German war broke out, and with it came the downfall of the Second Empire. Paris was besieged by the Germans, and after this disaster the Commune followed. Cormack's prospects of an easy-going practice were thrown to the winds, and, like every one in Paris, he felt how hard are the uses of adversity. But now it was in this dark hour of disaster that Cormack really came forth in great form and showed what was in the man. Amid the silent horrors of a severe winter, and the loud-sounding horrors of foreign invasion and civil war, he showed that he was a good man, by bringing out of his professional treasure things new and old. It was not now the work of a civil practitioner, but that of a military medical officer, that he had to undertake. If anything be needed to prove the propriety of every aspirant to the medical profession being ascertained, before he gets his degree, to be qualified, not only in one, but in all the practical branches of his profession, Cormack's case would supply it. His whole work hitherto had been essentially that of a physician, he now came out strongly as an operating surgeon, bringing to the front the surgical lessons he had in his youth received from Lister, Syme, and others in the surgical wards in the Edinburgh Infirmary, and some of his cases were really triumphant results of conservative surgery. It was in the *Ambulance Anglaise*, established near his then residence, and maintained entirely by Sir Richard Wallace, that he did his surgical work, and the writer of this notice saw one of his triumphs in the person of the Communist, Alphonse Brunet, whose arm he saved by resection

of the shoulder joint after it had been shattered by a rifle bullet For his good and courageous work at this time he was rewarded both by the British and French Governments, being knighted by our Queen, and made a Chevalier of the Legion of Honour.

Peace being at length restored, Cormack returned to his more usual work of physician, but now just as the sun of prosperity had begun to shine upon him, and when he had received honours of which any man might be proud, the end drew near. He had never fairly got over the effects of his exertions during the war. Although still looking fairly well, and in his usual good temper and spirits, he was a sufferer from bladder disease, and he died on 13th May 1882.

This was not the only bereavement which the Franco-German and Commune wars brought upon the Cormack family. In 1842 Cormack had married Miss Hine, the daughter of a merchant at Trelawney, Jamaica. She, too, was one of the victims of these times of political trouble. She never recovered from the effects of the privation and distress to which all Paris at that time had been more or less subjected. The inclemency of a hard snowy winter, the bursting of shells and the rattle of the fusillade, the crash of falling houses, the want of due supplies of food, and the necessity of waiting, sometimes for hours, in the *queue* of persons who had to go, single file, to the bakers' shops to get their loaf of bread, were not likely to leave unshattered the health of a lady born in the West Indies, and who had been the mother of eleven children, and no one therefore need be surprised to learn that in three months Lady Cormack followed her husband to the grave. She died on 19th July 1882.

Cormack had had eleven children, and among his trials of life was the mortality which occurred among them. Two died in childhood, of scarlet fever and typhoid respectively. One who was grown up died in Brazil, of phthisis after yellow fever. In 1876 death dealt heavy blows on Cormack. His daughter, Mrs Lyon, died in India soon after giving birth to a boy, who was a great solace to his grandfather in his last years; and within a week of this event in India he lost in Paris his son Bailey Cormack, who was a promising young member of the medical profession—his father's right hand man in his surgical work in the war time, and whose excellent qualities cannot be better recorded than they are in the

following preface to Sir John's account of his patient Brunet, whose case has already been alluded to :—

" For nearly a year I had not seen him (Brunet) till we met on the 29th April 1876, at the funeral of my dear son John Rose Bailey Cormack. Weeping bitterly he grasped my hand, and said, ' I never liked any one so much as Dr Bailey : he did not know what fear was, but he was to me and all the other wounded kind as a brother and gentle as a woman.' In justice to Brunet, I cannot refrain from here placing on record his tender appreciation and beautiful tribute to my late son—my skilful assistant in most trying circumstances—one who was the joy and hope of my life. It is pleasant to record that even men of ' Communistic type ' are amenable to kindness, and can love as well as hate their fellow-men."

Shortly after Bailey Cormack's death, his sister Margaret died of pleurisy, induced, it was thought, by nursing her brother.

Five of the family survive, one married and three unmarried daughters, and a son, Charles Edward, who, following his father's footsteps, is now a student of medicine.

Cormack was a voluminous writer, exclusive of what he did in the way of journalism. In 1876, under the title of *Clinical Studies*, he republished his various detached writings in two volumes. These embrace such a variety of subjects besides those already noticed, as cholera, scarlatina, granular kidney, several gynæcological matters, infantile convulsions, diphtheria, syphilis, concussion of the brain, and certain forms of insanity. It can by no means be said that all these are of equal clinical importance, but all of them manifest good observing power and determination to study the subject fully.

It was a considerable shock to many of Cormack's friends to learn after his death that he had left his family in straitened circumstances. It is revealing no secret to mention this, for it was prominently brought forward by the *British Medical Journal* in the very practical form of advocating a memorial subscription for the benefit of Lady Cormack and her family. The way in which this was responded to, showed that Cormack had had many friends who esteemed him highly. It did not surprise those, however, who knew, nor will it surprise any one who hears the narrative of his

chequered life, as stated in this notice. It is obvious that Cormack never got into that steady sort of practice which fills the purse. His journalistic work was an impediment rather than a help to him. It is not easy to see why he did not succeed in practice, especially at Putney, where he had a good opening. It was not want of professional knowledge; his writings show that this was full and extensive. It was nothing wrong with his *morale* or his relations with religion, for although he did not carry a broad phylactery, or enlarge the border of his garments, he was essentially a quietly and unobtrusively Christian man. It is neither pleasing nor profitable to pursue this theme, and one can only fall back upon the trite expression of the country of his adoption, that he wanted the "*Je ne sais quoi*," the absence of which has hindered the success of many a man as full of erudition and observing power as himself.

Cormack was a warm and steadfast friend, and the writer of these lines desires to record that this was the constant relation to himself of the subject of this obituary notice.

Sir Charles Wyville Thomson, F.R.SS. L. and E. By Peter Redfern, M.D. Lond.

Charles Wyville Thomson was born on the 5th of March 1830 at Bonsyde, a small property in Linlithgowshire, which had long been in his family. His father was the late Mr Andrew Thomson, who spent most of his life abroad as a surgeon in the service of the Honourable East India Company. His mother was Sarah Ann Drummond, the only daughter of Dr Wyville Smith, Inspector of Military Hospitals. His grandfather was a distinguished Edinburgh clergyman, and his great-grandfather was "Principall Clerke of Chancellary" at the time of the Rebellion of 1745. His father was rather a strict disciplinarian, and expected to see successive distinctions at school and college following in the wake of the admirable education which he placed at the command of his son.

These were stirring times for Scotland. Unembarrassed by troubles from without, her people were continually struggling for intellectual advancement. They furnished and maintained schools

of the highest class in the larger towns, and such as offered classical training to the youth, in the whole length and breadth of the country, in preparation for the Universities. In Edinburgh, Glasgow, Aberdeen, and St Andrews, she gave university education to twice as large a proportion of her population as Prussia did; and it is not to be wondered at that her sons have distinguished themselves in every corner of the globe. In settling in the north of Ireland, they introduced order and liberty, manufactures and prosperity, and raised one of its towns at least to take its place amongst the most enterprising and prosperous in the United Kingdom, whilst they themselves neither required soldiers nor police for the maintenance of order and securing the advantages of the administration of just laws. These were the days of Thomson's youth.

The mother's fond affection for her son led her to anticipate his wishes—to supply his wants—and to present him to his father as almost faultless in every relation of his early life. The son's devotion to his parents led him to make many an effort for their gratification, no less than for his own success in life. His early training was at Merchiston Castle School, when it was under the management of Mr Charles Chalmers, brother of the famous divine. Mr Thomas Chalmers, of Longcroft, near Linlithgow, and son of the then proprietor of the school, stated at a meeting of the commissioners of supply for the county of Linlithgow, that "when young Thomson entered the school he himself had passed from being a scholar to the position of a master, and thus became Thomson's first teacher in some of those branches of science in which he afterwards became so eminent." Mr Chalmers adds—"No doubt the lessons he received were of a very elementary description, still I may be allowed to recall with some satisfaction, if not pride, the happy early days of our intercourse, when, with botanical boxes or geological hammers in hand, we rambled on Saturday holidays, or in the long summer evenings, among the woods of Braid and Colinton, or over the uplands of the beautiful Pentland Hills, in search of some of the interesting flora or geological and mineral specimens in which the neighbourood of Edinburgh so richly abounds," and then describes the exultation and satisfaction with which they returned with any new or rare specimen after a long day's excursion. Mr Chalmers says that Thomson was a universal favourite with his

schoolfellows, and was highly esteemed by his teachers for his conscientious discharge of every duty.

Shortly after he had entered the University his desire to engage in original work began to show itself in his devotion to the study of botany and zoology. It was his original intention to graduate in medicine, but the attractions of biological science were too great to permit of his devoting any large amount of attention to the other subjects of medical study, and at length he gave up all idea of qualifying as a medical man or entering into medical practice. At this early period his desire to be free from the trammels of customary methods of study, and to trust to his own efforts, was well shown by the reply which Professor Balfour, then Professor of Botany, made to him on his soliciting a certificate of attendance on his class—"I will willingly testify to your knowledge of botany, but I cannot certify that you attended my class." This early formed spirit of self-reliance and determination to investigate natural objects themselves, rather than trust merely to the results of the observations of others, seems to have pervaded his life, and to have led to the urgent requests he continually made for aid to establish collections in public institutions, and at length to the appeal to the Government itself for the means of carrying out what became the most important work of his life, that of determining the physical and vital conditions which prevail at different depths of the ocean.

I first met Wyville Thomson at the house of my late dear friend, Professor J. H. Bennett, at the time of the meeting of the British Association in Edinburgh in 1850, when the friendship commenced which remained unbroken for a single hour during the whole of his life. I was greatly impressed with his knowledge of botany, and with the energy and determination in the pursuit of science of one who appeared to me to have the most tempting professional career open before him.

Dr Dickie had been removed from the University and King's College, Aberdeen, to the chair of Natural History in Queen's College, Belfast. Finding that Thomson had no disinclination to devote himself, for some time at least, to scientific work, I had the pleasure of recommending him for the vacant lectureship, and seeing him start on the career which ended so brilliantly but prematurely. In 1851 he was prevailed on to leave King's and to

lecture in Marischal College, Aberdeen, where he continued to study his favourite subjects and to teach botany until 1853, when he was selected by the Crown as the successor to the Rev. Wm. Hincks, F.L.S., in the chair of Natural History in Queen's College, Cork.

The charming flora and fauna of Aberdeen, which had been laboriously worked for years by Dr Dickie, who had indicated the spots where the most interesting specimens were to be met with, was open and ready for further examination by the more daring and speculative spirit of Thomson. It was not long before a visit to his study possessed the greatest attractions and charms. Around the room, on shelves, tables, and floor alike, there lay, in what would have seemed to a casual observer the most grotesque confusion, the treasures of description and illustration of the most eminent naturalists of the day and of former times. The shelves and mantelpiece were, shortly, crowded with selected and neatly preserved specimens of Polyzoa and sertularian zoophytes taken in the neighbourhood, picked off the fishermen's lines, or dredged up by Thomson himself; here and there lay heaps of plants already in their places in the herbarium, or in process of preparation for being preserved; and, what was more charming than all, there were bowls and dishes and aquaria of all kinds, containing the actual living specimens which were being examined, and of which the characters were rendered permanent by the naturalist-artist himself in the most beautifully executed drawings. Amidst all these signs of true scientific work there were indications of the enjoyment which the tenant of this sanctum himself derived from indulging in his natural tastes. The specimens of the most elegant forms were always in the foreground; there never failed to be seen two or three rare or beautiful flowers, made ten times more beautiful than ordinary by the tasteful way in which they were displayed, whilst the newest photographs or sketches of the glens or other scenery in the neighbourhood found a home on any unoccupied spots there chanced to be on the walls. Those who had the privilege of witnessing the progress of this happy life, of noticing how the varying forms of these elegant sertularians gradually proclaimed their mode of development, how their myriad medusoids were produced, at length set free, and then settled in life, under the observation of the loving intruder into their inner life, could not but wonder how time was obtained for all

this varied work, attended as it was with the enjoyment of the pleasures of social life, and with that of adding to those pleasures the charms of wit, of elegance, and manner of one who was equally at home, and even more happy, in the society of ladies and educated men than in the company of his home family of zoophytes, or other of the lower forms of living beings.

Thus two or three of the earlier years of the public life of Wyville Thomson were spent. They produced many papers of great interest, which were published in the *Annals of Natural History* and other periodicals, and gave birth to very noticeable philosophical speculations on the development of certain medusoid forms, startling many older naturalists, and only partially accepted by others, such as Johnston of Berwick-upon-Tweed, and Edward Forbes, who considered them too daring advances on what was then known of the modes of life of those beings.

On leaving Aberdeen, Thomson had secured a large number of sincere friends to whom his departure was a great loss. His kindness of heart and his many estimable social qualities had made him desired in every circle, whilst in both the colleges to which he had been attached he was looked upon as a rising naturalist, destined to attain great eminence as years advanced. The degree of LL.D. was conferred upon him by Marischal College, Aberdeen.

In Cork, to the duties of teaching botany, those of teaching zoology were added, and both were discharged with equal vigour and success. But other changes were awaiting him. Early in 1854 he married Jane, the elder daughter of Adam Dawson, Esq., deputy lieutenant of the county of Linlithgow, and proprietor of Bonnytown, the neighbouring estate to Bonsyde. As the friend of the bridegroom, I had the pleasure of participating in the great gathering of members of the county families, and of Thomson's numerous Edinburgh friends, to celebrate what was deemed a most auspicious union of two families, both held in high estimation by all who knew them. In the same year the chair of Mineralogy and Geology in Queen's College, Belfast, became vacant by the resignation of Professor Frederick M'Coy, who was elected to a professorship in the new University of Melbourne, and Thomson was then transferred from Cork to Belfast.

The studies incident on the occupation of the chair of

Mineralogy and Geology and the charge of the Natural History Museum, following in succession on the study of Botany and Zoology, the subjects of Thomson's former chairs, now completed his training as a Professor of Natural History. He devoted much time and attention to Palæontology, and thus, in comparing the old world forms with those at present existing, obtained much useful insight into the relations between them. Under his guidance the Museum of the College was greatly enlarged, especially in the departments of Zoology and Palæontology, and his efforts in this respect received the hearty co-operation of the president and vice-president. Specimens for teaching and for the enrichment of the museum were sought for everywhere, and properly arranged and classified. Whatever new objects possessed unusual interest were made the subjects of papers read before scientific societies, or published in the journals of the day. It was at this time that a paper appeared on a genus of Trilobites; this had been read before the London Geological Society. Another, on a fossil Cirriped, was published in the *Annals of Natural History*. One can well imagine the growing consciousness of power in dealing with fossil forms which Thomson's previous knowledge of the existing living forms gave him, and that, as the accumulation of specimens proceeded, the series would be seen to be in certain parts more or less complete, whilst in others it would be found wanting, and thus the necessity of further investigation would be pointed out. It was natural that, when he came to the collection and investigation of the numerous varieties of extinct forms of echinoderms, the eye which was always open to the charms of beauty should have been arrested, and that it should have occurred to him that what was needed for a complete understanding of them was a correct knowledge of everything which their living forms could teach. From this time he returned to his study of the development of the larval forms of these low organisms, especially with reference to Comatula and Pentacrinus, no doubt with the hope of arriving at some general conclusions as to the relations of their peculiar mode of development with that of the higher animals, and of showing their connection with extinct forms.

Mr J. V. Thomson had found his *Pentacrinus Europæus* in the Bay of Cork in 1823, and was thus the first to discover a recent

encrinite in the seas of Europe, as he was the first who ever had the opportunity of examining one in its living state. As the result he declared it to be the young of Comatula, and, comparing his youngest Comatula with the oldest Pentacrinus he could find, he demonstrated this relation to the satisfaction of Professor Edward Forbes, Dr Ball of Dublin, and the late William Thompson of Belfast. Yet much remained to be done to clear up the whole history of this single form, and this occupied Wyville Thomson for several years. A sketch of his "On the Embryogeny of *Comatula rosacea*" appeared in the *Proceedings of the Royal Society* for 1858, a paper "On the Embryology of *Asteracanthion violaceus*" was published in the *Microscopical Society's Journal* for 1861–62, and his paper "On the Pentacrinoid Stages of Comatula" was sent to the Royal Society in December 1862, read in February 1863, and published in the *Philosophical Transactions* for 1865. This paper is a model of care and accuracy, illustrated by many beautiful and highly artistic drawings of the various stages, executed by the author, and itself attests his powers of research and his accuracy of discrimination and delineation. Whilst engaged in this work, Thomson accumulated a large amount of material, with a view to give an account of the whole genus Pentacrinus at some future time. Indeed, as far as these researches on development are concerned, it is almost to be regretted that they so soon led to the great work of deep sea research, which, when once entered upon, took up so large an amount of time.

In a correspondence with Michael Sars, the celebrated Professor of Zoology in the University of Christiania, Wyville Thomson learned that the professor's son, M. Oscar Sars, whilst engaged, as one of the acting Commissioners of Fisheries, in a series of investigations as to the fisheries off the Loffoten Islands, north-west of the coast of Norway, had dredged up from about 300 fathoms a number of living animal forms. In response to an invitation from Professor Sars, Thomson visited Norway to examine these objects, and he states that amongst them there was a small crinoid of surpassing interest, which they at once recognised as a degraded type of the Apiocrinidæ, an order which had up to that time been regarded as entirely extinct. Some years previously M. Absjörnsen, dredging in 200 fathoms in the Hardangerfjord, procured several

examples of a starfish "Brisinga," which seems to find its nearest ally in the fossil genus Protaster. In this way it had become certain that animal life does not cease in the ocean at a depth of a few hundred fathoms, as the late Edward Forbes had supposed. Wyville Thomson tells us that, long previously to 1868, he "had a profound conviction that the land of promise for the naturalist, the only remaining region where there were endless novelties of extraordinary interest ready to the hand which had the means of gathering them, was the bottom of the deep sea." And when, in his visit to Norway, he became fully acquainted with the advantages which Professor Sars and his son had enjoyed through the means of their Government, he resolved to lose no opportunity of pointing out how greatly the Government of the most powerful maritime nation in the world might aid science by placing at the disposal of naturalists one of their numerous unemployed vessels to assist in the exploration of the ocean depths. A favourable opportunity presented itself when engaged with Dr W. B. Carpenter in working out the structure and development of the Crinoids in the spring of 1868. Dr Carpenter was a vice-president of the Royal Society, through which body alone it seemed that the Government could be influenced. He considered the subject carefully, and having arrived at a conclusion favourable to the project, it was resolved that he should bring the subject under the notice of the Society, introducing it by a letter which his colleague was to write to him after his return to London. The Royal Society and the Government entered heartily into the plan, and the dredging cruises of H.M.S. "Lightning" and "Porcupine" and the expedition of the "Challenger" were the result.

It has been already stated that Wyville Thomson was appointed to the chair of Mineralogy and Geology in Queen's College, Belfast, in 1854. On the removal of Dr Dickie to Aberdeen in 1860, the duties of the chair of Botany and Zoology were also entrusted to him, and he became from that time Professor of Natural History in these four branches. For the discharge of these duties he was very peculiarly fitted from the happy way in which he had at different times been called upon to teach the four subjects in succession. He had now the entire responsibility of the department of Natural History. In the year 1860 he was admitted to the degree of LL.D.

in the Queen's University *ad eundem*. As a resident in Belfast he entered heartily into every plan for the spread of knowledge or the improvement of his townsmen. He was an active member of the Natural History and Philosophical Society, and at its meetings he contributed many valuable papers. As a lecturer he was fluent in style, easy in manner, and lucid in thought and expression. He conveyed to his hearers much of the interest he had in his subject, and encouraged them to engage in original work. He was instrumental in placing the School of Science and Art in its present relation to South Kensington, and took a constant and lively interest in its success.

He was an active member of the local committee in connection with the meeting of the Social Science Congress in Belfast, under the presidency of the Earl of Dufferin, in 1867.

Interested in education, and attached to the system on which the Queen's Colleges and Queen's University were founded, he strenuously opposed all attempts to interfere with their academic character or their privileges. When a supplemental charter was issued, which it was believed would lessen the necessity for thorough academic training to obtain degrees in the Queen's University, Wyville Thomson came forward very prominently, and succeeded in collecting large funds, and obtaining still further guarantees from an influential committee, which enabled the validity of the charter to be tested in the Court of Queen's Bench. The result was that, after long and protracted arguments, an injunction was granted in 1867 by the Master of the Rolls which rendered the supplemental charter inoperative, and helped to prevent for many years the substitution of a system of mere examinations for the most complete academic training which prevailed in any university.

Regardless of the differences of religious and political opinion which prevailed in Belfast, he was courteous to all, tolerant of every opinion frankly formed, and never obtrusive of his own. He was esteemed by all classes and parties, he had friends everywhere, and his house was always open to all of distinction in science or art who might happen to visit Belfast. Engrossed in his own proper studies, he never obtruded them upon others, but whenever assistance or advice in connection with them were needed, he spared no pains to make both effective. His tact,

consideration, and good taste never failed him. His passion for flowers seemed a part of his nature; he cultivated them with the greatest care, and, though delighting to display them to the greatest advantage in his own house, he enjoyed equally his regular practice of carrying any particularly choice specimen he might have grown to present it to some of his many friends.

In 1867 he was made Vice-President of the Jury on Raw Products at the Paris Exhibition.

On the 30th May 1868 he addressed the letter to Dr Carpenter which was previously agreed upon, pointing out that Edward Forbes's conclusion, that a zero of animal life was reached at a depth of a few hundred fathoms, was incorrect, as had been proved by M. Absjörnsen's dredging starfishes at 200 fathoms, and by M. Oscar Sars dredging living crinoids from 300 fathoms; that the effect of pressure has probably been greatly exaggerated, because an equal pressure within and without by water would probably produce no injurious effect on animal life, and might even contribute to increase the æration of the water; and that, looking at the condition of the cave fauna, it is probable that the diminution of light at great depths may only affect the development of colour and of the organs of sight. He suggested that, whilst dredging at 1000 fathoms was quite beyond the reach of private enterprise, it was quite practicable if the Admiralty could be induced to grant the use of a vessel for the purpose. He proposed to start from Aberdeen, to go first to the Rockall fishing banks, and thence north-westward towards the coast of Greenland, rather to the north of Cape Farewell.

Dr Carpenter wrote to General Sabine enclosing Professor Wyville Thomson's letter, pointing out the admirable results obtained by M. Sars, with similar aid granted by the Swedish Government; and showing that he and Dr Thomson had restricted their request within such conditions as could, without great expense or inconvenience, be acceded to by the Admiralty. On the evening of the day on which Dr Carpenter's letter was written, General Sabine brought the subject under the consideration of the council of the Royal Society, who at once approved of the proposal, recommended it to the favourable consideration of the authorities of the Admiralty, and advanced a sum of £100 to meet expenses. The

Lords Commissioners of the Admiralty wrote on the 14th July that they had given orders for Her Majesty's steam vessel "Lightning" to be prepared immediately at Pembroke to meet the wishes of the Royal Society. The "Lightning" left Pembroke on the 4th August 1868. Drs Carpenter and Thomson, and Dr Carpenter's son Herbert, joined the vessel at Oban, whence they sailed on the 8th August. They reached Stornoway on the 9th, and left it for the north on the 11th. On the same afternoon they dredged in 60 to 100 fathoms; on the 13th in 450 fathoms, finding no bottom, but the high temperature of 9·5° C.; afterwards in 600 to 700 fathoms in the same locality. Bad weather frequently impeded the dredging operations. On the return of the vessel to Stornoway on the 9th September, Dr Wyville Thomson was obliged to leave her to attend to duties in Dublin, but Dr Carpenter remained with the vessel, left Stornoway again on the 14th September and dredged in 650 fathoms, but on the 21st the weather was so bad that the work had to be concluded. There were only ten days available for dredging in the whole six weeks, and on only four of these was the vessel in water over 500 fathoms deep. Yet a fair measure of success had been achieved.

It was shown that varied and abundant animal life, represented by all the invertebrate groups, occurs at depths in the ocean down to 650 fathoms at least; and that, instead of deep sea water having an invariable temperature of 4° C., great masses of water, at temperatures varying from 2° C. to 6·5° C., maintain a remarkable system of oceanic circulation, and yet keep so distinct from each other that both may be found within the limit of an hour's sail. It was also ascertained that a large proportion of the forms living at great depths of the sea are of unknown species, and identical with tertiary fossils previously believed to be extinct.

The next year, 1869, saw Wyville Thomson again engaged in the examination of the physical, chemical, and biological conditions of the ocean depths, for the Lords Commissioners of the Admiralty had acceded to the additional request of the council of the Royal Society and had set apart the "Porcupine," a small vessel fitted up for surveying purposes and admirably adapted for the continuance of these researches, from the beginning of May to the middle of September. As it was impossible for those connected with the

previous expeditions to be absent from their public duties for any large portion of this time, it was resolved that there should be three separate cruises, one on the west coast of Ireland, the Porcupine Bank, and the channel between Rockall and the coast of Scotland, under the scientific charge of Mr Gwyn Jeffries, F.R.S.; a second to the north of Rockall, leading northwards to the point where the expedition of 1868 left off, under the charge of Dr Thomson; and the third to work over the "Lightning Channel" and check the former observations, under the direction of Dr Carpenter.

Mr Gwyn Jeffries was favoured with remarkably fine weather, and found it possible to dredge during seven days at depths greater than 1200 fathoms, and on four days at less depths. His deepest dredging was 1476 fathoms, and the whole of them yielded an abundance of novel and interesting results in every invertebrate sub-kingdom.

Captain Calver was accustomed to minute accuracy in surveying, and thoroughly versed in the use of instruments and in the bearings of scientific investigation. His crew were chiefly known and tried men, Shetlanders who had spent many successive summers in the "Porcupine" under his command. Aided by a staff of zealous officers, Captain Calver soon obtained so entire a mastery over the operation of dredging that he made it almost a certainty at depths at which this kind of exploration would have been previously deemed out of the question. Wyville Thomson at once recognised these favourable conditions, and having found that the experiences of the previous year, and all their anticipations for the present, had been realised, at least for the depth of nearly 1500 fathoms, and that even at that depth nearly all the types of living marine invertebrata were represented, though the number of species seemed reduced and the size of the animals dwarfed, he suggested that it would be desirable that the second cruise should be made in deeper water than had originally been intended, and pointed out the position of the deepest water easily accessible, 250 miles west of Ushant, as a fitting place for the next observations.

The Hydrographer cordially acquiesced in this proposed change of plan, and it was arranged that the next dredging should be done at this spot, in water 2500 fathoms deep. Professor Wyville

Thomson left Belfast in the "Porcupine" to take the scientific direction of this cruise on the 17th July 1869, taking with him Mr Hunter, F.C.S., chemical assistant in Queen's College, Belfast, to examine and analyse the samples of sea water. At Queenstown Mr P. Herbert Carpenter joined the ship to practise the gas analysis which he was to undertake on the third cruise.

The vessel proceeded on her voyage at 7 P.M. on the 19th July, steaming in a south-westerly direction across the mouth of the channel. At 4.30 A.M. on the 21st they were still only on the plateau of the channel in 95 fathoms of water, but from midday to the afternoon they passed over the edge of the plateau and dredged in 725 fathoms, the bathymetrical horizon of vitreous sponges in the northern seas, bringing up several specimens of these beautiful forms, and a slight admixture of globigerina ooze in sand. On the 22nd they were in water of about the greatest depth they had reason to expect, 2435 fathoms, at a temperature of 2·5° C. A successful dredging yielded 1½ cwt. of grey chalk mud, containing examples of each of the invertebrate sub-kingdoms, which, though dead, had evidently been alive when they entered the dredge. Similar results attended a dredging on the 23rd at the same depth, after which the party returned to the coast of Ireland, dredging and noting the results at intervals on the way. The vessel reached Cork on the 2nd August, and Belfast on the 4th.

She left again on the third cruise for the year, on the 11th August, under the direction of Dr Carpenter, Mr P. H. Carpenter undertaking the analyses, and Wyville Thomson accompanying them. He busied himself in drawing, naming, and describing new species, and in noting the great general features of the prevailing physical and vital conditions. It is scarcely possible for anyone, however little imaginative, to read the graphic accounts of the incidents of these voyages without having his enthusiasm aroused, and almost wishing to have been present on many of the occasions so forcibly depicted. It seems more like a dream than a reality that at a single haul the dredge should have brought up in its bag and on its tangles not less than 20,000 specimens of the pretty little urchin, *Echinus norvegicus*, and we have Dr Thomson's authority for such an event having happened. On other occasions, one is irresistibly brought to watch, with bated

breath, the landing of the great prizes which the dredge had collected. His account of the glimpses from time to time as the dredge was coming in, of what seemed to be a large scarlet urchin, the disappearance of it now and then as if lost altogether, then its quiet settling down as a round red cake, and beginning to pant,—that he had to summon up some resolution before taking the weird little monster in his hand,—show the graphic power of the author no less than the enthusiasm of the naturalist.

I cannot forbear giving another illustration :—" I do not believe human dredger ever got such a haul. The special inhabitants of that particular region—vitreous sponges and echinoderms—had taken quite kindly to the tangles, warping themselves into them, and sticking through them and over them, till the mass was such that we could scarcely get it on board. Dozens of great Holteniæ, like

> 'Wrinkled head and aged,
> With silver beard and hair ;'

a dozen of the best of them breaking off just at that critical point where everything doubles its weight by being lifted out of the water, and sinking slowly away back again to our inexpressible anguish ; glossy whisps of Hyalonema spicules ; a bushel of the pretty little mushroom-like Tisiphonia ; a fiery constellation of the scarlet *Astropecten tenuispinus ;* while a whole tangle was ensanguined by the 'disjecta membra' of a splendid Brisinga."

The effect of the brilliant phosphorescence of the contents of the dredge are vividly pourtrayed ; and the argument in favour of the urchins, which are only one-fourth of the size of others whose characters are indistinguishable from theirs, being dwarfed specimens of the same genus, is not easily forgotten :—" The Shetland variety of *Equus caballus* is certainly not more than one-fourth the size of an ordinary London dray-horse, and I do not know that there is any good reason why there should not be a pony form of an urchin as well as of a horse."

Wyville Thomson had arranged with his colleagues to take part in an exploration of the deep sea to the south of Europe and the Mediterranean in 1870, but he was prevented from doing so by an attack of fever. Yet he gave at second hand a brief account of the

first part of the work under the direction of Mr Gwyn Jeffries to complete his sketch of the condition and fauna of the North Atlantic; and directed attention to the entirely exceptional conditions of temperature and animal life observed by Dr Carpenter in the Mediterranean as compared with the outer ocean.

In the whole life of Thomson, notwithstanding his vivid appreciation and accurate descriptions of the most minute details of structure necessary for the determination of new species, and for allotting them their proper position in nature, he never allowed himself to be dragged down to the level of a mere collector, accumulating myriads of individual objects and cataloguing them. He invariably rose superior to details, and, subordinating them as merely means for arriving at just conclusions regarding the physical and vital characters of the earth and its living freight in long past ages or the present time, he devoted his best thoughts to the consideration of the means by which great results might be achieved. The idea that either individual or even imperial aid was necessary neither occasioned him anxiety nor discouraged him; he resolutely set forth the conditions, showed how important results could be arrived at, and the means never failed him.

His discussion of the effects of the Gulf Stream on the climate of the coasts of Northern Europe, in comparison with the influence of any possible general ocean circulation, is a good illustration of his wide and powerful grasp of natural phenomena bearing on any particular point. He had measured in the North Atlantic the extent of the warm and cold areas of water, and recognised the fauna which are proper to each; he had determined the existence of the vast layer of cold water, 1500 fathoms thick, at the bottom of the Bay of Biscay, and that the temperature there at 1230 fathoms from the surface is the same as that of the bottom off Rockall; he saw that, whilst the communication of the North Atlantic and the Arctic Sea is restricted, the communication with the Antarctic basin is, as he describes it "open as the day,"—a continuous and wide valley, upwards of 2000 fathoms in depth, stretching northwards along the western coasts of Africa and Europe; and then pointed out how much less startling than it appears at first sight is the suggestion that the cold water filling deep ocean valleys in the northern hemisphere may be partly derived from the southern. He calls to mind

that the floor of the Atlantic is covered by a creamy, flocculent layer of microscopic animals; whilst, wherever there is any known current, this deposit is absent and replaced by gravel, and thus shows that the movement of any cold indraught of water at the bottom must be excessively slow. He dispels the chimerical idea that there is a kind of equatorial diaphragm between the northern and southern ocean basins, and explains that it is only on the surface of the sea that a line is drawn between the two hemispheres by the equatorial current. He then gives as evidence of the slow indraught of cold water from the Southern Sea, that it is colder than the mean winter temperature of the area which it occupies and that of the crust of the earth, and that its temperature rises as it is traced northward; whilst, owing to Behring's Straits being only 40 fathoms deep, there is no adequate northern source of such a body of cold water.

In 1869 Wyville Thomson was elected a Fellow of the Royal Society; and in the year following, on the resignation of Dr Allman, he was appointed Professor of Natural History in the University of Edinburgh. His friends in Belfast recognised the distinction which had thus been conferred upon him, but felt the loss which the college and the town had sustained by his removal, and, on taking leave of him, presented him with a handsome service of plate and an illuminated address at a public meeting presided over by the mayor. The honorary degree of D.Sc. was conferred upon him by the Queen's University about the same time.

His duties now became more arduous than ever. His class-room was crowded with students, whom he taught not merely by lectures but by practical demonstrations. In 1871, the meeting of the British Association in Edinburgh, the arrangement and plans of the new University buildings, troubles in connection with the admission of females to the college classes, and the transfer of the Museum of which he was Regius Keeper, to the Museum of Science and Art, added greatly to his necessary labours.

At this time the rapid extension of ocean telegraphy gave practical value to everything which concerned the depth of the ocean, the character of its bottom, and the presence there of animals which might injure the coverings of telegraphic cables, whilst great interest was being manifested by the public in the remarkably novel experiences of the cruises of the "Lightning" and the "Porcupine."

From America and from Europe more or less effective expeditions had been sent out, but it was evident that it rested very specially with England to lay down the first broad outlines of the physical and biological conditions of the bottom of the ocean.

The circumstances were very propitious; and when Dr Carpenter addressed a letter to the First Lord of the Admiralty, urging the despatch of a circumnavigating expedition for this purpose, their lordships, after a favourable report of the Hydrographer to the Navy, agreed to despatch such an expedition, if the Royal Society recommended it and furnished them with a feasible scheme.

Mr Lowe, then Chancellor of the Exchequer, with great interest and sagacity, saw that such an enterprise was entirely beyond the reach of private means, and agreed to furnish the necessary funds. The "Challenger" was chosen for the purpose, with Captain Nares, a surveying officer of great experience and skill, to command her, and Professor Wyville Thomson as director of the Civilian Scientific Staff. He tells us that "when the suggestion was made to him at the commencement of the negotiations to join the expedition, the sacrifice appeared in every way too great; but as the various arrangements progressed, so many friendly plans were proposed on all hands to smooth away every difficulty, that he finally accepted a post which, to a younger naturalist, without the ties of a family and a responsible home, would be perhaps among the most delightful the world could offer."

The President and Council of the Royal Society nominated the members of the Civilian Scientific Staff, and a Circumnavigating Committee, amongst whom were Dr Carpenter and Dr Wyville Thomson, suggested a scheme whereby it was believed the best results might be obtained. Sixteen of the eighteen large guns which the "Challenger" carried were removed; she was fitted with a natural history workroom, a chemical laboratory, and furnished with every scientific appliance to the satisfaction of the director, and in a way entirely unprecedented for scientific purposes. With a ship thus equipped, and the responsibility of directing the most delicate and difficult scientific observations at sea for a period of three or four years, Dr Wyville Thomson left Portsmouth on the 21st December 1872 with the good wishes and ardently expressed hopes of every lover of science in Great Britain.

The first part of the voyage, that to the Canary Islands, was made merely tentative, with a view of getting everything on board into perfect order for correct observations, and dividing the labour of research in the most convenient way amongst the members of the staff.

On the 14th February 1873 the "Challenger" sailed from Santa Cruz to cross the Atlantic, and the real work of the expedition commenced. She reached Sombrero on the 15th March, the Bermudas on the 4th April, and Halifax on the 9th of May. Leaving Halifax again on the 15th, she went southwards and back to the Bermudas, to make another section of the Gulf Stream. On both occasions the most detailed and interesting observations were made. Subsequently she crossed the Atlantic three times, visited Australia, New Zealand, the Malay Archipelago, Hong Kong, and Valparaiso, sailing altogether 68,930 miles, and returning to Sheerness on the 24th May 1876, after an absence of three years and a half.

Shortly after his return, Dr C. Wyville Thomson received the honour of knighthood, and was appointed by the Lords Commissioners of Her Majesty's Treasury "Director of the 'Challenger' Expedition Commission." In the same year he was awarded a Royal Medal by the Royal Society for his successful direction of the scientific investigations carried on by H.M.S. "Challenger."

In July he and the other members of the scientific staff of the "Challenger" were entertained at a banquet in Edinburgh. On going with Emeritus Professor Balfour to Upsala, as the representative of the Senatus of the University of Edinburgh on the occasion of the tercentenary of that ancient University, the King of Sweden created him a Knight of the Order of the Polar Star. He was a Fellow of the Royal Societies of London and Edinburgh, a Fellow of the Royal Irish Academy, Ph.D. Jena, Fellow of the Linnean, Geological, Zoological, and Palæontological Societies of London, and of various foreign and colonial institutes. In 1877 he was appointed to deliver the Rede Lecture at Cambridge, and in 1878 he presided over the Geographical Section of the British Association at its meeting in Dublin, and was made LL.D. of the University of Dublin.

Sir Charles discharged the duties of his chair with his customary vigour on his return from the voyage of the "Challenger," and

worked laboriously at the vast amount of material and observations which had been accumulated. In 1877 he published two volumes of a preliminary account of the results of the voyage, a work of surpassing interest, not alone from the scientific value of the observations recorded in it, and the conclusions which the author draws from them, but for the beautifully executed illustrations it contains, and the graphic sketches which occur here and there of the general as well as the scientific features of the places visited and examined. In this work Sir Charles has recognised the valuable assistance of his colleagues in the scientific staff; the aid which all the naval officers, without exception, gave in the most friendly spirit to the civilian staff; the wonderful temper with which the commander and first lieutenant tolerated all the irregularities inseparable from dredging and other scientific work; the friendly readiness with which the chief of the naval scientific staff placed his valuable observations at the disposal of the civilian staff; the patience and care displayed by the lieutenants who superintended the dredging and trawling and the estimations of temperature; and his debt of gratitude to the sailors for the respect and consideration with which they treated all the civilians on board.

These were, no doubt, remarkable results—this combination of everyone on board to achieve success, this subordination of the discipline, cleanliness, and order of a man-of-war to the prosecution of the study of Natural Science in various departments; and it cannot be doubted that they were mainly due to the genial disposition, the many engaging social qualities, the gentlemanly bearing, and the untiring energy of Sir Charles Wyville Thomson.

He admitted that the strain, both mental and physical, was long and severe, and that it had told upon all of them. His friends observed that, with the continuance of the labours necessary for bringing out the full account of the whole results of the "Challenger" Expedition, his vigour by no means kept pace, but until 1879 there was no real cause for anxiety. In June of that year, however, he had a serious illness, from which he only partially recovered. His place in the University of Edinburgh had to be supplied, and at length arrangements were made for securing to him a well-deserved retiring allowance. From time to time he persevered in endeavouring to forward the publication of the com-

plete reports of the Expedition, and still attended meetings of the Commissioners of Supply for his native county. He even sat as magistrate sixteen days before his fatal illness. But from his first seizure he was unable to discharge the duties of his chair, and retired from them altogether in the October of 1881. Subsequently he had also to relinquish his position as "Director of the 'Challenger' Expedition Commission." In the beginning of March 1882 his critical condition was manifested by his making a personal application to be relieved from attending at the Fiars' Court on the 10th of that month, the very day on which his last seizure proved fatal. He died at the early age of fifty-two, having made many lasting contributions to science, secured large numbers of sincere admirers and friends, and received the applause and approval of scientific men everywhere for the wisdom, energy, skill, and courtesy which he had shown in the direction of the most extended and successful of scientific expeditions.

Lady Wyville Thomson survives her husband. He left an only child—Mr Frank Thomson, M.A. Ed., a student of medicine.

The Commissioners of Supply of the county of Linlithgow, with a committee of scientific and other friends in Edinburgh, have collected several hundred pounds for the purpose of erecting a lasting memorial to commemorate the distinguished services of the late Sir Charles Wyville Thomson, and it has been resolved to place a bust by Hutchison in the University of Edinburgh, and a memorial window in the beautiful collegiate church in his native place.

The following is a list of Sir C. Wyville Thomson's principal publications :—

On the Application of Photography to the Compound Microscope. *Brit. Assoc. Rep.*, part 2, 1850.

Notes on some Scotch Zoophytes and Polyzoa. *Annals Nat. Hist.*, ix., 1852.

On the Character of the Sertularian Zoophytes. *Brit. Assoc. Rep.*, part 2, 1852.

Notes on some British Zoophytes. *Annals Nat. Hist.*, xi., 1853.

On Native Irish Zoophytes and their Allies. *Nat. Hist. Rev.*, ii., 1855.

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Note.—Sir Charles Wyville Thomson had also undertaken to write the "Report on the Crinoidea" of the voyage of the "Challenger" in conjunction with Dr P. H. Carpenter.

Mr Thomas William Rumble. By William Connor Steel Rumble.

Mr Thomas William Rumble was born in London, 26th December 1832. He received part of his education at the Reading Grammar School, under the celebrated Dr Valpy. At an early age he was transferred to the office of his father, an architect in good practice, where he was taught the rudiments of his future profession. Tiring of the dull routine of the drawing-office, he left home to try his fortune across the Atlantic, where, after many adventures, he was appointed in November 1850 assistant engineer on the Central Railroad of New Jersey, under J. Laurie, Esq., C.E., he being then not quite 18 years of age. He remained in America till June 1852, during which time he was actively engaged in laying out the Erie and Forest Lawn Cemeteries, superintending the building of the Berks County Baths, the Buffalo Public Wash-houses, &c., and occasionally giving lectures on architectural and engineering subjects. Dr Calvin Fairbanks, in a letter dated 1st October 1851, speaks thus of his ability as a lecturer:—"I must say I was gratified with the clearness with which you presented the necessity of developing the yet undeveloped facts in architecture, in your last evening's lecture. It would have been happy had there been a more general interest at an earlier period. I hope, Sir, it may be convenient for you to favour us again with a repetition of the same, followed by illustrations and remarks."

Almost immediately on his return to England, Mr Rumble obtained work in Kensington, superintending the building of All Saints' Church and the laying out of the Kensington Park Estate.

In October 1853 he went out to Bombay, as assistant engineer on the Bombay, Baroda, and Central India Railway, then in course of construction. An attack of fever obliged him to return on sick

—Continued in a later volume.

Thos. Wm Rumble

leave to England, where he arrived in February 1854. He next obtained the post of engineering superintendent of the Arthington Extension Waterworks under Mr Hawkesley, with whom he remained till the completion of the work, when he received a flattering letter from Mr Alderman Hepper, chairman of the Leeds Water Works Committee, expressing the great satisfaction of that body with the manner in which he had conducted the works. Returning to London, Mr Rumble experienced some difficulty in finding employment to his taste, and was, for short periods, draughtsman in the offices of Messrs Conybeare and Brikinshaw, the London and South-Western Railway Company, the Admiralty, &c., till in 1857 he was appointed engineer to the Atlas Steel Works, then entirely in the hands of Mr (now Sir) John Brown, in which capacity he was entrusted with the conduct of many transactions requiring much tact and diplomacy. In 1858 Mr Rumble was elected Fellow of the Society of Engineers; in 1860 a member of the Institute of Mechanical Engineers; in 1861 a member of the Institute of Naval Architects; and in 1866 a Fellow of the Royal Microscopical Society, in the proceedings of which body he was always deeply interested.

During these years Mr Rumble had opened an office in Westminster, and was practising as a civil and mechanical engineer, and was fortunate enough to secure much good work. In 1869 he paid a second visit to the United States, and spent six months visiting many engineering shops and acquiring a thorough knowledge of the recent mechanical improvements. Shortly after his return to England, Mr Rumble had the honour of two interviews with his late Majesty the Emperor Napoleon, who was pleased to express his satisfaction with the plans, drawings, &c., submitted for his approval. On New Year's Day, 1872, Mr Rumble was again in New York, and visited the various Safe Deposit Companies in that and other cities, with the view of obtaining information for the National Safe Deposit Company, then about to be formed in London. He visited Philadelphia, Boston, Halifax, &c., and the ruins of Chicago, then scarcely cold after the great fire, and examined the vaults and safes remaining intact. He returned to London on the 28th January, and was for the rest of the year employed in designing the safes, strong-rooms, buildings, and other arrangements of the National Safe Deposit Company, which were afterwards carried out under his superintend-

ence at the corner of Queen Victoria Street. The extravagance of his partner at this time considerably involved Mr Rumble, and in 1875 he dissolved the partnership. In 1876 Mr Rumble obtained the position of chief engineer of the Southwark and Vauxhall Water Company. His unceasing energy and untiring industry gradually brought this company, from the state of confusion in which he found it, to such a state of order that the dividends rose from $2\frac{1}{2}$ to $7\frac{1}{2}$ per cent. In 1877 he was admitted member of the Institution of Civil Engineers. In 1878 he successfully laid a 30-inch main under the Thames at Richmond without the aid of dams—the only feat of the kind accomplished at that date in England. Under Mr Rumble's direction a trench was dredged across the bed of the river a few feet below Richmond Bridge. The lengths of the pipe, made on the ball and socket principle, were joined on the banks of the river, and in the early morning of July 3rd were shipped on board three barges strongly lashed together, carefully brought into position, and safely lowered into the trench. When the necessary connections at either end were made, the main was charged, and has ever since been in full work. During the year 1879 the Directors of the Southwark and Vauxhall Water Company determined to supplement the supply of water derived from the Thames by sinking a well into the chalk. In conjunction with Professors Prestwich and Ansted, Mr Rumble selected a spot in the Manor Park, Streatham; a trial bore-hole was made, and in 1881 the sinking of the well begun. The work is still in progress, but so far fully justifies the hopes formed for its success. Struck by Mr Rumble's manner of handling the matter, and entirely without his knowledge, Professor Prestwich and Professor Ansted proposed him as Fellow of the Geological Society, into which he was admitted in December 1879. In February 1881 he was elected Fellow of the Royal Society of Edinburgh.

Towards the end of 1881 the excessive overwork and heavy responsibilities of his position began to tell on his health, which steadily though very gradually failed, and he developed symptoms of pernicious anæmia which defied every effort to overcome it. In December 1882 Sir William Jenner recommended immediate and absolute rest of body and mind for six months. Leave of absence being unanimously granted by the directors of the company, various places were visited in search of health, until on the 5th

April 1883 he returned to Bonchurch, Isle of Wight, where he rapidly grew worse, and died on Saturday, 21st April, surrounded by nearly all his family. He was buried in the New Churchyard, Bonchurch, on 28th April.

Mr Rumble was twice married, and has left a large family to mourn his loss. In business Mr Rumble was straightforward and unerringly honest to his employers, often nervous about small matters, but without fear in cases of grave import, when he was always calm and self-possessed. The rapidity and clearness of his perceptive faculties amounted almost to the gift of second sight, and led him to form swift conclusions which rarely proved false. His firmness in dealing with faults in those under his charge was moderated by great kindness to his men when suffering under any affliction, illness, or distress. He was considered by them always more as a friend than master, and they showed their appreciation of his goodness by presenting him with a testimonial on the celebration of his 50th birthday, 26th December 1882. In private life Mr Rumble's genial spirits, shrewd observations, and witty remarks, endeared him to a large circle of friends. Indeed, his critical condition was almost to the last concealed by his courageous efforts to appear better than he was, and thus relieve the anxiety of his family. He possessed a most retentive memory, and had the faculty of readily assimilating those portions of the books he read which were likely to be useful to him in his professional work. His travels over the greater part of Europe and America naturally enlarged his ideas, and he drew full benefit from the varied experience thus acquired. He had deeply studied the legal as well as the technical points of his profession, and so was particularly well fitted to fill the various appointments he held during his lifetime.

Joseph Liouville. By Professor Chrystal.

Joseph Liouville was born at St Omer on the 24th March 1809. He came of a family of Lorrainers, more than one of whom were distinguished for talents beyond the common. Liouville's father held a public office under the Empire, and an elder brother, Felix Silvestre Jean Baptiste, was a distinguished Parisian advocate. Joseph gave early indications of mathematical ability, and entered

upon that stereotyped course of training which has been famous for the nurture of so many Frenchmen of genius. At the age of sixteen he entered the École Polytechnique, and on leaving it in 1827 was classed as an engineer in the Department of Roads and Bridges. After two years he forsook engineering for the cultivation of the higher mathematics. He speedily distinguished himself in his chosen career; for as early as 1829 we find a paper of his ("Démonstration d'un Théorème d'Éléctricité Dynamique") in the *Annales de Chimie et de Physique;* and in 1831 he became a repétiteur, and seven years later a professor, in the Polytechnic School. In the interval he had performed perhaps his greatest service to his favourite science by starting in 1836 *The Journal de Mathématiques Pures et Appliquées.* This journal came most opportunely to fill the gap left by the discontinuance of the *Annales de Gergonne;* but it could scarcely have attained its brilliant success had it not been for the many excellent qualities of its editor, whose critical discernment, that enabled him to enter so readily into the spirit of the works of other mathematicians, and to assist at the *debut* of so many men of distinction,—whose amiability, candour, and freedom from national prejudice,—whose own inexhaustible powers as a contributor of original memoirs, all combined to fit him uniquely for the post which he filled so admirably for nearly forty years.

In 1839 Liouville was elected a member of the Academy of Sciences in succession to Lalande, and the year following he was put upon the Board of Longitude, in whose proceedings he took a lively interest to the end of his life. In 1852 he became a professor in the College de France, and continued to lecture in that capacity until about a year before his death.

If we except his continually recurring successes as a teacher and as an investigator in the most recondite of all the sciences, and the honours accorded to him by the scientific world in token of their appreciation, Liouville's public career was uneventful, as the career of a devoted man of science usually is. On one occasion, however, he departed from the "even tenor of his way." In 1848, a year of much tribulation for France, he received a flattering mark of widely spread popular esteem by being elected a member of the "Constituent Assembly." He promptly answered this call of

public duty, and served his alloted time with efficiency if without special distinction. When, however, his mandate expired, instead of seeking re-election, he betook himself once more to the uninterrupted pursuit of the career for which his abilities best fitted him.

Liouville was as fortunate in his private life as he was successful in his public career. He lived to a good old age in the happiest domestic circumstances, until a cruel accident deprived him of his wife. His son, a councillor in the Court of Nancy, died soon afterwards, and the aged mathematician never completely recovered from the effects of this double bereavement. Although his health gave way, his intellect remained unclouded; and it was only in the beginning of 1882 that he gave up his favourite work of lecturing at the College de France. He still continued, however, to attend the meetings of the Academy, but expressed to his friends his consciousness that the end was near. He died on the 8th September 1882, as he himself said, "in his turn"; for, since the death of Chasles, he had been the patriarch among European mathematicians.

Some idea of the extent of Liouville's mathematical writings may be obtained by consulting *The Catalogue of Scientific Memoirs* published by the Royal Society of London. The entries under Liouville's name number 379, and cover some twelve pages. Many of these are merely remarks made on contributions to his journal, or notes appended to works by other mathematicians which he edited; yet, brief as they are, they frequently contain matter of much importance. As specimens of this part of his work, we may mention his "Notes on Two Letters of Mr Thomson relative to the Employment of a New System of Orthogonal Coordinates in certain Problems in the Theories of Heat and Electricity, and in the Problem of the Distribution of Electricity on the Segment of a Spherical Shell of Infinite Thinness" (*Jour. d. Math.*, xii. 1847), in which he draws attention to the analytical and geometrical importance of the method of Inversion, which had just been brought under the notice of mathematicians by the brilliant use that Thomson had made of it in his physical researches.

In another note (*Jour. d. Math.*, xv. 1850) he enunciates the important theorem that the equation

$$dx^2 + dy^2 + dz^2 = \lambda(d\alpha^2 + d\beta^2 + d\gamma^2),$$

where x, y, z, λ are functions of α, β, γ, has for its unique solution the stereographic or inversion transformation.

The following rough analysis will give some idea of the territory covered by his more elaborate memoirs :—

One of the earliest subjects that engaged his attention was Generalised Differentiation ("Différentielles à Indices Quelconques"). The subject is developed at considerable length in five memoirs printed in the 13th and 15th volumes of the *Journal de l'École Polytechnique* (1832–37).

Some of his most important work relates to the Integral Calculus, more particularly that part of it which deals with the theory of elliptic and other transcendental functions.

The earliest memoirs on the subject are two in the *Journal de l'École Polytechnique* (xiv. Cah. 1833, see also *Comptes Rendus*, 1837), "On the Determination of Integrals whose value is Algebraical." He here follows up the researches of Abel on the same subject; and arrives, *inter alia*, at the following important results :—

1. If χ be any rational function of x, then $\int dx\sqrt[m]{\chi}$, if algebraically expressible at all, can be expressed in the form $P\sqrt[m]{\chi}$, P being rational. And, farther, that the integral $\int dx\sqrt[m]{\chi}$ can always be reduced to the form $\theta/\sqrt[m]{T}$, where T is a known rational integral function, and θ a rational integral function whose coefficients have to be determined. This theorem enables us at once to find the value of the integral, if it is algebraically expressible; or else to show that it has no finite algebraical value.

2. If y be an algebraical function of x, *i.e.*, connected with x by means of an equation $F(x, y) = 0$, which is rational and integral in both x and y, then, if the integral $\int ydx$ is expressible explicitly in finite terms by means of algebraic, exponential, or logarithmic functions, it will be expressible in the form

$$\int ydx = t + A\log u + B\log v + \;.\;.\;.\;.\; + C\log w\,;$$

where AB . . . . C are constants, and t, u, v, ... w algebraic functions of x.

Among the other memoirs on the present subject may be mentioned the following :—

"On the Elliptic Transcendents of the First and Second Species,

considered as Functions of their Amplitude." *Jour. Éc. Polytech.*, xiv., 1834, and *Jour. de Math.*, v., 1840.

"On the Integration of a Class of Transcendent Functions." *Jour. de Math.*, xiii., 1835.

"On a New Use of Elliptic Functions in Celestial Mechanics." *Jour. de Math.*, i., 1836.

"On the Classification of Transcendents, and on the Impossibility of Expressing the Roots of certain Equations as a Finite Function of their Coefficients." *Jour. de Math.*, ii., 1837.

"On a very Extensive Class of Quantities whose value is neither Algebraic nor reducible to Algebraic Irrationals." *Jour. de Math.*, xvi., 1851.

Relating to the theory of differential equations, we have the following :—

"On the Equation of Riccati." *Jour. de l'Éc. Polytech.*, xiv., 1833.

"On a Question in the Calculus of Partial Differences." *Jour. de Math.*, i., 1836.

"On the Development of Functions or Parts of Functions in Series, whose various terms satisfy the same Differential Equation of the Second Order containing a Variable Parameter." Three Memoirs. *Jour. de Math.*, i. and ii., 1836–37.

"On the Integration of the Equation $\frac{du}{dt} = \frac{d^3u}{dx^3}$." *Jour. de l'Éc. Polytech.*, xv., 1837.

"On the Theory of Linear Differential Equations, and on the Development of Functions in Series." *Jour. de Math.*, iii., 1838.

"On the Integration of a Class of Differential Equations of the Second Order explicitly Infinite Terms." *Jour. de Math.*, iv., 1839.

Some of Liouville's most important work was in the department of applied mathematics. When, in 1834, Jacobi enunciated his theorem that an ellipsoid with three unequal axes is a possible figure of equilibrium for a mass of rotating fluid, and challenged the French mathematicians to give a proof, Liouville at once published one in the *Journal de l'École Polytechnique*, xiv., 1834. He afterwards returned to the problem, and, in continuation of the work of Meyer on the same subject, showed that Jacobi's form is not possible

unless the ratio of the angular momentum to the mass exceeds a certain limit.—*Comptes Rendus*, xvi., 1843; *Jour. de Math.*, 1851.

In the *Journal de Mathématiques* for 1855 we have a farther contribution to this branch of hydrodynamics in the memoir entitled "General Formulæ relating to the question of the Stability of the Equilibrium of a mass of Homogeneous Liquid rotating uniformly about an Axis." The memoir, "On a passage of the *Mécanique Celeste* relating to the Theory of the Figure of the Planets" (*Jour. de Math.*, ii., 1837), in which he points out and corrects an error of Laplace, should also be mentioned.

On dynamics we have three memoirs in vols. xi., xii., and xiv. of the *Journal de Mathématiques*, dealing with certain cases in which the equations of motion of a material point, or of a system of such, can be integrated. The equations are transformed by the substitution of various systems of generalised coordinates (mostly elliptic coordinates), and then the form of the Force Function (Potential) is so specified that integration in finite terms shall be possible. The third of these memoirs, which deals with a system of material particles, is interesting mainly as regards the theory of Abelian integrals. In addition to these there are memoirs, "On a particular case of the Problem of Three Bodies," *Jour. de Math.*, i., 1856; and "On Developments of a chapter in Poisson's *Mécanique*," *Jour. de Math.*, iii., 1858.

Liouville made several contributions to Planetary Theory, among which which we may specially mention his memoir, "On the Secular Variations of the Angles between the straight lines that form the Intersections of the Orbits of Jupiter, Saturn, and Uranus." *Jour. de Math.*, iv., 1839.

In a variety of scattered notes are to be found some very important additions to our knowledge of Theoretical Dynamics. Perhaps the most striking of these is that "On a Remarkable Expression of the Quantity which in the Movement of a System of Material Particles connected in any way is a minimum in virtue of the principle of Least Action." *Jour. de Math.*, 1856. If we take the case of a single free particle, and use Cartesian coordinates, Liouville's result for the form of the integral which expresses the action is—

$$A = \int d\theta \sqrt{\left\{ 1 + \frac{\left(y\frac{d\theta}{dz} - z\frac{d\theta}{dy}\right)^2 + \left(z\frac{d\theta}{dx} - x\frac{d\theta}{dz}\right)^2 + \left(x\frac{d\theta}{dy} - y\frac{d\theta}{dx}\right)^2}{\theta^2} \right\}}$$

where θ is a function of xyz, which satisfies the equation

$$\left(\frac{d\theta}{dx}\right)^2 + \left(\frac{d\theta}{dy}\right)^2 + \left(\frac{d\theta}{dz}\right)^2 = 2(E - V).$$

Liouville gives this theorem in terms of generalised coordinates for any system of particles, and points out that it opens up a new method of treatment leading readily to all the known results of Theoretical Dynamics.

During the latter part of his life, Liouville's researches were almost entirely directed to the Theory of Numbers. From 1857 to 1873 we have a list of over 200 notes and memoirs on this subject, all published in his own journal. A few occur with earlier dates, for examples the following :—

"On the equation $Z^{2n} - Y^{2n} = 2X^n$." *Jour. de Math.*, v., 1840.

"On a Theorem of the Indeterminate Analysis." *Comptes Rendus*, x., 1840.

"On the Two Forms $x^2 + y^2 + z^2 + t^2$, $x^2 + 2y^2 + 3z^2 + 6t^2$." *Jour. de Math.*, x., 1845.

The most important of all the memoirs on this subject are the series entitled "On some General Formulæ which may be useful in the Theory of Numbers." *Jour. de Math.*, vols. iii.–viii., New Ser., 1858–1863.

Very few of the longer memoirs are devoted to Pure Geometry; but many interesting and novel geometrical theorems occur incidentally in Liouville's mathematical writings. A full account of these will be found in the third chapter of Chasles' "Report on the Progress of Geometry in France."—*Recueil de Rapports sur l'État des Lettres et les Progrès des Sciences en France*, Paris, 1870.

We may mention here some of the results arrived at in two memoirs, "On certain general Geometrical Propositions, and on the Theory of Elimination in Algebraical Equations (*Jour. de Math.*, vi., 1841), and "Developments of a Geometrical Theorem" (*Jour. de Math.*, 1844). The following results among others are arrived at :—

1. The points of contact of a geometrical surface with all the

tangent planes parallel to a given plane have a fixed centre of mean position whose position is independent of the direction of the given plane.

2. The centre of mean position of the meeting points of two algebraical curves is also the centre of mean position of the meeting points of the asymptotes of one of them with the other or with its asymptotes.

3. If through the points of intersection of a curve and a circle normals to the curve be drawn, these normals intercept on a transversal through the centre of the circle segments measured from the centre, which are such that the sum of their reciprocals is zero.

If the circle be drawn to touch the curve at P, and we take for the transversal the normal at P, this proposition gives us a construction for the centre of curvature at P.

4. Considering all the tangents to a curve parallel to a given line. The centre of mean position of the points of contact is the centre of mean position of the interventions of the asymptotes.

The centres of curvature corresponding to the points of contact have the same centre of mean position as the points of contact themselves; the sum of all the corresponding radii of curvature is zero, and the same is true of the sum of their inverses.

5. Considering all tangent planes to a surface parallel to a given plane, the sum of the principal radii of curvature at the points of contact is zero, and the same is true of the sums of their reciprocals.

The work of the scientific teacher is scarcely less important than that of the scientific investigator, although the record of the former is more perishable, being at best an oral tradition handed over by the immediate disciples of the master. It would appear that in this walk Liouville was worthy to rank with his illustrious predecessor Monge, whose pupils shed such lustre on the French school of mathematicians. M. Faye, in his funeral oration, says, "M. Liouville was one of the most brilliant professors that ever lectured. So lively was my youthful impression of his lectures that to this day I have a vivid recollection of the captivating clearness that was so peculiarly his own. Accordingly, when in later years I had the good fortune to hear him speak at the Institute, I was the less surprised at the effect which his words produced on my colleagues, who marvelled at being able, for a moment, under his guidance, to pene-

trate the most difficult questions of the higher analysis. No one, with the exception perhaps of Arago, ever produced this effect in the same degree."

His lectures at the College de France were attended by the *élite* of French mathematicians, and doubtless did much to keep alive the ardent spirit of pure mathematical research which still lives among his countrymen. Among those who either were his pupils or were indebted to his encouragement and patronage may be reckoned Le Verrier, Hermite, Bertrand, Serret, Bour, Bonnet, Mannheim, all of whom are or have been pillars of French science.

If we compare Liouville as an investigator with other great contemporaries whose rolls of achievement like his own are already closed, we can scarcely put him in the highest rank of all, along with Abel and Jacobi, whose fortune it was in the course of their discoveries to open up new fields of research and create new branches of the analytic art. Nevertheless, so profound are some of his isolated contributions, and so elegant is all his mathematical writing, that it will be long before the traces of his handiwork vanish from the fabric of mathematical science; and it seems certain that future generations will accord him all but the highest rank in the temple of mathematical fame.

Robert Wilson. By Professor Fleeming Jenkin, F.R.SS. L. and E.

Mr Robert Wilson was born in 1803 at Dunbar. In 1810 he lost his father, who was connected with the royal and mercantile navies. This brave man, after having twice reached the wreck of the "Pallas" frigate in the Dunbar life-boat, was drowned in the third attempt to reach the ship and rescue the remainder.

Mr Robert Wilson was apprenticed to a joiner, and, like many other distinguished Scotchmen of the same generation, he owed his high standing as a mechanical engineer almost entirely to his natural genius, since he does not appear to have received any special advantages in respect of education.

During his apprenticeship, and at a date considerably prior to the successful introduction of the screw propeller into our navy, he

made models of boats with various forms of screw, which worked successfully. He himself considered that the first idea of the screw propeller had occurred to him as a mere child; his first model was that of a ship $2\frac{1}{2}$ feet long, and a drawing which he published of it in later life shows a very good four-bladed screw propeller; he attempted unsuccessfully to drive this by a windmill on the boat. In 1821, after seeing the "Tourist" paddle steamer, he made some further experiments, but having to leave the sea coast he dropped the subject. In 1825 he returned to Dunbar, and again attacked the problem, trying first four blades, then three, and then two, driven by the main-spring of a clock. At first the screw was placed in front of the rudder, and the sketches since published by Mr Wilson show the exact arrangement now usually adopted. He abandoned this plan, however, in consequence of leakage at the stern tube; and in order to get the opening above water line he used two single blade right and left propellers immersed for less than half their diameter and driven in opposite directions, being placed one behind the other, and connected by bevel wheels.

In 1827 young Wilson was introduced to the Earl of Lauderdale, whose son saw a small boat about 3 feet being driven in this way. Lord Lauderdale appears to have brought the matter to the notice of the Admiralty, but the young inventor met with no encouragement in official quarters. He next exhibited his model before the Dunbar Mechanics' Institution. A record of the exhibition was made in the minutes of the institution for October 18, 1827; and the *Edinburgh Mercury* of the 29th December 1827 alludes to the invention. The Highland Society of Scotland in 1828 appointed a committee, which after seeing the small model made a grant of £10, to enable Mr Wilson to have propellers made on a larger scale. Consequently a boat 25 feet long was fitted with a pair of these screw blades, to be driven by two men with winch handles; the committee, which included two captains in the Royal Navy, reported very favourably on the performance of this boat, during a trip in Leith Roads, lasting $17\frac{1}{2}$ minutes. The model became the property of the Highland Society.

In 1832 a committee of the Society of Arts reported favourably on the trial of another model 18 feet long, fitted with the same arrangement of two blades revolving in opposite directions. The

prize committee of this Society awarded him a silver medal and a prize of five sovereigns, pointing out in their report that the stern paddles, as they call the propellers, "can be kept altogether under water and out of the reach of surf, and answer equally well in rough as in smooth sea."

Mr James Hunter of Thurston had introduced the invention successively to the Dunbar Institute, the Highland Society, and the Society of Arts. Notwithstanding the encouragement received from those Societies and the support given by various influential men, the Admiralty, to whom he again applied, declined to make any trial of the plan, and Mr Wilson had the mortification of seeing the simple screw introduced into the navy by Mr Smith of Hendon.

Mr Wilson was, however, by no means the first who had thought of a screw as the propeller of a boat, and it must be admitted that he pushed the right and left hand geared screws in preference to the simple plan which was ultimately successful. He met with some reward indirectly, becoming known to many influential persons as an ingenious and able young mechanic; and ultimately in 1880 he had the satisfaction of receiving a sum of £500 from the Admiralty for the use of his double-action screw propeller as applied to the fish torpedo.

In 1832 Mr Wilson was in business as an engineer in Edinburgh, in the North Back of the Canongate. A few years afterwards he went to Manchester, and in 1838 he was manager of the famous Bridgewater Foundry at Patricroft. That he should, with no educational advantages, have attained this position at thirty-five years of age, is perhaps as high a testimony to his ability as his connection with the screw propeller or even with his steam hammer itself. It is universally admitted that the conception of the steam hammer was due to Mr James Nasmyth, but Robert Wilson was the inventor of important details which he considered essential to its success. On the one hand, we must remember that a steam hammer at Creuzot, suggested by Mr Nasmyth's sketch, worked successfully with no assistance given by Mr Wilson; but on the other hand, there is no doubt that some details largely used in connection with the hammer, as commonly made in England, were due wholly or in great part to Mr Wilson. There was unfortunately some disagreement between him and Mr Nasmyth on this point; and indeed

it is nearly impossible, when men are working together at the improvement of a machine, to appraise with any exactness the precise share of merit due to each.

The first steam hammer made in England was delivered to the Lowmoor Iron Works in 1843. Mr Wilson left Patricroft, and became engineer to the Lowmoor Works, where in 1853 he added what is known as the circular-balanced valve to the original machine. This invention was patented by Mr Wilson. In 1856, when Mr James Nasmyth retired from business to follow the scientific pursuits by which he has greatly added to his reputation, Mr R. Wilson was recalled to Patricroft, where he became the managing partner of Messrs Nasmyth, Wilson, & Co.

Mr Wilson did not take much part in local affairs, but was for some years president of the Patricroft Mechanics' Institution. In 1873 he was elected a Fellow of the Royal Society of Edinburgh. He continued to apply himself to the management of his works until his death, which occurred on the 28th July 1882.

A list of no less than thirty patents stand in his name, either solely or jointly with others.

Mr Wilson will be remembered as worthy of mention among the group of able Scotch mechanicians who, by their power of invention, energy, and business capacity, have not only won distinction and wealth for themselves, but have added to the resources and strength of the empire

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James Young, LL.D., F.R.S. By Dr Angus Smith.

James Young was born in Glasgow, and on leaving school was engaged for some time in a joiner's shop. It is characteristic of his energy that at this time he would, during his holidays, make long journeys on foot, having on one occasion walked as far as Aberdeen, and on another having walked the greater part of the way to London, visiting places of historic interest on his way. His occupation in the joiner's shop was the occasion of his becoming a chemist. He attended the class of chemistry in Anderson's College, and his skill as a workman led to his being employed by Professor Graham, who then taught the class, in constructing

pieces of apparatus for the experiments. By his usefulness and intelligence he eventually became assistant to Professor Graham, and lectured when the Professor was absent. He held the assistantship for seven years, and accompanied Professor Graham to London when the latter obtained a chair in University College. Among his friends at Glasgow were Dr Stenhouse, F.R.S., Dr Lyon Playfair, and Charles Griffin, an eminent manufacturer of chemical apparatus.

Young now engaged in the great enterprise from which he became widely known as a public benefactor, and which was destined to bring him both fame and profit. One Mr Oakes mentioned to Dr Lyon Playfair that there was oil flowing from a pit at Alfreton in Derbyshire. Dr Playfair then told this to Young, who at once perceived what an improvement might be made in the system of domestic lighting by the utilisation of this natural product. The flow of petroleum, small though it was, from the source in question, and the results obtained from it by Young, were the means of leading the Americans to avail themselves of the vast supplies of this useful substance that are to be found in their own continent.

The discovery, however, with which his name is most intimately associated, was his mode of obtaining oils from coal and shale, by which he succeeded in producing an illuminant oil at a price which enabled him to compete with the oil that was latterly obtained in such quantities from the petroleum springs in America.

Young did not discover solid paraffin; two little bits had been produced before his time; but he saw that it could be profitably made on a large scale, and, by the methods he introduced, hundreds of tons of solid paraffin are now made annually, and by his improved processes in the manufacture of this article he has transformed the candle, as he had previously by the introduction of petroleum transformed the lamp.

He founded a chair for the advancement of Technical or Economic Chemistry in Anderson's College, Glasgow, whilst he liberally contributed to the endowment of professorships in other branches of science in that institution.

When he worked in the laboratory of Professor Graham, solid caustic soda, as now manufactured on a large scale, could only be made in small quantities in silver vessels. Dr Young first made

it in iron vessels, and caused to be recalled an order for a silver vessel to cost £1500, by showing how that alkali could be prepared in iron.

He had the degree of LL.D. conferred on him, and became a Fellow of the Royal Society. He was elected a Fellow of this Society on April 1st, 1861. He was Deputy-Lieutenant for Kincardineshire. Though his successful enterprises had brought him wealth, he was unostentatious in his habits, and of a kindly and hospitable disposition. He died in May 1883.

John Miller, M.Inst.C.E.

Mr John Miller was born at Ayr on the 26th of July 1805. He was educated at the Academy of his native town, and on leaving it entered a solicitor's office; but feeling no liking for the legal profession, he determined to abandon it for that of a Civil Engineer. After making himself well acquainted with the theory and practice of engineering, he became a partner of Mr Thomas Grainger, M.Inst.C.E., whose office was in Edinburgh. Whilst in partnership with that gentleman, he was engaged in constructing roads in various counties in Scotland, and in the south of Ireland, and was acting engineer for the Dundee and Arbroath Railway; the Glasgow, Ayr, and Kilmarnock Railway; the Edinburgh and Glasgow North British Railway. He also designed and constructed the North British Railway, Edinburgh to Berwick, and the Edinburgh and Hawick Railway; the Dundee and Perth Railway; the Stirling and Dunfermline Railway. Mr Miller was also engineer for many other lines, both in Scotland and England. In November 1845 he deposited in Parliament plans for upwards of 1500 miles of railway.

On the above railways there are probably some of the finest viaducts in Great Britain, notably the Almond Valley Viaduct, consisting of 46 arches of 50-feet span; the Dunglass Viaduct, the centre arch of which has a span of 135 feet; whilst the centre arch of the Ballochmyle Viaduct has a span of 180 feet. Mr Miller, however, always considered the Lugar Viaduct, with nine arches of

50-feet span, and four of 30-feet span, as his greatest work. The rails of that viaduct are 150 feet above the River Lugar.

Mr Miller retired from the profession of Civil Engineer in 1850. In 1868 he was returned to Parliament as one of the members for the city of Edinburgh, but lost his seat at the General Election in 1874. He purchased the estates of Leithenhopes in Peeblesshire, and Drumlithie in Kincardineshire, and devoted a great part of his time to improving them. He died on the 8th May 1883.

Mr Miller at the time of his death was Senior Member of the Institution of Civil Engineers, and was elected a Fellow of this Society in 1841.

Charles Adolph Wurtz.

Charles Adolph Wurtz was born on November 26, 1817, at Wolfheim, in Alsace. He studied at the University of Strasburg, where he completed the medical curriculum by taking the Doctor's degree in 1843. From Strasburg he went to Paris, where he occupied several positions successively, until he became in 1883 Professor at the École de Médicine; and in 1866 he was made Dean of the Faculty. In 1867 he was elected member de l'Institut, in preference to Berthelot, who was his only serious opponent. He died on the 12th May 1884, having only three weeks previously pronounced a brilliant and affectionate *éloge* at the tomb of his great master Dumas, whose successor as perpetual Secretary of the Academy he was, on all sides, designated to be.

Few chemists have done more or more remarkable work than Wurtz. His first publication is on the nature of hypophosphorous acid, which he explained; and in the course of his studies on the compounds of phosphorus he discovered the oxychloride. In hydride of copper he discovered the first definite combination of hydrogen with a metallic body. In 1848 he made perhaps his most important discovery, namely, that of the compound ammonias, which did so much to assist in establishing the type-theory of his countryman and contemporary Gerhardt. It was extended by his discovery of glycol and the consequent introduction of the idea of polyatomic alcohols. The controversy on the constitution of lactic acid, in

which Wurtz took an important part, had the effect of clearing up the distinction between the *atomicity* and the *basicity* of an acid. In 1855 he discovered the mixed alcoholic radicals by a method which has since become a standard one for the synthesis of hydrocarbons. His researches on *aldol*, a body uniting in itself the properties of an alcohol and an aldehyde, bring us down to the present date.

It would be impossible, in a notice of reasonable length, to give any adequate idea of the importance of the work done by Wurtz during the forty years of his active life of investigation, and it would be equally impossible adequately to appreciate the far-reaching effect which the school which he founded around him has had in the development of modern chemistry. Many of his most illustrious pupils remained to the last workers in his laboratory, influenced by the spirit of enlightenment with which he inspired all who came in contact with him. The personal charm which Wurtz exercised on all who were associated with him cannot be better expressed than in the words spoken at his grave by his distinguished pupil and attached friend Friedel. After extolling his rare powers as a lecturer, he says—"We see him in his laboratory, receiving with unwearying kindness even the humblest of his pupils, interesting himself in his work, and discussing his ideas as with an equal, sowing his ideas broadcast, and as happy and proud of a discovery made by one of his pupils as he was modest and unassertive of his own. Singularly open to new ideas, and afraid of no scientific speculation, however bold, provided it received the sanction of experiment, he was peculiarly fitted to promote the progress of science, and to lead it over firm and solid ground. It was owing to these rare qualities that he attracted so many chemists, both French and foreign, to his laboratory. Of these many in their turn have become masters, and all will unite in saying that the time which they passed in daily association with Wurtz counts amongst the happiest and most fruitful of their lives."

Sir A. Grant.

By the death of Sir A. Grant on the 30th November 1884 the Royal Society of Edinburgh lost one of its Vice-Presidents, who took a constant interest in its proceedings; the University lost a Principal who for sixteen years administered its affairs with remarkable ability and success, and who has left a more enduring mark on its history than any Principal during the present century; and the cause of liberal education in Scotland lost one of its most enlightened and consistent supporters. Although of Scottish extraction, he was, unlike all previous Principals of the University, neither born nor educated in Scotland; and when invited at the age of forty-two to assume his position in Edinburgh, he had already gained distinction, in two widely separate spheres of usefulness, as a scholar and writer on philosophy, as a teacher and lecturer, and as an administrator of education. From the time when his own University course was finished, his whole life was devoted to the practical work or to the organisation and administration of education: first, during ten years, from 1849 to 1859, in the University of Oxford; next, for nine years, from 1859 to 1868, in the Presidencies of Madras and Bombay; and finally, for sixteen years, from 1868 to 1884, in the University of Edinburgh. In Oxford and in India, as well as in Edinburgh, his influence is still felt and his loss regretted by many friends.

By birth he belonged, on the father's side, to an old Scottish family, the Grants of Dalvey on Speyside. His mother was of mixed French and Scottish extraction, and was the daughter of a planter in the Danish West Indian Island of Santa Cruz. The family estate in Morayshire had been sold by his grandfather, and the whole fortune, which had been invested in West India property, had been lost before Sir Alexander succeeded his father as 8th Baronet in 1858. He was born in New York on the 13th September 1826, and passed two or three years of his childhood in the West Indies. The principal part of his school education was received at Harrow, which he entered in 1839, and left as head of the school in 1845. In November 1844 he had been elected to a Balliol scholarship, and he entered on residence at Oxford in the

Easter term of the following year. He came up to the University an excellent classical scholar of the type produced by the great English public schools, and with the social tastes and disposition which are fostered in those schools. He was especially eminent in Greek and Latin composition, and the faculty of lucid and graceful statement developed by these accomplishments proved of invaluable service to him in the various administrative duties which he was called upon in after life to perform. He combined with his scholarly attainments an appreciative taste for modern literature, and especially for the great English poets, and his interest in the philosophical, which are combined with the more strictly literary, studies of the University, and in the speculative questions by which Oxford life was powerfully stirred in the years succeeding the great religious movement of which Dr Newman was the centre, was soon awakened. He read widely and discriminatingly, but with no special eye for examinations; and thus it happened that his name is remembered among those of a select few (including Clough, Mr M. Arnold, Mr Froude, Mr Freeman, M. Pattison, and others), who, by their subsequent eminence, justified the opinion that the second class in *Litteræ Humaniores* often contained men of greater power and promise, if of less minute knowledge, than the first. He graduated in 1848, and in the following year he was elected, out of a large number of candidates, to an Oriel Fellowship. As circumstances had made it necessary for him to support himself from this time forward by his own exertions, he immediately became one of the private tutors, a class somewhat like that of the *privat-docenten* in the German Universities, who performed a much more important part in Oxford education in those days than they do at present. The preparation of the best men for their final examination in philosophy was almost entirely in their hands. Although most of them were men of older standing, he very soon was recognised as the most eminent of the body, and amongst his pupils were several men who have since obtained distinction in various walks of life, who acknowledged the benefit they derived from his instruction. He lived with his pupils on the most easy and familiar footing, and attached them to himself by his friendliness and social geniality. At the same time, he taught his subject—the Nicomachean Ethics of Aristotle—more thoroughly than it had been taught in England

before his day. While fully realising the living interest which the book, regarded as a treatise on human nature, has for all times, he was one of the first to recognise the truth, now universally acted upon, that it was to be interpreted, not vaguely and arbitrarily in accordance with any theological bias or with the moral sentiment of our own time, but historically in accordance with the evolution of Greek thought and the conditions of Greek life, and with the whole system of the Aristotelian philosophy. The mature result of his study and teaching was his edition of the *Ethics of Aristotle*, the first volume of which was first published in 1857. It is on this work, of which a fourth edition appeared a few weeks before his death, that his reputation as a scholar and a writer on philosophy mainly rests. Though it is more than a quarter of a century since it was given to the world, and though during all that time the subject has been assiduously studied and taught at Oxford, his edition still remains the standard one, and among English scholars his name is as familiarly associated with the *Ethics of Aristotle* as that of Conington with Virgil, and of Munro with Lucretius. In proof of the estimate still formed of its merits by those who are constantly using it, I may be allowed to quote the words of one of the most competent among the younger tutors at Oxford. While admitting that the work is exposed to some criticism in the present day, he adds—"We are too apt not to realise how much such a work has done directly and indirectly for the appreciation of Greek philosophy in this country. It was the first and it still remains the only attempt in any language to unite a scholarly study of the very difficult text with a literary and philosophical appreciation of the treatise in its relation to the whole history of Greek thought. Certainly no one of the German editions attempts anything so extensive, and only one of them (in Latin) has a philosophical value." He goes on a few sentences later—"In Edinburgh his name will always be associated with a most brilliant period in the history of the University. Throughout the world of English-speaking scholars he will be remembered as one of those who have set before themselves and others an ideal of scholarship which excludes neither philosophical thinking nor a regard for literary excellence. We are sometimes apt to boast that this is a specially English or even a specially Oxonian ideal; we are too often

reminded that few even endeavour to attain it, and any of these few can ill be missed." *

It is no paradox to say that even the defects of the work, such as they are, as well as the great merits which make it the best introduction to the study of Greek ethical philosophy, are connected with what was his greatest quality—the largeness and breadth of his nature. It was not possible for him to become a pure specialist—a mere scholar, or abstract thinker, or man of letters. A complete change in his circumstances, which took place shortly after the publication of this work, made it clear that he was rather a man of great general capacity, fitted to obtain success and eminence in any important province of life, than one born with the special bent and genius of a scholar or philosopher. During the last twenty-five years of his life it was to the sphere of action more than to that of thought and research that his energies were directed; and, however great may have been the loss to the University of Oxford, and to classical learning, caused by this diversion of his powers, there is little doubt that his own capacities were expanded by it, and that he was enabled to do more useful work in the world than if he had been appointed to the Professorship of Moral Philosophy in Oxford, for which he was an unsuccessful candidate in the year 1859. His marriage in that year with the daughter of Professor Ferrier of St Andrews, and the grand-daughter of "Christopher North," was the immediate cause of his seeking a new career in India, and was probably the remote cause of his final connection with the University of Edinburgh. He accompanied Sir Charles Trevelyan to Madras, and began his career in that Presidency as Inspector of Native Schools. From Madras he was soon called to the Presidency of Bombay, where in rapid succession he filled the posts of Professor of History and Political Economy in the Elphinstone College, of Principal of that College, of Vice-Chancellor of the University of Bombay, of Director of Public Instruction, and of Member of the Legislative Council in the Presidency. The best work of his life was probably that which he gave to India, during the nine years of his active employment there. His name was soon as familiarly associated with Bombay as it had been, and still is, with the *Ethics of Aristotle.* An important Government minute of the 3rd October

* *Oxford Magazine,* January 21, 1885.

1868, after his appointment as Principal of the University of Edinburgh, affirms that he had "undoubtedly set his mark on the history of education in India." It adds—"While supporting the complete independence of the University, he used it as the crown of the Government educational system." In a despatch written about the same time to the Governor of the Presidency, the Duke of Argyll, then Secretary of State for India, speaks of "the solidity and reality of his administration," and concludes with expressing "concurrence in the just remarks recorded by your Excellency in Council, relative to the very valuable services rendered by Sir A. Grant to the cause of education in India." A minute of the University of Bombay, of the same time, speaks of "his ability in administration," of "his important suggestions and effective aid in the revision of the bye-laws of the University, especially as bearing on the extension, arrangement, and balance of studies," of "his temper and tact when discharging the duties of the chair," and of "his extensive influence with the public in the matter of endowments and beneficiaries." Great as his intellectual gifts of organisation and administration were, the power of his personality was still more remarkable. Along with his general interest in Indian education he combined a warm personal interest in individuals, and the aid which he afforded to the advancement of able and deserving men among them is still gratefully remembered by natives of India.

He entered on his duties in Edinburgh in the beginning of the winter session of 1868–69, and continued during the remainder of his life to perform them with ever-growing capacity and knowledge, and with the most loyal attachment to the institution to which he came as a complete stranger. With his sound practical sagacity he combined a high imaginative faculty, and while minutely attending to and mastering the details of business, he set constantly before himself the ideal of what the University ought to be as a nursery of intellect and character, and as an organ for the elevation of national life. He gained the entire confidence of his colleagues in the Senatus, whether they agreed or disagreed with him on particular questions, by the impression he produced of absolute devotion to the good of the University. He gained the regard and admiration of the students by his frank, dignified, and cordial bearing in all his relations with them, and by his genuine sympathy with them in their aspirations,

their work, and their amusements. He wished every one to feel as he did, proud of his University, and determined to uphold its credit by intellectual effort and by honourable conduct.

Although the pursuits of the last twenty-five years of his life tended to force him into the groove of action, rather than of letters, yet they were by no means barren in literary results. In India, besides delivering several interesting addresses, which may still be read with pleasure and instruction, he was a frequent contributor to the English newspapers published in the Presidency. His recently published *History of the University of Edinburgh* is the most important literary product of his later years. Inspired and pervaded by his idealising love of his University, it is a work at once of learned research and of strong human interest in its record of many of those by whom the chairs in the University were filled at various times. His Lives of Aristotle and of Xenophon, undertaken for Blackwood's series of Ancient Classics, are written with scholarly taste and simplicity, and with that insight and vivacity of feeling which, without vulgarising it, can invest an ancient theme with modern meaning. His last address to the students, delivered only a few weeks before his death, affords more than his more elaborate works a true image of the man, in his intellectual power, his serious enthusiasm, his large-heartedness, the dignity and simplicity of his bearing. It produces an indefinable impression of greatness. His colleagues in the University, certainly, will always think of him as their "greatest, yet with least pretence."

No record of his career would be complete without some reference to the services which he rendered when a member of the Scotch Education Board. His most eminent colleague on that Board ascribes to him the chief credit in preparing the First Scotch Code, which was "a great improvement on anything of the kind previously prepared." He adds—"My own clear impression is, that no man ever knew about educational organisation from top to bottom better than Grant." His eminence as a scholar and administrator was recognised by the Universities of Oxford and Cambridge, of Edinburgh and Glasgow, which conferred on him their honorary degrees of D.C.L. and LL.D. The most enduring monument of his Principalship will be the New University Buildings, which owe more to his active services and his personal influence than to any other in-

strumentality. The great Tercentenary celebration of 1884 will, through all the future history of the University, be associated with his name. The conception of the celebration was altogether his, and its successful realisation owed more to him than to any one else. The shock of his unexpected death, on the 30th of November 1884, following so quickly on the memorable events of the preceding April, is still fresh in the memory of his colleagues in the University and in this Society.

JAMES NAPIER. By Robert R. Tatlock, F.I.C., F.C.S., F.R.S.E.

James Napier was born in the village of Partick, one of the suburbs of Glasgow, in 1810. His father was a hand-loom weaver in humble circumstances, and his mother was a sempstress. At the age of seven or thereby he was sent to a small day school in the village, kept by Mr Neil, a medical student, where in less than twelve months he learned to read with comparative fluency. On account of the straitened means of his parents, however, he was then sent to work, and found employment as a "tearer" in a calico printing works, his remuneration being 1s. 3d. per week. When he was between twelve and thirteen years of age he was put to his father's trade, and, being conscious of the limited character of his education, he endeavoured successfully to earn a little money, by extraneous efforts of various kinds, to enable him to attend a night school for two winters, by which his writing and knowledge of arithmetic were greatly improved.

Owing to dulness in the weaving trade, he betook himself to that of a dyer, and was employed by the Messrs Gilchrist at their works, Meadowside, Partick, where, at the age of eighteen, he was promoted to the post of foreman "piece dyer," his wages being then 11s. per week. When only twenty-one years of age he married, on the slender income of 13s. per week. About the year 1833, on account of the dull condition of the dyeing trade, a trades-union was formed among the workmen, in which he joined, and would not be dissuaded, even by offers of extra remuneration from his employers, in consequence of which he was dismissed. He was next employed as a

dyer at Glasgow Field Bleach Works, where he remained for four years, after which, his health failing, he endeavoured to earn a subsistence by keeping a lending library, but without success, and ultimately returned to the dye-works where he was first employed, in the capacity of a clerk. Prior to this Mr Napier had written an essay of great excellence on dyeing, which had attracted the notice of the late Mr John Joseph Griffin, who combined the business of a dealer in chemical and philosophical apparatus with that of a publisher, in Glasgow, and afterwards in London. He accepted an appointment in this establishment to prepare and bottle chemical reagents, and to make up apparatus—an employment which he found very congenial, as in some autobiographical notes which he has left, he says :—" My position brought me into contact with all sorts of inquirers; people in different trades came, not only to buy apparatus, but to question about difficulties. I had access to all kinds of chemistry books, and gave willing search to help them, thus gaining a knowledge of different trades; but I wanted system, and to improve myself in this respect, I invited the members of our Mutual Instruction Society to my house, and went through a course of chemistry, following Graham's work. By these means, and by nightly study, I obtained a pretty good knowledge of the principles of the science." It was, doubtless, while in this employment that he made the acquaintance of the late Dr James Young, F.R.S., of Kelly, at that time laboratory assistant to the late Professor Graham, of Anderson's College, Glasgow, who afterwards became Master of the Mint, which resulted in a life-long and most friendly intimacy. In the year 1839 the results obtained by Mr Thomas Spencer in the new art of electrotyping and electro-metallurgy excited much interest, and Mr Napier, on Mr Griffin's account, carried out some laborious and important work with the object of applying the art to useful purposes, such as copying woodcuts and engraving plates, &c.; and in 1842 he was appointed to take a leading position in the London electroplating works of Messrs Elkington & Mason, where, it is needless to say, he discharged his duties in a manner which reflected the highest credit upon him, some of the work which he turned out being truly very fine. The interest he took in the process of copper-smelting led to the discovery of a great improvement in the refining and granulation of copper, by the application of soda ash, in which

he encountered much opposition, notwithstanding which the process was taken up and wrought by a private company in 1847, who acquired the Spitty Works at Swansea for the purpose, the result of which was that at the end of the first year the books showed a net profit amounting to £19,000, or upwards of 55 per cent., £900 of which fell to Mr Napier's share. In 1844 he devised and described a process for extracting silver from its ores by calcination with common salt, which was, in principle, identical with the "wet process," devised by Mr William Henderson many years later, for the extraction of copper from poor ores. He also, some time thereafter, patented a method for the removal of tin, antimony, arsenic, &c., from poor Cornish ores. In 1852 he revised and extended some magazine papers which he had previously published, and they were published and issued by Messrs Griffin & Co., under the title of *A Manual of the Art of Dyeing.* This was succeeded by his well-known book entitled *A Manual of Electro-Metallurgy,* which in 1860 reached a fourth edition.

Mr Napier returned to Glasgow in 1849, and to his native place—Partick—in 1852, where he engaged in literary work and interested himself in its sanitary condition, analysing its potable waters, and instigating the movement which led to Partick being made a Police Burgh for its own local government. For several years he thus occupied himself, but retained his laboratory, employing himself as an investigating and consulting chemist. In 1860 he was requested by the Marquis of Breadalbane to visit and inspect a copper mine on his estate at Killin, on the south side of Loch Tay, which had been worked for years without profit, the result of which was that the works were started on the spot, under Mr Napier's superintendence, for preparation of copper regulus and the manufacture of sulphuric acid and artificial manures, but owing to the failure in the quality of the ore from the mine these were soon suspended. In 1861 he returned to Glasgow, and commenced business as consulting chemist, where he continued till 1864, when he erected sulphuric acid works at Vinegar Hill, near Glasgow, the operations of which occupied his time and attention for six or seven years. Being then relieved of the active management by his son, he returned to his literary pursuits, and soon produced his *Notes and Reminiscences of Partick* (1873), *Ancient Workers in Metals, Manufacturing*

Arts of Ancient Times (1879), and *Old Ballad Folk-Lore* (1879).

From the annexed list of his papers it will be seen that Mr Napier has earned for himself a recognised position in general science, and particularly in the special branches of technical chemistry and metallurgical science which were so congenial to him; and that, with a more liberal education, his mental activity and aptitude for scientific study would have won for him a great name in the scientific world.

Mr Napier was elected a Fellow of this Society in 1874. He was also a Fellow of the Chemical Society, and took much active interest in the local scientific societies with which he was connected, particularly the Philosophical, Natural History, and Archæological Societies of Glasgow. He was likewise a zealous worker in the management of Anderson's College, and of the "Young" Technical School, Glasgow, of which he was a trustee. The loss of his wife in the year 1881 proved a heavy blow to him, from which he never fully recovered; and on 1st December 1884 he terminated an active and useful life at the age of seventy-four, esteemed and respected by all who knew him.

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12. On Copper-Smelting. *Phil. Mag.*, iv., 1852, pp. 45–59, 262–271, 345–355, 453–465 ; v., 1853, pp. 30–39, 175–184, 345–354, 486–493.
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16. On Incrustations in Steam Boilers. 1858. *Glas. Phil. Soc. Proc.*, iv., 1860, pp. 191–198.
17. Analysis of a small portion of Stone fused by Lightning. May 1854. *Glas. Phil. Soc. Proc.*, iv., 1860, p. 206.
18. On Trap Dykes between Condon and the south end of Whiting Bay, Island of Arran. *Glas. Phil. Soc. Proc.*, iv., 1860, pp. 321–324.
19. Black and Clayband Ironstones ; their Composition and Valuation. *Glas. Phil. Soc. Proc.*, v., 1864, pp. 210–217.
20. On the Cyanides of the Metals, and their Combination with Cyanide of Potassium. *Chem. Soc. Mem.*, ii., 1843–45, pp. 82–96 ; *Phil. Mag.*, xxv., 1844, pp. 56–71. (This paper was prepared conjointly by Mr Napier and Mr C. J. O. Glassford.)
21. On the Dynamics of the Galvanic Battery. *Phil. Mag.*, xxii., 1864, pp. 52–54.
22. Notes upon Dyeing and Dyed Colours in Ancient Times. *Glas. Phil. Soc. Proc.*, v., 1864, pp. 175–197.
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25. On some Popular Superstitions common in Partick forty years ago. *Glas. Archæol. Soc. Trans.*, i., 1868, pp. 391–398.
26. The Farmer and the Chemist. *Glas. Phil. Soc. Proc.*, vii., 1871, pp. 387–398.
27. On the Results of some Experiments on the Leaves of various Trees and Shrubs. *Glas. Nat. His. Soc. Proc.*, iii., 1878, pp. 105, 106.
28. Miscellaneous Notes in Natural History. *Glas. Nat. His. Soc. Proc.*, iii., 1878, pp. 194, 195.

Communications to the "Sanitary Journal."

1. On Damp Walls and Newly Built Houses. Nov. 1, 1876, i. p. 156.
2. The best Means of Drying the Walls of Newly Built Houses. Dec. 1, 1876, i. p. 162.
3. The Rivers Pollution Scheme. April 1, 1879, iii. p. 53.
4. Water Supply for Villas. Oct. 1, 1880, iv. p. 255.

Professor Thomas Croxen Archer. By J. D. Marwick, LL.D.

Thomas Croxen Archer, Director of the Edinburgh Museum of Science and Art, was born in Northamptonshire in 1817, and was educated in London as a surgeon. When about twenty-five years of age, however, he received an appointment in the Import Department of the Customs at Liverpool, and in that service he remained, receiving successive promotions, till 1856. Having a natural taste for botany, which he had zealously cultivated as a branch of his medical studies, Mr Archer took a keen scientific interest in all the vegetable imports of Liverpool, and when the authorities of that city were invited by the promoters of the Great Exhibition of 1851 to contribute to its success, and were puzzled to know how best to do so, Mr Archer proposed a scheme which met with universal acceptance. His suggestion was that Liverpool should be represented by a complete and systematically arranged collection of specimens of all the mineral, animal, and vegetable importations into the Mersey, and this scheme was admirably carried into effect under his own direction. He was subsequently invited to write one of the official reports on the Exhibition. The services rendered by him in connection with this collection, and also as agent in Liverpool for the Exhibition, were recognised by a medal; and in the following year he was appointed agent in Liverpool for the Crystal Palace Company, who marked their sense of the value of his work for it during 1852–3 by conferring three medals upon him. In the latter of these years Mr Archer contributed a volume on "Economic Botany" to a series of popular works on Natural History published by Lowell, Reeve, & Co., of London. About this time Sir William Hooker was forming the great museum of Economic Botany at Kew, and had much correspondence on the subject with Mr Archer, who afterwards set himself, with characteristic energy, to make a somewhat similar, though smaller, collection for the Royal Institution at Liverpool. His interest in botany also attracted him to the Botanic Gardens of the city, in which he took an active concern, and led to many excursions in the neighbouring counties,

in the pursuit of his favourite study. He was appointed Lecturer on Botany in the Medical School at Liverpool, and afterwards Professor of Botany in Queen's College and in Blackburn College of the same city.

On 10th May 1860, Mr Archer became Director of the Edinburgh Industrial Museum, a name which was subsequently, by order of the Committee of Council on Education, changed into that of the Edinburgh Museum of Science and Art. The office had become vacant by the death of Professor George Wilson, who had done much, by the charm of his persuasive advocacy, to commend the objects of the museum to the public. But the state of his health, and the want of any suitable building in which the necessary collections could be exhibited, prevented much progress being made. Shortly after Mr Archer's appointment, however, the requisite steps were taken by the Government to proceed with the erection of a suitable museum, and in 1861 the foundation stone of the present building was laid by the lamented Prince Consort. To all the work connected with that ceremony—with the subsequent completion of the structure—with the transference to it of the collection made by Professor George Wilson, and also of his own private collection of upwards of two hundred specimens, chiefly of vegetable products used in the Arts—and with the supplementing and completing of these collections, Mr Archer devoted himself with untiring energy. What written appeals did not succeed in obtaining for the museum, personal solicitation at the various centres of industry rarely failed to secure. The enthusiasm of the director carried everything before it, and year after year he went over various countries in Europe, visiting important seats of manufacture and noteworthy art collections, and carrying back with him, as purchases or free gifts, the results of his untiring labours to enrich the museum at Edinburgh or South Kensington. When the eastern wing of the present building was completed, everything that Mr Archer could do to hasten on the extension of the central portion was done, and when the central portion was completed, no effort on his part was spared to induce the Government to complete the entire structure by the erection of the west wing now in progress. This he had the satisfaction of seeing commenced. In the arrangement and classification of the museum, as it now exists, and in the

completeness with which every exhibit is made to tell its own story to the student, there remains, and will, it is to be hoped long remain, the evidence of Mr Archer's loving care and devotion to his work.

Mr Archer was appointed one of the jurors of the International Exhibition of 1862, and, along with Mr Peterson of the office of Crown Domains in St Petersburg, wrote an official report on the vegetable substances used in manufactures shown at that Exhibition. His services in relation to this work were acknowledged by a medal; and in the following year he was appointed a Corresponding Member of the Ministry of Crown Domains of Russia. He acted as Associate Commissioner at the Paris Universal Exhibition of 1867, and reported on the class of exhibits connected with forest products, for which report he received three medals. In 1870 he reported on the International Exhibition in London; and in 1871 he was commissioned by the British Government to attend the Exhibition at Moscow and Copenhagen. He was appointed a member of the Committee for Selection of the annual International Exhibition of 1872, for which Exhibition also he acted as Deputy Commissioner for Scotland. In 1873 he was appointed a juror at the Vienna Exhibition held in that year, and he prepared several reports for the British Commission in connection with it. His services at this Exhibition were acknowledged by the decoration of a Commander of the Order of Franz-Joseph of Austria. In the same year he was awarded the gold medal of the Russian Ministry of Crown Domains; and, in recognition of literary and scientific merit, was appointed Chevalier of the Order of St Hiago of Portugal. In 1876 Mr Archer was appointed Executive Commissioner for Great Britain and Ireland, along with Colonel Sandford, at the Philadelphia Exhibition of 1876, and in recognition of this service he was awarded a medal. In 1878 he acted as one of the jurors in the Paris Universal Exhibition of that year.

Mr Archer was connected with many literary and scientific societies. He was a director of the Royal Institution of Liverpool, a life member of the Liverpool Literary and Philosophical Society, an honorary member of the Liverpool Chemists' Association, a member of the Liverpool Microscopical Society (of which he was for several years secretary), an honorary member of the Birkenhead

Philosophical Society, a president of the Botanical Society of Edinburgh, a member of Council of the Royal Society of Edinburgh, and a member of the Royal Society Club, a president of the Royal Scottish Society of Arts, a secretary of the Edinburgh Microscopical Society, an honorary member of the Edinburgh Geological Society, a fellow of the Society of Antiquaries of Scotland, an honorary president of the Edinburgh Association of Science and Art, an honorary member of the Pharmaceutical Society of Great Britain, and an honorary member of the Pharmaceutical Society of Austria, the Philosophical Society of America, Philadelphia, and the Franklin Institute of Pennsylvania.

He was a liberal contributor to the *Transactions* of the various literary and philosophical societies with which he was connected, to the *Art Journal*, and *Journal of the Society of Arts*, to the eighth edition of the *Encyclopædia Britannica*, to *Chambers's Encyclopædia*, and to other works.

Called frequently to London on the business of his Department, Mr Archer had occasion to be there in February 1885, and had arranged to return to Edinburgh on the 19th of that month. Two of his daughters had joined him in the Midland Grand Hotel, and were proceeding to breakfast, intending afterwards to accompany him to the railway station, when, without premonition of any kind, he fell down in the hall and expired. He was predeceased in 1879 by his wife, and is survived by one son and four daughters. His eldest son and a daughter predeceased him.

Mr Archer was a man of great energy of character, and of wide and varied information. Throughout life he enjoyed exceptional facilities for becoming acquainted with an infinite variety of men and things in this country and abroad, and these facilities he utilised to the fullest extent. His retentive memory supplied him with inexhaustible sources of interesting conversation, and—underlying occasional apparent sharpness of manner—there were unfailing kindness, a high sense of honour, and, to those who knew him best, a depth and tenderness of feeling which were irresistibly attractive.

The writer of the present notice enjoyed Mr Archer's friendship for five-and-twenty years. He had the privilege of accompanying him in travels abroad, and enjoying his close friendship at home, and

the result of that long and intimate knowledge entitles him to say that no more faithful public servant, no truer or more reliable friend, could be desired than Mr Archer was.

P.S.—Since this notice was prepared the writer has had placed in his hands the following letter from Colonel Donnelly, the secretary of the Science and Art Department, to Mr Archer's son :—

Science and Art Department.

3rd March 1886.

Sir,—The Lords of Committee of Council on Education have directed me to convey to you and the other members of his family their sense of the loss which has been sustained by the Science and Art Department by the death of Professor Archer.

My Lords deeply regret losing so valuable an officer; and, in desiring me to express their condolence with those who are mourning for him, have instructed me to place on record their appreciation of the exceptional zeal, energy, and ability with which he at all times discharged the important duties intrusted to him at the Edinburgh Museum of Science and Art.

They consider that it was mainly owing to his exertions that this Museum, which may almost be said to have been created by him, attained such popularity and importance since it was opened to the public under his direction in 1860.—I am, Sir, your obedient servant,

(Signed) W. D. Donnelly.

Cecil Archer, Esq., 20 Greenhill
Place, Edinburgh.

Joseph Mitchell.

Joseph Mitchell, civil engineer, for long resident in Inverness, and latterly in London, was born at Forres on the 3rd November 1803. His father, John Mitchell, was appointed, under Mr Telford, as inspector of works, in connection with the extensive road works, which, with the other great works then being carried out by Mr Telford, were the first means of opening up the Highlands to full access with the southern parts of Great Britain. Mr John Mitchell held the general inspectorship of the extensive system of communication known as the Highland Roads and Bridges. It is worthy of remark that Mr John Mitchell attracted the attention not only of Mr Telford, but also of the poet Southey, who wrote in terms of high commendation, both of his talents and moral qualities.

Joseph Mitchell seems in many respects to have inherited his father's energy and abilities. He was educated at the celebrated Academy of Inverness, which for many years was under the charge of the well-known Alexander Nimmo, C.E., who was long the personal friend of Telford and of Sir David Brewster, for whose *Encyclopædia* he wrote several articles.

Mr Mitchell became a Fellow of this Society in 1843. He became a civil engineer under the immediate auspices of Mr Telford, who caused him to engage in the practical masonry of the lockgates of the Caledonian Canal at Fort Augustus, after which Mr Mitchell not only became an apprentice in his office, but an inmate of his house, a striking proof of the high opinion which that great engineer had formed of his personal qualities. Here he made the first survey of the St Catherine Docks, which were carried out under Mr Telford's superintendence.

In 1824, when only twenty-one years of age, the whole of the Highland Roads and Bridges system was, on the death of his father, put under Mr Joseph Mitchell's charge, a fact which shows how great a trust was reposed in him by Mr Telford. During the long period of thirty-nine years he had these roads, extending over the rugged and difficult counties of Inverness, Ross, Cromarty, Sutherland, and Caithness, not only to inspect but to provide additional roads and

bridges over the most difficult ravines and passes and the most formidable kind of rivers. There was in all this difficult country ample room for varied engineering practice, which was throughout of a most successful type. Besides all this Government work, he carried on an extensive private practice, involving the expenditure of about £180,000. He was also employed by Government to design and erect forty churches in outlying districts and lonely islands.

In 1828 he was appointed engineer to the Scottish Fishery Board, and designed and superintended the execution of very many useful harbours, as for example at Burnmouth, Coldingham, and Dunglass, in Berwickshire, at Buckhaven and Cellardyke on the Fife coast, as well as at various fishing stations on the coasts of Caithness and among the Hebrides. The large fishing harbour at Dunbar, which cost nearly £40,000, was also one of Mitchell's, as well as Lybster, on the Caithness coast, where, in order to avoid as much as possible conflict with the open sea, he preferred to recess the basin landwards of high water, instead of carrying the works outwards. He also designed improvements at Wick on the same principle. Mr Mitchell is well known as having been the engineer of the extensive work known as the Highland Railway between Perth and Inverness, as well as of most of the railways to the northward of Inverness. Mr Mitchell, in conjunction with Messrs William and Murdoch Paterson, was engineer for the Skye line.

Mr Mitchell has left behind him so many works of a varied, and some of them of a difficult nature, as to prove his natural sagacity and skill. He was always highly esteemed by his professional brethren for his geniality and high professional honour as well as his ability. True and genial as a friend, his death was felt by many as a personal loss, both in Scotland and in London, where he principally lived in later years. He died in London on 26th November 1883, at the advanced age of eighty years.

Professor Henry Charles Fleeming Jenkin. By W. H. P.

Professor Henry Charles Fleeming Jenkin, only child of Captain Charles Jenkin, R.N., of Stowling Court, Kent, and his wife (Cora Jackson), a Scotchwoman and a novelist of some mark, was born in Kent on the 25th of March 1833. Fleeming Jenkin was at the age of seven taken to Scotland, when he went to Dr Burnett's school at Jedburgh. There he stayed for three years, and then for other three years attended the Edinburgh Academy. In 1847 he went to school in Paris, where he saw the Revolution of 1848, of which he was wont to give vivid and interesting descriptions; and after the June riots he left for Italy, where he attended the University of Genoa in the Arts Faculty, taking his degree as Master of Arts in 1850. It was at Genoa also, in a locomotive shop, that he began his distinguished career as an engineer, under Philip Taylor of Marseilles. In 1851 he returned to England, and was apprenticed to Fairbairn's in Manchester for three years. His first practical work was done under Mr Hemans, on a survey for the Lukmanier Railway, in Switzerland; then he went to Messrs Penn at Greenwich; and then to Messrs Liddell & Gordon, in railway work. Thence he went to Messrs Newall (Birkenhead), while they were engaged on making the first Atlantic cable in 1857. He was very soon entrusted with the chief management of the Messrs Newall's engineering and electrical business. His work included superintendence of machine construction in the factory, the designing of picking-up and paying-out machinery, the fitting up of steamships for submarine cables and the electrical testing. This work he continued to do during the making of part of the first Atlantic cable, of the Red Sea cable, of a cable from Singapore to Batavia, and of several Mediterranean cables. In 1859, the year of his marriage with Ann, daughter of the late Mr Alfred Austin, C.B., he was elected Associate I.C.E., and began to write on scientific subjects, encouraged thereto by Professor (now Sir William) Thomson, whom he had first known through Mr Gordon, of Liddell & Gordon. In 1860 he took out a patent jointly with Sir William Thomson for signalling apparatus through long submarine cables, and in 1861 he entered into a partnership, which lasted

seven years, with Mr H. C. Forde for general and telegraphic engineering. In the same year he acted as second in command under Sir William Thomson at the establishment of the British Association Committee on electrical standards. Of this committee he was appointed secretary, and he wrote their reports for several years. He also carried out most of the committee's important experiments in conjunction with Clerk Maxwell and others. He was juror for Physical Apparatus at the 1862 Exhibition, and was named Reporter for Electrical Apparatus. In 1865 the late C. F. Varley joined Sir William Thomson and Fleeming Jenkin in an agreement to work at the development of the signalling apparatus through long submarine cables already referred to. The apparatus devised by the three inventors was used by all the great companies, and Jenkin displayed a remarkable business faculty in the commercial management of the patent. In this same year Jenkin was elected a Fellow of the Royal Society, and in 1866 he was appointed Professor of Engineering at University College, London. In 1868 he dissolved partnership with Mr Forde, and resigned his post at University College, in order to accept the Chair of Engineering in Edinburgh University, which chair he filled until his death. After accepting this post he entered into a new partnership with Sir William Thomson, under the provisions of which Sir William and he acted as joint engineers to various submarine cable companies. Among the lines which were laid under their direction were those of the Western and Brazilian Telegraph Company, the Platino-Brazileien Telegraph Company, the West Indian and Panama Telegraph Company, and the Mackay-Bennett or Commercial Cable Company. In 1871 Fleeming Jenkin was President of the Mechanical Section of the British Association, which met that year at Edinburgh; and in 1873 he went to Brazil in the interests of the Western and Brazilian Telegraph Company. In 1877, in a lecture at Edinburgh given for the Edinburgh Philosophical Institution, he took occasion to propose the establishment of a Sanitary Protection Association. In consequence the Edinburgh Sanitary Protection Association was started, and succeeded so well that its example has since been followed in many parts both of Great Britain and of the United States. To most of these associations in Great Britain Fleeming Jenkin was consulting engineer. At the Paris Exhibi-

tion of 1878 he was juror in Engineering, and in 1884 was juror in Electricity in the Health Exhibition in London. From 1879 onwards to the time of his death he was Vice-President of the R.S.E., and from this Society he had the Keith Prize for the period 1877–79. This, the Society's highest distinction, was awarded for his paper on "The Application of Graphic Methods to the Determination of the Efficiency of Machinery," a continuation, full, however, of originality, of the subject treated in Reuleaux's *Kinematics of Mechanism.* In 1882 he took out his first patent for "Telpherage," a system of electrical carrying of burdens, or at need of passengers. The Telpher line is a conductor, which may be either flexible or rigid, of electricity supporting an electric motor and train of steps or of travelling chairs, which hang below it, and itself supported by strong posts. The line is in electrically distinct sections, and the train, itself a continuous conductor, and larger than any section, bridges over the interval between successive sections as it passes. The power is derived from a fixed engine and dynamos placed at convenient distance from the line. The "Telpherage Company," whose first working line was opened at Glynde, four months after Jenkin's death, which took place on the 12th of June 1885, was due to Jenkin's joining forces with Messrs Ayrton & Perry, who were, like him, turning their attention to the application of electricity to locomotion. His death was due to the unfortunate result of a surgical operation, slight in itself.

Apart from his own most special work, and from the sanitary engineering work which has been referred to, and which has had wide and good influence, Professor Jenkin took a deep and keen interest in Technical Education (he was for many years a Director of the Watt Institution) and in science generally. Witness among other instances his reviews of the Origin of Species (*North British Review*, June 1867) and of the Atomic Theory, an article supported by Munro's *Lucretius* (*North British Review*, 1868). Both Darwin and Munro, in subsequent editions of their works, acknowledged the value of Professor Jenkin's criticism. In the fine arts, and notably in dramatic art in its widest sense, his interest was equally keen and wide; but by the world at large, outside his own belongings and friends, he will be remembered best for his special and admirable work in electricity and engineering.

John Watson Laidlay.

John Watson Laidlay was born at Glasgow on the 27th of March 1808. At an early age he went to London, and began his education at a private school at Blackheath.

After passing through the ordinary curriculum there, he commenced a brief course of technical study, preparatory to going out to India, and with this view entered the laboratory of Faraday, by whom he was initiated in practical chemistry. At the same time he studied Hindustani under Dr Gilchrist, and it was here that he made the acquaintance of Bishop Heber.

This period was, however, very short; when he reached only his seventeenth year it was decided to send him out at once to India, and at this point his normal education may be said to have ended, his subsequent learning, the varied extent and scholarly accuracy of which was known only to his intimate friends, being entirely the result of self-imposed study.

In the end of 1825 he reached Calcutta, and joined his uncles, Messrs John and Robert Watson, merchants and indigo planters, Bengal, who subsequently purchased from the East India Company many of their best silk filatures and indigo factories, such as Berhampore, Rampore-Beauleah, Surdah, &c.

He was now constantly in charge of one or other of the filatures, and succeeded in introducing several valuable improvements in the machinery for winding silk.

His spare time he devoted to studying science and natural history, but, above all, the Oriental languages, for which he had a very decided talent; and, in addition to the native dialects, soon made himself familiar with Persian, Arabic, Sanskrit, and subsequently Chinese.

He originated the *Bibliotheca Indica*, a serial publication of native literature, which has proved a most valuable work, and is still continued.

His love of deciphering inscriptions on ancient monuments was great; and, with a view to assisting the labours of those engaged in this work in India, he translated *The Pilgrimage of Fa Hian* from the French edition of the *Foe Koue Ki*, with additional notes and illustrations of his own.

He made a valuable collection of coins, including many uniques; a portion of these were unfortunately stolen, but the remainder, together with his collection of shells, he presented to the British Museum.

His most numerous literary and scientific publications appeared from time to time in the *Journal of the Asiatic Society*, to which he acted as co-secretary, and afterwards vice-president and secretary to the Natural History Department.

He also compiled a comparative dictionary of Chinese words, and made translations of several Persian poems and other works, which were never published.

In 1839 he visited the Straits of Malacca for his health, and there he made the acquaintance of Rajah Sir James Brook. He went home to England in 1843, where he married, and returned the following year.

The remainder of his time in India, until his final return to England in 1850, was spent at Calcutta, where he associated with the leading scientific and literary men of the day, together with many other notable people.

On leaving India he gave up active work, and shortly settled down at Seacliff, Haddingtonshire, where he spent the remainder of his life.

He was now elected a Fellow of the Royal Society of Edinburgh, in whose proceedings he took a lively interest, although prevented by uncertain health from taking an active part in their meetings. He also became a member of various other societies in this country.

At all times of a retiring and unambitious disposition, he showed little inclination to enter society afresh and without the companionship of his former friends, but preferred rather to cherish and extend those kindred studies which had been so much to him in the past. In the quiet of his unostentatious life at Seacliff, he found a never-failing source of pleasure in his library, his laboratory, and in the wider field of nature. His was essentially a pure love of scientific truth for its own sake, and, although furnished with introductions which would have brought him in contact with many celebrated literary and scientific men, his extreme humility and modesty of self-assertion prevented him from availing himself of these opportunities of bringing his own learning into greater prominence.

Thoroughly versed in the classics, he delighted to read and re-read the works of the principal authors, most of whom he could quote at pleasure. Perhaps his favourite modern language was Italian, and Dante his favourite poet. Schiller and Goethe, too, he held in high esteem, while in our own literature he was intimately conversant with all the standard authors.

In the sciences, chemistry, archæology, geology, meteorology, and natural history, each afforded an inexhaustible field of research, his varied reading enabling him to keep abreast of the latest discoveries.

In medicine his knowledge was extensive and accurate, and so late as his seventieth year he attended, for his own pleasure, a whole winter course of anatomy at the Edinburgh University, under Professor Turner.

Some time before his death his eyesight began to fail, and thus he was reluctantly forced to abandon, one by one, his favourite studies; but his memory continued clear until his death, which took place upon the 8th of March 1885.

In private life he was esteemed by all who knew him for the gentleness of his disposition, his kindness and unvarying courtesy to all, rich and poor alike; but few, save his own family, knew his completely unselfish nature, his infinite goodness of heart.

The following papers are recorded under his name:—

On Catadioptric Microscopes. *Journal of the Asiatic Society*, vol. iii., 1834.

Analysis of Raw Silk. Vol. iv., 1835.

On the Rate of Evaporation in the Open Sea. Vol. xiv., 1845.

On the Coins of the Independent Mohammedan Sovereigns of Bengal. Vol. xv., 1846.

Sanskrit Inscription from Behar. Vol. xvii., 1848.

Daily Evaporation in Calcutta. *Ibid.*

Note on the Nido-Scythia Coins. *Ibid.*

Note on the Inscriptions found in Province Wellesley. *Ibid.*

Notice of a Chinese Geographical Work. Vol. xviii., 1849.

Note on an Inscription from Keddah. *Ibid.*

On Preparing Fac-similes of Coins, &c. *Ibid.*

On the Connection between Indo-Chinese and Indo-European Languages. *Journal of the Royal Asiatic Society*, vol. xvi. p. 59, 1856.

Francis Brown Douglas, Esq. By Professor Duns, D.D.

Francis Brown Douglas was born at Largs, Ayrshire, April 2, 1814. His father was Mr Archibald Douglas, advocate, Edinburgh, and his mother was a daughter of Dr Francis Brown of St Vincent. He was educated at the Edinburgh High School, and for a short time in England. In early youth he went to the West Indies, where he was soon called to undertake the management of family estates. On his return to Scotland he studied for the Scottish Bar, and was admitted an advocate in 1837. He practised at the Bar for a short time only. Having an ample private fortune, he felt himself at full liberty to follow pursuits to him more congenial than the hard work of his profession, and accordingly he devoted his time and energy earnestly to municipal, philanthropic, and religious work. He was elected a Fellow of this Society in 1839. A man of general culture, he was intimately acquainted with several branches of current literature, and took a warm interest in public education. He was elected a member of the Edinburgh School Board when it was formed in 1872, and continued to serve in it till last election. Mr Brown Douglas entered the Edinburgh Town Council in 1850, and, after having acted as a magistrate for several years, he was chosen Lord Provost of the city in 1859. Two events occurred in the course of his Lord Provostship which may be mentioned, viz., the passing of the Annuity Tax Act, and the laying of the foundation stones of the New Post Office and the National Museum by the Prince Consort. Mr Brown Douglas was a Liberal in politics. He stood for the representation of the city in 1856, but was defeated. Later, he became a candidate for the St Andrews Burghs, but was again unsuccessful.

It was, however, chiefly in connection with religious and philanthropic work that he stood prominently out before his fellow-citizens. For more than forty years his name was associated with almost all movements of this kind. He threw himself with great earnestness and zeal into the Scottish ecclesiastical controversies that characterised the decade ending in 1843. He continued till the time of his death in August 1885 to take the most active and cordial interest in the work of the Free Church, both at home and abroad,

and succeeded Mr David Maclagan in the convenership of its Continental Committee.

Mr Brown Douglas devoted a great deal of time and attention to some aspects of philanthropy which do not bulk largely in the public eye, but are full of good to many. He was for forty years president of the District Sick Society. He took an active share in the Society for Teaching the Blind in their own houses, in the Indigent Old Men's Society, and other kindred institutions—bringing, as director or as member, to their affairs sound practical wisdom, excellent business habits, wide views, keen sympathy, and helpful liberality.

Mr Brown Douglas was twice married—first in 1845, to Mary, second daughter of the late Charles Maitland Christie, Esq. of Durie, and again, in 1852, to Marianne, second daughter of the late Hon. Alexander Leslie Melville, who, with four sons and six daughters, survives him.

David Maclagan, F.R.S.E. By Professor Duns, D.D.

David Maclagan was born in Edinburgh on the 9th of October 1824. His father, Dr Maclagan, a distinguished physician, had retired from the Army Medical Service after a noted career, and had settled down to a highly successful civil practice in Edinburgh. His mother was Miss Whiteside of Ayr. David was the fourth of seven sons, six of whom survive—Professor Douglas Maclagan, M.D., Dr Philip Whiteside Maclagan, Berwick-on-Tweed, General Maclagan, R.E., William Dalrymple Maclagan, Lord Bishop of Lichfield, John Thomson Maclagan, Secretary of the Church of Scotland's Widows' Fund, and Dr J. M'Grigor Maclagan, Riding Mill-on-Tyne. Mr Maclagan was educated at the Edinburgh High School. He began business life in the office of "The Scottish Union Insurance Company," and was appointed manager of "The Insurance Company of Scotland" in 1847. He was an original member of the Society of Accountants, a body which was incorporated by charter in 1854. Mr Maclagan removed to London in 1862, on his appointment as secretary to "The Alliance Fire and Life Insurance Company." In this position, his business associations

brought him into close relations with the Rothschilds, Mr Goschen, Sir Moses Montefiore, and other well-known financiers. His acquaintanceship with Sir Moses soon ripened into intimate and lasting friendship. Mr Maclagan returned to Edinburgh in 1866, and entered on the managership of the Edinburgh Life Assurance Company, an office which he retained till his death on the 30th of March 1883, at Mentone, whither, for health's sake, he had gone to spend the winter. In 1848 Mr Maclagan married Miss Jane Finlay, who, with five sons, still survives.

On his return to his native city he threw himself with great heartiness and zeal into religious and philanthropic work. He was one of the most earnest promoters of the Apprentice School Association, a society which at the time did much good, both by supplying the means of education to a much neglected class, and also by leading the way to better arrangements for the same purpose. Mr Maclagan was elected a Fellow of this Society in 1872.

A man of academic tastes, fond of literature, the intimate friend of many engaged in literary and scientific work, and himself earnestly interested in the growth of knowledge, he yet found himself, in a great measure, precluded from practical literary effort by the onerous duties of his business position, and the devotion to work necessary to success in his profession. He had, however, as a relaxation and a delight, early cultivated the habit of the pen, and he has left good evidence of his scholarly accomplishments, intellectual vigour, and fine taste. From boyhood almost Mr Maclagan had taken great interest in Scottish religious and ecclesiastical movements. The stirring events which preceded and immediately followed the Scottish Church crisis of 1843 greatly influenced him. He entered into them with an earnestness and fervour in strong contrast with his wonted quiet and placid habits of thought. And though some rough points of his churchism may have afterwards been a good deal smoothed down, there was not, through life, any abatement of his early enthusiasm and zeal. Yet few men were freer from sectarian narrowness. His toleration for the views of others was as characteristic of him as the firmness with which he held his own. The writer, as Secretary of the Free Church College Committee, was associated for more than ten years with Mr Maclagan in work in which he had good opportunity to observe the breadth and the balance of his well-

stored mind, the singular wisdom of his counsel, his sound judgment, and practical sagacity. Mr Maclagan was for many years a member of this Committee, and the Convener of its Finance Sub-Committee by which the Committee itself is guided in its administration of College property and finance. Reference is made to this to indicate the great business ability, tact, and shrewdness which Mr Maclagan brought to bear on it. Mr Maclagan was also for several years Convener of the Free Church's Continental Committee—an office which had been previously held by Sheriff Jameson, and, later, by Sheriff Cleghorn. He was an effective public speaker, and never spoke on any question of interest but when he had something to say, while his utterances were always clear, pointed, earnest, and telling. Notwithstanding the engrossment of business life, he did a good deal of literary work, mostly of a biographical kind. His last effort was a brief, but hearty, appreciative notice of his friend the late Mr Samuel Raleigh, written at the request of the Council of this Society. The death of Mr Maclagan was the removal from a wide circle of friends, and from the Fellows of this Society, of an accomplished Christian gentleman. In his able and touching "Memorial Sketch" of Thomas Cleghorn, "one of his dearest and truest friends," Mr Maclagan wrote of Sheriff Jameson in terms singularly applicable to himself: —"A mind well cultivated, and always fresh in thought and feeling —a decision in religious matters, thorough and uncompromising, united with a large toleration of the views of others—characterised him; while he was a lover of all good men, and the blithest of companions in hours of relaxation and social fellowship."

Dr John Alexander Smith. By Professor Duns, D.D.

John Alexander Smith was born in Hope Street, Edinburgh, in June 1818. His father was the late James Smith, a well-known Edinburgh architect. Mr Smith was educated at the High School and the University of Edinburgh. While still a student he became a Fellow of the Royal Physical Society. He graduated in medicine in 1840, was elected a Fellow of the Society of Antiquaries of Scotland in 1847, a Fellow of this Society in 1863, a Fellow of the Royal College of Physicians in 1865, and succeeded the late Dr

Somerville in the Treasurership of the College in 1874. Dr Smith began practice as a physician, and continued in it through life, but his practice was never large. Possessed of means sufficient to enable him to follow his tastes for natural science and archæology, independently of professional income, these pursuits became the leading work of his life. But his professional brethren were never slow to acknowledge his skill in the diagnosis and treatment of disease. Had circumstances made it necessary to devote himself earnestly to his profession, it would hardly have been possible for him to have done half the work he accomplished in the literature of his favourite studies and in behalf of the Societies of which he was a Fellow.

When Dr Smith joined the Royal Physical Society it counted among its working Fellows, Knox, Captain Thomas Brown, Carpenter, Edward Forbes, Greville, Simpson, Goodsir, George Wilson, John Coldstream, Sir J. G. Dalyell, Charles Maclaren, and, later, Robert Chambers, John Fleming, Hugh Miller, and others who have done good work in Scottish Zoology and Geology. Dr Smith succeeded the late Sir Wyville Thomson as its Secretary, an office which he held for more than twenty years. In addition to his work as Secretary, he contributed many papers to the Society, some of much value. More than a hundred notices of birds, many of them new to Scotland, and some new to Britain, were written by him. In these he put on record all peculiarities as to time of visit, plumage, food, &c. In the *Proceedings* of the same Society are between twenty and thirty notices of fishes by him, including remarks on the divergence of some of the specimens from typical varieties in hermaphroditism, and other highly exceptional features. But he did not limit his observation to birds and fishes. Insects, reptiles, and mammals were described with equal interest and skill, while several mineralogical notices indicate his familiarity with this branch. On resigning the office of Secretary he was elected President, and in November 1876 delivered the address at the opening of the 106th Session of the Society. This address contains a complete list of his friend Dr Strethill Wright's numerous original papers on Protozoa and Cœlenterata, and in a somewhat incisive way he states his opinion of Darwinism, and frankly avows his belief in the doctrine of special creation and in the Bible views of the natural history of man.

On the 16th of April 1866 Dr Smith read to this Society a

paper entitled "Description of Calamoichthys, a New Genus of Ganoid Fish from Old Calabar, Western Africa, forming an addition to the Family Polypterini." This paper, which is illustrated by two plates, is published in the *Transactions* for Session 1865–66. It affords an exceedingly good proof of Dr Smith's ability as a naturalist. Several specimens of both sexes were examined. The characteristics are precise, and embrace the minutiæ of every part, while much interesting information is given concerning the habits and food of these forms.

Dr Smith also contributed papers to the *Edinburgh New Philosophical Journal*, the *Annals and Magazine of Natural History*, and the *Journal of Anatomy*.

Dr Smith was Secretary of the Society of Antiquaries from 1852 till the time of his death, with the exception of two triennial periods, 1870–75 and 1875–78, when he held the office of Vice-President. Between 1850 and 1882, he contributed forty-five papers to its *Proceedings*. For many years he acted as joint-editor of the *Proceedings*, first in association with his intimate friend David Laing, and afterwards with Dr Arthur Mitchell. Dr Smith's archæological contributions are all interesting, and many of them valuable additions to the literature of Archæology. They illustrate the wide range of his knowledge. And all who were acquainted with his conscientious habits of study, with his devotion to true scientific method, and with the thoroughness of the investigation brought to bear on the subjects in hand, will not think it too much to affirm that he dealt with each of these subjects as if it only had ever held the chief place in his thought. His papers might be grouped thus,—Prehistoric Remains, Roman Remains, Mediæval Remains, Antiquarian Literature, Rock Sculpturings, and Archæo-Zoology. His contributions to the last group are very able and important. They bear emphatic testimony to Dr Smith's great attainments in Comparative Zoology. With characteristic precision he identifies the bones of mammal, bird, and fish, and skilfully uses the articles found in cave, or grave, or gravel heap in association with them, to serve for deductions touching the industrial conditions of the time, or for supplying a key to the age of the deposits themselves in which they occur. He did much, along with others who survive, to apply the recognised principles of historical

criticism to antiquarian research, to free this branch of knowledge from the reproach of mere "curiosity hunting," and to give to Scotland a school of Archæology as thoroughly in the lines of true science as her school of Geology was held to be. This might be very fully illustrated by a criticism of his special contributions to this branch of study, such as the papers on "Roman Remains found near the Village of Newstead," or his "Notes on Melrose Abbey," or his "Notices of the Ancient Cattle of Scotland," or his "Notice of the Remains of the Reindeer found in Scotland." The last named is a peculiarly able and exhaustive paper. It is crowded with facts, which supply abundant material for trustworthy generalisations as to the climate and the inhabitants of the localities where the remains occurred.

In January 1883, Dr Smith began to suffer from the growth of a tumour in the upper jaw, which in a few weeks assumed a malignant form. But, both in the intense pain of the early stages of the disease, and in the rapid waste of the affected parts in the later ones, it was great satisfaction to his friends to see how calmly and bravely the Christian hope, which had long been his, enabled him to bear his sore affliction. He died on the evening of the 17th of August 1883.

Sir John M'Neill. By Professor Duns, D.D.

The Right Honourable Sir John M'Neill, G.C.B., third son of John M'Neill, Esq. of Colonsay and Oronsay, was born at Oronsay House, Argyllshire, August 12, 1795. He studied at the Universities of St Andrews and Edinburgh. Having graduated in medicine in 1815, he proceeded to India as an army surgeon. Four years later he was attached to the H.E.I.C.'s mission to Persia,—first in a medical and afterwards in a diplomatic capacity. His linguistic attainments, apt business habits, natural shrewdness, literary acquirements, and wide knowledge of Eastern affairs led to his appointment as assistant Envoy at Teheran in 1831. In 1834 he became secretary of the Embassy, and in 1836 he was appointed Envoy Extraordinary and Minister Plenipotentiary at the Court

of the Shah—a position which he held for about six years. Soon after he became Ambassador an anonymous work on the influence of Russia in the East was published, and generally ascribed to Sir John. The work attracted a good deal of attention in England at the time. After his return home, he was appointed in 1845 chairman of the new Board of Supervision for Relief of the Poor in Scotland, an office which he held till 1868. While head of the Board of Supervision he made an extended tour of investigation, by desire of Government, through the Western Highlands, during the period of the famine. The Report which resulted from this tour was published in 1861. Characterised by much ability and great good sense, the Report is full of interest, both for the information it contains and for the remedial measures recommended in it, the chief of which, emigration, has recently been much canvassed in connection with the present social and industrial condition of the Highlands and Islands.

During the Crimean War, Sir John was requested by the Minister of War, Lord Panmure, to proceed along with Colonel Tulloch to the Crimea, and make careful inquiry into the disasters of that campaign in connection with the defective commissariat. On presenting the joint Report in 1855, Sir John received the thanks of Parliament and the distinction of G.C.B. He was sworn a member of the Privy Council in 1857. Though the accuracy of some parts of this Report was called in question by the military commission at home on the same subject, its value was generally admitted. Sir John was a D.C.L. of the University of Oxford, and LL.D. of Edinburgh. He was for some years one of the curators of the University of Edinburgh, and at the time of his death was honorary president of the Edinburgh Literary Institute. He became a Fellow of this Society in 1840, and was a member of many other learned societies, both British and foreign.

His latter years were spent partly at his residence, Burnhead, near Edinburgh, and partly at his villa, Poralto, Cannes, where he died on May 17, 1883.

Sir John was, by one of his marriages, brother-in-law of Professor John Wilson (Christopher North), and by his marriage in 1870 he became brother-in-law of the present Duke of Argyll.

Morrison Watson, M.D., F.R.C.P., F.R.SS. Edin. and Lond. By Professor Alfred H. Young.

Among the losses sustained by the Royal Society during the past year, that occasioned by the sudden death of Dr Morrison Watson was perhaps the saddest.

In the prime of life, and apparently in good health, Dr Watson, when seized with the illness which soon afterwards proved fatal, was engaged in the active duties of the chair of Anatomy in the Owens College at Manchester, to which he had been elected—as the first occupant—eleven years previously. Prior to this, Dr Watson resided in Edinburgh, where he received—at the Queen Street Institution—his early education, and where later he studied medicine in the University, and in course of time took the degree of Doctor of Medicine. For some years he worked in the anatomical rooms of the University as demonstrator with Professor Goodsir, and thereafter with Professor Turner, by the latter of whom he was appointed to the office of principal demonstrator of anatomy. It was during this period that Dr Watson became a Fellow of the Royal Society, whilst more recently he was also elected a Fellow of the Royal Society of London.

By his friends, and he had many, he was regarded with feelings of the highest respect and esteem, and the news of his sudden and early death gave rise to widespread expressions of deep and sympathetic regret. To those who knew him best he was a genial and affectionate friend, and it was only those perhaps who could fully realise his sturdy independence of character, his straightforward nature, and the strength and depth of his friendship.

The loss which the Medical School of Manchester and the Owens College sustains by Professor Watson's death is great. He was appointed to the newly instituted chair of Anatomy, at the period when the amalgamation of the previously separate institutions, the Medical School and the Owens College, was effected, and he at once devoted himself with energy and vigour to the work of creating a School of Anatomy in Manchester. Under his direction the resources for teaching were greatly developed and materially augmented, and the now fairly complete collection in the Anato-

mical Museum of the Owens College was formed under his direct supervision. As a lecturer, Dr Watson was lucid and always practical. Alike by his own work, and by the generous encouragement he readily gave to others, he fostered a spirit for original investigation in the Anatomical School of the Owens College with such success, that it now occupies a prominent position amongst the English schools as one of the few in which, not only is human anatomy efficiently taught, but in which good work is also done in the wider field of animal morphology. It is to be hoped that in this respect the influence of Dr Watson's work will continue to exercise its power for good over the future of the anatomical department of the Medical School at Manchester.

Of Dr Watson's own additions to scientific anatomy, undoubtedly the most complete is the able and comprehensive contribution to the reports of H.M.S. "Challenger" (vol. vii., Zoology)—"On the Anatomy of the Spheniscidæ." An important memoir, "On the Anatomy of the Northern Beluga (*Delphinapterus Leucas*) compared with that of other Whales," of which he was joint author, appeared in the *Transactions of the Royal Society of Edinburgh* (vol. xxix.). A more recent paper by Dr Watson, "On the Female Organs and Placentation of the Raccoon," was communicated to the Royal Society of London (1881). The results of many of his investigations were communicated to the Zoological Society of London,—notably a series of interesting papers, "On the Anatomy of *Hyæna crocuta*" (1877–81); "On the Anatomy of *Chlamydophorous Truncatus*" (1879); "On the Muscular Anatomy of Proteles" (1882); and "On the Anatomy of the Indian Elephant" (1881–83). To the *Journal of Anatomy and Physiology* Dr Watson contributed articles, "On the Mechanism of Perching in Birds" (1869), which constituted part of a graduation thesis for which a gold medal was given; on "The Termination of the Thoracic Duct on the Right Side" (1872); "Notes on a Remarkable Case of Pharyngeal Diverticulum" (1874); on "A Case of Double Aortic Arch" (1877); on "The Homology of the Sexual Organs, illustrated by Comparative Anatomy and Pathology" (1879); on "The Curvatores Coccygis Muscle in Man" (1880); and a series of "Contributions to the Anatomy of the Indian Elephant" (1871–73). Dr Watson's remaining writings include papers on "The Anatomy of

the Elk" (*Journal of the Linnean Society*, vol. xiv.); "Notes on . . . . two Species of Crustacea (*Ann. and Mag. of Nat. History*, 1870), and "Notes on Congenital Absence of the Kidney" (*Edin. Med. Jour.*, 1874).

Rev. Francis Redford, M.A. By Henry Barnes, M.D.

The Rev. Francis Redford, M.A., who died on the 20th of last September, was one of the oldest and most notable clergymen in the diocese of Carlisle. He was born at York in 1813, and at an early period showed remarkable intelligence and aptitude for scientific work. He received his early education in the public schools of the city in which he was born, and afterwards, with the intention of adopting the medical profession, he entered King's College, London, as a medical student. After obtaining some considerable amount of medical training, which was very useful to him in after life, circumstances arose which made it desirable that he should adopt another career, and in 1837 he was sent out by the Church Missionary Society to Trinidad as a catechist. He remained in that country for four years, doing much good work, but owing to failure in health he was compelled to return to this country in 1841.

He then set about studying for the ministry of the English Church, and I am informed he was ordained deacon on June 11, 1843, by Charles James Blomfield, Bishop of London. He held curacies both in Herefordshire and Nottinghamshire, but a love of missionary work and travel induced him to again try the climate of the West Indies, and in 1844 he went out to Jamaica. A breakdown in his health compelled him to return in 1847. Three years later, in 1850, he was appointed to the living of St Paul's, Silloth, and here he continued to reside during the remainder of his life. Here it was that he made those observations on meteorology by which he will chiefly be remembered. At the time of his appointment, the now popular watering-place of Silloth was a desert of sandhills; there was not a single house there, and the part which he took in developing the place and promoting its prosperity is generally recognised. In the place of a sandy desert, there is now an

important seaport, with its docks, pier, regular railway and steamboat communication, and abundant accommodation for visitors. By his meteorological observations, which were regularly published in the Reports of the Registrar-General and in the *Transactions* of the English and Scottish Meteorological Societies, of both of which he was a Fellow, he demonstrated the remarkable fact, that as regards the amount of ozone, signifying an absence of impurity, and in the amount of sunshine, Silloth occupied a very conspicuous position. This demonstration, proved by the careful record of many years' observations, has given Silloth a character which has undoubtedly contributed to its popularity as a health resort. He was the honorary secretary of the Cumberland and Westmoreland Convalescent Institution, from its foundation in 1862 to within a few weeks of his death, and by his energy and painstaking efforts in this capacity, he contributed materially to the success and prosperity of this valuable institution. His knowledge of botany was considerable, and he was an intimate friend of the late Professor Balfour. In the use of the microscope and telescope he was often engaged, and for many years he was a Fellow of the Royal Astronomical Society. The degree of M.A. was conferred upon him in 1860 by the Archbishop of Canterbury, and he was elected a Fellow of this Society in 1865.

Although he was in the daily habit of making scientific observations, I cannot find that he ever contributed any paper to our *Proceedings*. This is much to be regretted, as he had undoubtedly accumulated much valuable material, and the record of his labours in the department of meteorology fill many volumes. These have now come into my possession, and as I do not think a more fitting home could be found for them than the Royal Society of Edinburgh, I have much pleasure in handing them over for permanent preservation. In doing so it may be of service if I give a short account of the contents.

No. 1. A large volume of meteorological observations, taken daily at 9 A.M. and 9 P.M. from March 1854 to December 1868, giving the rainfall, wind, thermometer, hygrometer, and barometer. The ozone observations commence in April 1868. There is an interval of a few years, and the observations begin again in January 1874, and continue to December 1875.

No. 2. A volume containing observations of a similar kind, taken twice daily from January 1876 to January 1877.

No. 3. Twelve volumes containing detailed observations for the following years, viz., 1871, 1872, 1873, 1876, 1877, 1878, 1879, 1880, 1881, 1882, 1883, and 1884. The sunshine observations commence in 1881.

No. 4. A volume containing full observations from October 1876 to March 1877.

No. 5. A volume containing daily notes on thermometry and rainfall for 1869 and 1870, and monthly averages for 1872 and 1873.

No. 6. A volume giving a comparative statement of the rainfall, and readings of barometer and thermometer taken at Lewisham, Kent; Highfield House, Notts; and St Paul's Parsonage, Cumberland, from March 1854 to February 1855.

Together with these volumes there is another MSS. volume illustrative of the meteorological history of this district, which I desire to present to the Society at the same time. It is a volume of observations from January 1838 to May 1842, taken at Carlisle by the late William Elliot, M.D. Edin., and was found among Mr Redford's papers.

Before a Society like this, it would be out of place to refer to the manner in which he discharged his parochial and ministerial duties. Suffice it to say that he had many attached friends, that he found his chief relaxation in scientific studies, and that in a widely scattered agricultural community he could not find much companionship. The development of Silloth did much to give him, in this latter matter, much of what he formerly missed, and few of the scientific visitors left without visiting his observatory.

His health of late years had not been robust, but latterly symptoms of malignant disease of the abdomen set in. His last illness, which was of a painful character, was borne with great fortitude. No one knew better than himself there was only one possible termination, but amid his sufferings he found relief by turning to his scientific pursuits, and by the reflection that during the course of a long and active life he had accumulated observations that would be of value to his fellow-men.

General A. C. Robertson. By T. Stevenson, *P.*R.S.A.

General A. C. Robertson, the eldest son of Lieutenant David Robertson, of Royal Marines, was born at Edinburgh, February 8, 1816, and was educated at the High School and University there. I knew him from boyhood as a born soldier, always ready to fight his battles with other boys under every circumstance of disadvantage, and when he found a difficulty in getting his commission he at once joined other volunteers from this country who took part in fighting against Dom Miguel in Portugal in 1834. Serving under Sir de Lucy Evans, he saw much heavy service with the British Auxiliary Legion in the north of Spain. He was present at the relief of St Sebastian and at the other battles which followed in quick succession during the years 1836 and 1837. At Ametza Robertson was severely wounded by a rock splinter from a round shot. He received for his services in Spain two medals and the cross of the first class of San Fernando. Robertson had risen to be captain in the Legion when, owing to some difference with his commanding officer, he threw up his commission and enlisted in another regiment, and rose again to be captain—a promotion which took place before he was one-and-twenty. In 1837 he obtained a commission in the 34th Regiment, serving with it for three years in Canada, having got his company in 1845.

In 1842–43 he had studied at the Senior Department, Sandhurst—the germ of the present Staff College,—and obtained superior certificates in mathematics and surveying. In April 1846 he exchanged into the 8th (the King's), with which he continued for the remaining twenty-nine years of his regimental service. A few days after the arrival of his regiment at Delhi, Robertson joined it there along with Colonel Baird Smith of the Engineers, and served in the siege till September 11, when the breakdown of his health compelled his being sent to the hills. Colonel Greathead says in his diary:—"Robertson was under fire from seven in the morning until six in the evening;" and again, regarding the mutiny, "The work was very well done, and the King's behaved very steadily under Captain Robertson, who is one of the bravest and coolest men under fire that can be seen." For his services during the mutiny he was

mentioned in despatches, received the brevets of major and lieut.-colonel, a medal and two clasps; and after obtaining his lieutenant-colonelcy in 1865, he commanded the 2nd battalion of the King's—a post which he retained for nine years at Malta and in various home quarters. In 1876 he was nominated to the command of the 13th and 14th Brigade Depôts, retiring finally in 1878. He was gazetted C.B. on 2nd June 1877, and 24th March 1880 was appointed honorary colonel of the 15th Lancashire Rifle Volunteers.

After his retirement he lived chiefly in Edinburgh, and subsequently died at Liverpool, on 2nd December 1884, of a disease of an incurable and most painful nature, borne with singular patience and cheerfulness. Robertson was a man of much thoughtfulness, as well as a zealous soldier, and wrote with great independence on the following subjects:—Infantry; the tactics of the three arms; on the first three parts of the field exercises and evolutions of the army, and on some of the resemblances and differences between them and the corresponding part of the French ordnance, &c.; on the means of applying the principle of stimulating the voluntary exertions of individuals to the improvement of the system of military training; and a variety of other subjects connected with his profession. Besides which, he devoted his leisure for some years to a verse translation of "Jerusalem Delivered." In later years he re-edited the Historical Record of the King's Regiment. Robertson was therefore a man whom we may well be proud to reckon among our Fellows.

Augustus John Darling Cameron. By T. Stevenson, *P.*R.S.A.

Augustus John Darling Cameron, the only son of the late John Cameron of Edinburgh, was born in October 1841, and was educated at the High School and University there. In 1860 he began an apprenticeship as a civil engineer with the late Mr John Paterson. He was subsequently in the employment of Messrs Foreman and M'Call, Glasgow, and Messrs Wylie & Peddie, and was engaged on railway and other works. He held an appointment of engineer in

the India Office for several years, after which he was engineer to the South London Tramways Company, and under his superintendence the various lines of that company were constructed. He died on 27th November 1884. He was elected a member of the Institution of Civil Engineers in 1880, and a Fellow of this Society on 6th June 1881.

John M'Nair. By T. Stevenson, *P*.R.S.A.

John M'Nair was for nearly twenty years a Fellow of the Royal Society of Edinburgh. He took a lively interest in physical subjects, but owing to his advanced age, and the latterly feeble state of his health, was prevented from attending our meetings very regularly. He was born at Belvidere, near Glasgow, in 1801, and died at Edinburgh in his eighty-fifth year.

William Lindsay Alexander. By Professor Flint.

William Lindsay Alexander was born at Leith on 24th August 1808. He was educated at the High School of his native town and in the Universities of Edinburgh and St Andrews. He distinguished himself in all his classes, but especially in those of Latin, Greek, Logic, and Moral Philosophy. While at St Andrews his earlier religious impressions were much deepened by intercourse with a pious fellow-student, and through the inspiring influence of Dr Chalmers. Although he began preaching when still a student of Arts, it was not until 1832, five years after he had left college, that he made choice of the Christian ministry as his profession. Teaching, literature, law, medicine, all presented themselves to him with competing claims. During the greater portion of this period of indecision and unsettlement he was occupied as classical tutor in the Congregational Academy at Blackburn. Passing through Liverpool in 1832, he was persuaded to occupy for a Sunday the pulpit of Newington Street Independent Chapel, then vacant. The result was that he remained in charge of the congregation for a

year and a half, putting his qualifications for the ministry to a thorough test, and gradually coming to feel that he had found his true vocation. He afterwards went to Germany, and studied theology for a short time in Halle and Leipsic.

On 5th February 1835 he was ordained to the ministry, and became the pastor of the congregation meeting in what was then called "North College Street Chapel," Edinburgh. The connection then formed lasted somewhat over forty-two years. Mr Alexander at once gained, and to the last retained, great popularity as a preacher. In stimulating and guiding the Christian energies of his congregation he was also eminently successful. Gradually he attained in his own denomination an influence with which that of no one else in Scotland could be compared, while his services to the common Christian cause, his genuine catholicity of spirit, and his candour and courtesy even as a controversialist, made his name honoured in all denominations. His scholarship, literary talents, and theological acquirements became attested by writings which spread his reputation far beyond the limits of Britain. Notwithstanding a certain shyness and reserve of manner, his amiability and affectionateness of nature attracted to him numerous warmly attached friends. Among the events and dates of his life during his ministry in Edinburgh, these may be specified,—his marriage in August 1837; his delivery of the Congregational Lecture at London in 1840; his editorship of the *Congregational Magazine* from 1836 to 1840; his receiving of the degree of Doctor of Divinity from the University of St Andrews in 1845; his candidature for the chair of Moral Philosophy in the University of Edinburgh in 1852; his appointment to the Professorship of Theology in the Congregational College in 1854; his removal with his people from Argyle Square to Queen Street Hall in 1855, and thence to the new church (St Augustine Church) on George the Fourth Bridge in 1861; his election as examiner in philosophy at St Andrews University in 1861; his visit to Palestine in 1869; his selection as a member of the Old Testament Revision Company in 1870; his appointment by the Council of Edinburgh University assessor to the University Court in 1871, and reappointment in 1875; and "the greatest sorrow of his life," the death of Mrs Alexander in the last-named year. He published *The Connection and Harmony of the Old and New*

Testaments in 1841; *Anglo-Catholicism not Apostolic* in 1843; *Switzerland and the Swiss Churches* in 1846; *Christ and Christianity* in 1854; *The Life and Correspondence of Ralph Wardlaw, D.D.*, in 1856; *Christian Thought and Work* in 1862; and *St Paul at Athens* in 1865. He contributed to the eighth edition of the *Encyclopædia Britannica* three elaborate treatises—*Moral Philosophy* (vol. xv., 1858), *Holy Scriptures* (vol. xix., 1859), and *Theology* (vol. xxi., 1860). From 1861 to 1869 he superintended the publication of the third edition of Kitto's *Cyclopædia of Biblical Literature*, and supplied a very large number of the articles which appeared in it. He was also the author of at least forty sermons, lectures, or pamphlets, published separately, and a frequent contributor to Reviews and Magazines.

Dr Alexander resigned his ministerial charge on 6th June 1877, and was in the same year appointed Principal of the Theological Hall, while retaining his professorship. From a sense of growing infirmity, these latter offices also he resigned in 1881. In 1884 he received the degree of LL.D. from the University of Edinburgh, on the occasion of its Tercentenary. Amidst deep and wide regret, on the 20th December of that year, he died at Pinkieburn, leaving behind him many a good work to perpetuate and endear his memory, and the example of an unsullied and beneficent, faithful, and consistent life.

Dr Alexander was elected a Fellow of the Royal Society on 29th April 1867, one of the vice-presidents on 24th November 1873, and was re-elected a vice-president on 22nd November 1880. He wrote a number of obituary notices of eminent members, and delivered the Opening Address of the Session 1876–77.

Having indicated the chief facts of his life, let us now glance at the chief aspects of his character.

It was impossible to think of him otherwise than as a remarkably accomplished scholar. He was throughout his life an earnest student. No one knew better that knowledge is not the supreme object of human life, yet no one could realise more how precious and pleasant it is, and how closely connected with what is best. Hence a large portion of his daily life was given to its acquisition, and not selfishly, but in the belief that through self-improvement he would the more profit others. He had a keen interest in most

kinds of science and learning; had cultivated with special care various departments of theology and philosophy; was intimately conversant with Biblical studies; was widely read in classical and modern literature; and was an excellent Latinist, Hellenist, and Hebraist. His mastery over the classical tongues as poetical media is amply attested by the collection of Greek and Latin verses which he printed privately and dedicated to his brethren of the Hellenic Society. His attainments as a Hebraist he had many opportunities of applying. He delighted in good English poetry, and was the author of a considerable number of very meritorious English hymns.

The amount of literary work which Dr Alexander performed must be regarded as marvellous, when it is considered with what diligence and success he discharged the many duties of his ministry and professorship. Yet none of his writings bear the marks of hasty and inadequate preparation. The briefest articles from his pen in *Kitto* are carefully executed. That he achieved so much as an author was doubtless due largely to strength and readiness of memory, clearness and vigour of thought, and facility of accurate and appropriate expression, but it was due as largely to his self-denying and methodical employment of his time. In this respect few can ever have surpassed him. At the commencement of his ministry he formed the resolution never to have what people called "a spare hour," but to lay out his work every day so as to know each hour exactly what to do; and to this resolution he steadily adhered to the close of his life.

From the time that he listened to Chalmers in St Andrews, philosophy, and especially moral philosophy, had strong attractions for him. How high was his estimate of philosophy and his ideal of the philosopher may be best learned from his eloquent and elaborate address to the Philosophical Society of the University of Edinburgh on his election as president in 1875. The most adequate measure of his philosophical ability is, however, the treatise on Moral Philosophy in the *Encyclopædia Britannica.* It presents us with a clearly defined, well-arranged, skilfully rounded system of ethical science. If not exhibiting much originality, it displays extensive learning and careful and independent reflection. It fully entitles its author to an honourable place among Scottish moral philosophers.

Dr Alexander devoted, of course, far more of his time and energy to theology than to philosophy. And it may safely be said that among his contemporaries in Scotland there was no more generally accomplished a theologian, although he was doubtless surpassed by several of them in particular qualities. He attained a high reach of excellence alike in exegetical, historical, apologetic, systematic, and practical theology. In all these departments he produced excellent works. On any special theological problem he could at once bring to bear ample knowledge and a rich combination of strong and disciplined mental powers. It cannot, indeed, be said that in theology, any more than in philosophy, he opened up or even followed out new paths of research. His mind rapidly reached maturity, and the religious opinions which he formed in youth remained almost unmodified to the close of his life. Within the limits of so-called orthodoxy, however, his intellect acted with admirable freedom and effectiveness. He held firmly to the Calvinistic system of doctrine and to the Congregational scheme of Church government, but with conspicuous independence of judgment. On various theological and ecclesiastical questions he differed decidedly even from those with whom he was in the main most in agreement. This appears very clearly in his Memoirs of Dr Wardlaw, in which criticism mingles so largely with admiration.

Dr Alexander took a somewhat prominent part in most of the religious controversies of his time. On the platform and through the press he felt called to set forth his views on Episcopacy, Anglo-Catholicism, Romanism, Church Establishments, and the like; he was drawn into the Voluntary, Spiritual Independence, Morrisonian, and some minor conflicts. It will be admitted by all, however, that while he always fought with vigour, he also always fought without bitterness or unfairness, and obviously from no love of strife itself or desire for personal or party victory, but from a sense of what he felt due to truth and the public good. As was to have been expected in the case of one whose mind was so justly balanced and so catholic in its sympathies, the more experience he acquired of religious controversy the more disappointed he became with its results, and in his later life he kept aloof from it.

As a pulpit orator he was of remarkable merit. Never aiming at

popularity, making it a rule not to prepare so-called "great sermons," constantly dealing largely in the exposition of Scripture and the setting forth of doctrine, habitually keeping feeling under restraint, and very sparing of gesture and action, he was yet not only a most instructive, but a most interesting and impressive preacher. His tall stature and noble presence, his admirable delivery, his style refined yet strong, somewhat elaborate but also singularly lucid, the amount of knowledge which he communicated, and of light which he cast on Scripture, his intellectual force, his vivid and deep realisation of spiritual things, and the judiciousness and pointedness of his practical applications of truth, combined to make him a great and beneficent power in the pulpit.

As a man his character commanded universal respect. None doubted his piety and benevolence any more than his learning or ability. He gained many friends, and alienated none. He was especially at home in scholarly and intellectual society, and where at home he was a charming companion, unaffected and genial, with a keen sense of humour and hearty love of mirth, and with an inexhaustible store of anecdotes, which he delighted to tell, and which he told exceedingly well.

His private and domestic life has been gracefully delineated by Miss E. T. M'Laren, in reminiscences originally printed for private circulation, but now incorporated in the Life of Dr Alexander by the Rev. Mr Ross. Mr Ross himself, as an old student of Dr Alexander, has given us an account of his character and work as a professor, from which it is apparent that he was nowhere more admirable and successful than in his class-room.

The memory of Dr William Lindsay Alexander will be long affectionately cherished and highly honoured.

William Chambers, LL.D. By David Patrick, M.A.

William Chambers, one of the founders of the publishing firm of William and Robert Chambers, and in his later years distinguished for his public spirit as a citizen, was born at Peebles on the 16th April 1800, his father being a cotton manufacturer there. William,

the eldest son, received a fair elementary education at Peebles; but as, owing to the father's misfortune, his school days terminated with his thirteenth year, his education for life-work was mainly due to the habit, very early acquired and long maintained, of miscellaneous and extensive reading. The household migrated to Edinburgh in 1813, and next year William was apprenticed to a bookseller. Immediately after his five years' apprenticeship was out, he began business in a very humble way for himself, and soon added to bookselling the occupation of a jobbing-printer. He by-and-by ventured to print and publish small pamphlets written or compiled by himself or his younger brother Robert, also established as a bookseller in Edinburgh. Through much industry, care, and economy they each prospered, and by 1825 what they called their "dark ages" had closed for ever. In 1825 William produced *The Book of Scotland*, and had a share with his brother in compiling a gazetteer; but it was the *Chambers's Edinburgh Journal*, projected and started by William in 1832, that brought great and permanent success to both brothers. From the beginning, Robert was the most important contributor to the Journal; and a month or two after it was fairly afloat, the two brothers united their resources and enterprise under the now so well known firm of W. & R. Chambers. Other important publications of the firm were the *Information for the People* (1833); the *Educational Course*, comprising an extensive series of text-books in history, science, and literature; the *Miscellany*; the *Encyclopædia* (10 vols., 1859–1868); the *Cyclopædia of Literature*, and numerous works by Robert Chambers. In 1859 the elder brother presented his native town with a public library, reading room, and museum. In 1865 he was chosen Lord Provost of the city of Edinburgh, and in this capacity carried out a very important measure for the improvement of the city, by substituting healthy houses and airy streets for lanes and closes of pestiferous hovels. He was the chief promoter of a partial restoration of the ancient Kirk of St Giles; and in 1878–83 he carried out a more complete and extensive restoration at his own expense. He found time to write an admirable county history of Peeblesshire (1864), a short history of France, sketches of tours in Holland, America, Italy, and France, various other short works, and a memoir (with autobiographical reminiscences) of his brother Robert,

who was a member of this Society from 1840 till his death 1871, and is known as the author of numerous works, including the *Traditions of Edinburgh*, the *History of the Rebellion*, the *Ancient Sea Margins*, and the (anonymously published) *Vestiges of Creation.* William became a fellow of the Royal Society of Edinburgh in 1860; and he was made LL.D. by Edinburgh University in 1872. For his services to the cause of public instruction, and his work as a civic ruler, an offer of knighthood, which he declined, was made in 1881. But when, a fortnight before his death, a baronetcy was offered him, he accepted it. It had not, however, been formally conferred or gazetted, when on the 20th May 1883 he died, at the advanced age of more than 83 years, and but a few days before the ceremony that marked the completion of the last work of his life, the reopening of the restored church of St Giles. He was buried in the place of his birth, and many manifestations were made of esteem and gratitude for his services to English-speaking men and women, by providing works of instruction and entertainment accessible at small cost to all of whatever rank or condition. His life of unremitting labour and his remarkable business abilities brought to him wealth, honour, and influence, and these were by him faithfully turned to account for the general good.

David Stevenson.

David Stevenson, the third son of the late Mr Robert Stevenson, the well-known civil engineer, was born at Edinburgh on the 11th January 1815. Educated at the High School and University of Edinburgh, he elected from the first to follow his father's profession. Before entering on his apprenticeship he was for some time in the workshops of one of the best millwrights of the day, where he acquired manipulative skill and the proper methods of working in different materials,—a course he always advocated for those who intended to follow the profession of civil engineering. After serving a regular pupilage under his father, during which period he had ample opportunities of attending various engineering works in progress, he was for some time engaged with Mr Mackenzie on

railway works, particularly the Edgehill Tunnel and the Liverpool and Manchester Railway. He gave a description of the "Liverpool and Manchester Railway" to the Royal Scottish Society of Arts in February 1835, and was awarded their medal for his exposition. This paper was followed, in March 1836, by "Remarks on the Dublin and Kingston Railway." In 1835 Brunel asked Mr Stevenson to join his staff at the Thames Tunnel works,—an offer which he could not accept, as he had been appointed to superintend the construction of other works.

During the summer of the year 1837, Mr Stevenson made a tour in Canada and the United States, and the result of his inspection of the engineering works of these countries was given in a volume, published in 1838, entitled "Sketch of the Civil Engineering of North America," which was republished with additions in 1859, as one of Weale's series of engineering works. The engineering practice therein described was peculiarly applicable to newly developed countries, where timber forms the chief material employed. The views expressed and the drawings given in this book, with reference to the fine lines and speed of American river and lake steamers, were received in this country by shipbuilders with distrust, if not ridicule; but the bluff bows of British sea-going and river steamers, and short-stroke engines, all below deck, gave way to finer lines and engines of high power and long stroke; and soon thereafter steamships were plying on the Clyde at higher speed than in America. He had models made in New York of two of the fastest steamers,—a sea and river boat,—which were submitted to the Admiralty after his book was published; but as the "lines" had been there given, the authorities did not care for the models, and they ultimately went to the Russian Government. In this book Mr Stevenson also pointed out the true conditions under which waves are formed, which his subsequent experience as a marine engineer amply corroborated.

Mr Stevenson entered into partnership with his father and brother Alan in 1838. As his father was then nearly 67 years of age, and his brother was wholly engaged with the arduous works at Skerryvore Lighthouse, the entire management of the business fell upon Mr Stevenson, and very soon his advice was much sought in reference to the improvement of rivers and harbours, and the con-

struction of docks and other marine works. There are, indeed, very few rivers and harbours in Scotland with which he was not in some way professionally connected. He was also called upon to report or to execute works for the improvement of the rivers Dee, Lune, Ribble, and Wear in England; the Erne and Foyle in Ireland; and the Forth, Tay, Ness, Nith, and Clyde in Scotland. He designed and executed the works of restoration and enlargement of the Fossdyke in Lincolnshire,—the oldest artificial navigation in Britain,—the widening and deepening being accomplished without stopping the traffic. His advice was also taken on many important questions relating to salmon fishings in rivers and estuaries, and in his Report of August 1842 on the Dornoch Fishings, he defined the different compartments of rivers, according to their physical characteristics, as "sea proper," "tidal," and "river proper." The views expressed in this report were subsequently treated at greater length in "Remarks on the Improvement of Tidal Rivers," communicated to this Society on 17th March 1845, which paper was afterwards published in a separate form by Mr Weale. In this paper he stated his views with regard to the special works which should be undertaken for the improvement of the different compartments, and he showed conclusively that, if tidal propagation were accelerated, difference of level or height of head will be lessened and the velocity of the tide currents decreased; and the notion that, by deepening a river and removing obstructions, the water in it may be caused to rise higher and to endanger property on its banks, was without foundation. All his subsequent experience went to prove that the views expressed more than forty years ago were sound. He was also the first to enunciate the true theory of the origin of bars at the mouths of rivers, and to point out the works necessary for removing them and preventing their reformation. The necessity of having accurate data before designing works for the improvement of rivers, estuaries, and harbours led to his writing a treatise on the "Application of Marine Surveying and Hydrometry to the Practice of Civil Engineering." His known standing as a marine engineer led his old friend, the late Mr Adam Black, to invite him to write the article "Inland Navigation" for the last edition of the *Encyclopædia Britannica*. This article was republished in 1858 as a separate treatise, entitled "Canal and

River Engineering," and it is now in its third edition. In this book he has given the results of his practice in the treatment of rivers, and it will long remain the standard work in this difficult branch of engineering. In 1877, at the request of the authorities, he delivered a course of four lectures on "Canal and River Engineering" to the students of the School of Military Engineering, Chatham.

During the height of the railway mania, when speculators were overwhelming the Admiralty and Woods and Forests with schemes too numerous to be dealt with by the officials, Mr Stevenson was appointed to hold Courts of Inquiry, under the "Preliminary Inquiries Act," into the merits of a large number of railway and harbour projects and water-supply schemes, and in all cases save one his views were given effect to by the Authorities and Committees of Parliament. This exception was the proposed crossing of the Clyde by the Caledonian Railway. He reported that the railway might be allowed to cross the Clyde above Stockwell Bridge, but the Admiralty refused their sanction. A crossing, however, was subsequently applied for, and has been made.

In 1853 Mr Stevenson succeeded his brother Alan as engineer to the Northern Lighthouse Board, and, along with his brother Thomas, who was at a subsequent date conjoined with him in the engineership, he designed and executed no fewer than twenty-eight beacons and thirty lighthouses, three of which—on North Unst, Dhu Heartach, and the Chickens Rocks—were works of great difficulty, requiring the exercise of great engineering skill. The advice of the firm was also taken by the Governments of India, Newfoundland, New Zealand, and Japan on lighthouse matters, and schemes for the lighting of the whole coasts of the two last countries were matured, and are now being carried out. In connection with the lighting of the coasts of Japan, where earthquakes are of frequent occurrence, Mr Stevenson devised the aseismatic arrangement, to mitigate the effect of shocks on the somewhat delicate apparatus used in lighthouses, and was awarded the Makdougall-Brisbane Medal of the Royal Scottish Society of Arts for his invention. Mr Stevenson took a great interest in the introduction of paraffin as an illuminant for lighthouses, instead of the more expensive colza oil. After experiments conducted at Edinburgh with the Doty burners,

and actual trial during a month at Girdleness Lighthouse, a report was made to the Northern Lighthouse Board, embodying the following results:—That paraffin, as now manufactured, with a high specific gravity and flashing point, is safe; that the flame is of great purity and intensity, and easily maintained; the lamp glasses and lamps used for colza are equally suitable for paraffin; and that the initial power of the lights on the Scottish coasts would be exalted, and this, too, at an annual saving of £3500. The relative merits of colza oil and paraffin were thus settled, and most British and many European, foreign, and colonial lighthouses now consume paraffin, with an increased luminous intensity, and decreased cost of maintenance.

Mr Stevenson frequently appeared before Parliamentary Committees, and also gave evidence before Special Committees and Royal Commissions on Harbours, River Improvements, Harbours of Refuge and Lighthouses. For these Committees and Commissions he always prepared with scrupulous care; and he was a most conscientious witness, never entering the box without being satisfied as to the soundness of the cause he was supporting; and in no case did a Committee ever throw out a Bill, the Parliamentary plans and estimates of which he had prepared. In his native city Mr Stevenson was greatly respected, and his advice on many important matters connected with city improvements was sought and highly valued. His views on the city improvement scheme, as conveyed to Lord Provost Chambers and the late Mr David Cousin, city architect, along with his letters which appeared in the *Scotsman* at the time, greatly facilitated the initiation of this great sanitary improvement, now nearing its completion. Among other local works, his firm designed and carried out the Edinburgh and Leith and North Leith sewerage schemes, and the widening of the North Bridge.

Mr Stevenson was consulting engineer to the Convention of Royal Burghs of Scotland and to the Highland and Agricultural Society. In the affairs of the latter he took a lively interest, especially in the trials of improved agricultural implements; and he contributed several papers and reports to their *Transactions*, notably one on the reclamation of land, and one on the relative merits of different systems of steam ploughing. The paper on "Reclamation

and Protection of Agricultural Land" was republished as a separate treatise in 1874.

Mr Stevenson's books have taken a permanent place in engineering literature. Amid the exacting calls of his profession he found time to write many papers on engineering and cognate subjects, in addition to the books already noticed. He also wrote several articles for the last and the present editions of the *Encyclopædia Britannica*, among which may be noticed "Canal," "Cofferdam," "Diving," and "Dredging." He was also the author of *Our Lighthouses*, being two articles written for his old friend, Dr Norman Macleod, while editor of *Good Words*, subsequently republished by Messrs Black, and of the *Life of Robert Stevenson*, published in 1878.

Mr Stevenson was twice elected President of the Royal Scottish Society of Arts, and for his Presidential Address of 1869 he chose as his subject "Altered Relations of British and Foreign Industries and Manufactures," in which he strongly urged the propriety of artisans improving their manipulative skill and their knowledge and management of the materials with which they had to deal, if they did not wish to be distanced in the race by foreign competition. He was elected a Fellow of this Society in 1844, and subsequently acted as a member of Council and one of its Vice-Presidents. In addition to the "Remarks on the Improvement of Tidal Rivers" already noticed, and several obituary notices of professional brethren, he contributed to our *Proceedings* "Notices of the Works designed by Sir C. A. Hartley for the Improvement of the Danube," "Notices of the Ravages of the *Limnoria Terebrans* on Creosoted Timber, and on Greenheart Timber." "Notice of Two Earthquakes on the West Coast of Scotland," and "Notice of Striated Rock Surfaces on North Berwick Law." He was elected a Member of the Institution of Civil Engineers in 1844, and acted as a member of its Council from 1877 till 1883, when he retired on account of ill-health; he was also a member of the Société des Ingénieurs Civils, Paris, and of other learned Societies.

Mr Stevenson took a warm interest in the better endowment of the Chairs of the University of Edinburgh, and was mainly instrumental in founding the Glover Divinity Fellowship. He was a great lover of art in all its branches, and had formed a somewhat

valuable collection of etchings and engravings, begun when a boy at the High School, his school companion in print-hunting being a life-long friend, Sir Theodore Martin. He was a man of sound judgment, unswerving rectitude, utterly devoid of ostentation, kind, open, and easily accessible. He was practically laid aside from work for about two years, and he died at North Berwick, on the 17th July 1886, in the seventy-second year of his age.

Bishop Cotterill. By Dr Cazenove.

Henry Cotterill was born in the year 1812, on the 6th day of January, a day ecclesiastically known as the Feast of the Epiphany, and in popular parlance as Twelfth Night. He was the son of the Rev. Joseph Cotterill, rector of Blakeney, in the county of Norfolk. Mr Cotterill had been educated at Cambridge, and had taken a high degree, coming out as Seventh Wrangler. The grandfather of the future Bishop had also been a Cantabrigian, and was known in Sheffield as a man of culture. He was the author of a book of family prayers. All three were successively members of the same College. All three could say, with the poet Wordsworth,

> "The Evangelist St John my patron was,"

and all could, with him, avow a fondness for mathematical as well as for classical studies.

In classics Henry Cotterill found a preceptress in his mother. This lady, known before her marriage as Miss Anne Boak, was very clever and clear-headed. She was a great friend of the celebrated Hannah More, and to some extent assisted that lady in the preparation of a tale, which was widely read at the time of its publication, *Cœlebs in Search of a Wife*. Miss Boak contributed some chapters which contained descriptions of scenery. She had also a considerable acquaintance with mathematics, and was generally a learned woman.

Not only did she superintend the education of her three daughters, but she instructed her two sons, Henry and George Cotterill, in Latin, until they were fifteen years of age.

Here, it will be said, is a clear case of heredity. I am far from

wishing to dispute the point; but we must not, I think, neglect another element of the case. A distinguished scholar and mathematician, who had been well acquainted with a school which was attended by boys from homes of very varied station and character, told me that, as a rule, the difference was immense between the progress of the pupils who went back daily, or at set periods, to homes where they heard nothing connected with their studies, and those who, when not at school, lived in the houses of relatives or of friends, which were the abodes of educated people. Extraordinary genius will no doubt triumph over these and many other obstacles. But to the youthful student the lack of sympathy at home is a formidable disadvantage. Oliver Wendell Holmes implies, I think, that it has been found so in the United States of America, as well as in Europe. And the inquirers into the influence of heredity ought, it seems to me, to take some pains in examining into this phase of the question, and ask whether this and that man of mark did or did not in youth enjoy the benefit of an atmosphere of culture.

Whatever be the decision, it is at least clear that Henry Cotterill was in this respect exceptionally favoured. He began to receive lessons in Latin when he was five years old; and at the age of fifteen he and his brother George were, as a favour, accepted as private pupils at Cambridge, by Mr Scholefield, subsequently so celebrated as the Regius Professor of Greek in the University of Cambridge. From this admirable teacher Henry Cotterill learnt his first lessons in Greek, and to his latest day he always expressed his deep gratitude to his tutor, and his keen sense of the good fortune he had enjoyed in having had such a preceptor. Nor was he less favoured in mathematics. A very eminent mathematician, William Hopkins (father of a philanthropic lady, Miss Ellice Hopkins, subsequently a great friend of the Bishop's), took charge of his studies in *Mathesis*.

At a later date, a brilliant Cambridge scholar described, in beautiful Greek Iambics, how Jupiter, being angry with mortals, commissioned Vulcan to invent a new plague, and how from this command came forth the penal infliction of mathematical study. Such was not, as has already been observed, the sentiment cherished towards the science of lines and of numbers by this youthful

classic. Contrariwise, his proficiency in this new field of learning seems to have even surpassed that which he achieved in ancient languages. And during vacations he was sure of living in an atmosphere where his studies would receive abundance of encouragement and sympathy. His sisters were ladies of cultivated taste, the youngest especially being a good Latin scholar.

At Cambridge he found friends among contemporaries, and also some among seniors, who, in different ways, influenced his career. Among the seniors, besides the tutors already named, he was acquainted with Professor Sedgwick, with Professor Airy (afterwards the Astronomer Royal), and the aged Mr Simeon. Among those of his own standing may be named Mr Dickinson, afterwards representative in Parliament for Somersetshire; and a member of a highly gifted family, a Merivale, brother of a distinguished Oxonian, W. Herman Merivale, and of equally eminent Cantabrigians, such as the present Dean of Ely. But specially intimate was he with a son of the Right Hon. Henry Goulburn, sometime Chancellor of the Exchequer. In both classics and mathematics these two, Goulburn and Cotterill, were keen, but most friendly rivals. We cannot doubt but that from such guides and friends, and from several more like them, Henry Cotterill must have learned a great deal, though the intercourse cannot possibly have been a merely one-sided bargain. Some of them, as Merivale and Goulburn, were not permitted to display all their powers, having died at a comparatively early age.

In 1832, a year known in British history as that of the passing of the Reform Bill, Henry Cotterill was elected a scholar of St John's College, Cambridge. In that same year he won the University Bell Scholarship, one of the blue ribands of undergraduate life, as an evidence of proficiency in classics; though its glories are perhaps slightly limited by its being confined to the sons of clergymen.

In 1835 he took his Bachelor's degree, his work at Cambridge culminating in the extraordinary success of his being Senior Wrangler, First Smith's Prizeman, and ninth in the First Class of the Classical Tripos, which was headed by the Second Wrangler, his young friend Henry Goulburn. This achievement probably justifies the remark of the late Principal of the University of Edinburgh, Sir Alexander Grant, that Henry Cotterill left Cambridge bearing a greater weight of honours than any other student had gained.

Among the few rivals who have nearly approximated, may be named Charles Webb Le Bas, afterwards Principal of Haileybury College, and the Hon. William Cavendish, now Duke of Devonshire. Almost as a matter of course, Mr Cotterill was, soon after taking his degree, elected a Fellow of St John's.

The present imperfect sketch is mainly concerned with the career of its subject in relation to those gifts which would be felt to constitute his claim to become a Fellow of this Royal Society. But it is impossible to pass by in silence certain elements of the case which exercised a very real, though in part an indirect, influence upon his mental development.

At this juncture he might have laid out for himself a course of life which would necessitate his remaining a man of study rather than of action; or he might have chosen a profession, such as that of the bar, in which it would be possible, while relegating to a secondary position his Cambridge studies, to make use of the mental training thus acquired. His University had abundant examples to encourage him in either of these directions,—brilliant honour-men who had become famous as scholars or men of science, and others who had been conspicuous as barristers and as judges.

But the tone of another profession ruled in the home in which he had been brought up, even more strongly than the love of culture for its own sake, or for the prizes which might thereby be won. Entirely by his own choice,—though no doubt it was a choice greatly moulded by the influences around him,—Henry Cotterill resolved to take Holy Orders. But this was not all. His father had begun to cherish, in his maturer years, a keen sympathy with missionary work. It came seemingly too late in life for him to change his sphere of labour. But he had expressed a hope that his sons, even if they did not feel called to devote their lives to direct missionary work, might at least for a time occupy positions in which they could support and assist missionary efforts. The father's wish was never forgotten by the son, who always regarded it as a grave mistake to send out to our Colonies only clergy of powers below the average.

Henry Cotterill was ordained deacon in 1835, and priest in the year following. In this latter year a presentation to one of the East India Company's chaplaincies was placed in the gift of the

University of Cambridge. Much to the surprise of many friends and of still more numerous bystanders, Mr Cotterill accepted the nomination. That such a step was not calculated to lead to preferment at home was obvious enough; but the father entirely sanctioned the proceeding; and the son never, I believe, for a moment regretted it.

The appointment took him to the Madras Presidency. Before leaving England he vacated his fellowship by marriage with Anna, daughter of John Panther, Esq. of Bellevue, in Jamaica. By this honoured and much-respected lady, who lives to mourn his loss, he became the father of four sons and two daughters, of whom all, but one daughter, have survived him.

At Madras he saw much of missionary work among the heathen, and acquired an interest in it which never left him. But the climate did not suit him, and at the end of eight years' residence his health was seriously affected. In 1847 he returned to England, and accepted the position of Vice-Principal of Brighton College, a newly founded institution. In 1851 he succeeded to the Principalship, rendered vacant by the death of its previous occupant, Dr Maclean.

Although this College prospered greatly under his care, there were those among his friends who felt that it was not a sphere that afforded full scope for his varied powers. One of these, a layman, a member of a family deeply interested in missions, walking in London, is said to have noticed an advertising board headed by the words "Wasted Steam." The title suggested to this gentleman's mind instances of such loss among men whom he had known, and it struck him that Henry Cotterill was at that moment an example of such waste. "By the way," so his thoughts ran onward, "Archbishop Sumner told me that the See of Grahamstown had become vacant by the decease of Bishop Armstrong, and that a successor was being sought for it. Mr Cotterill would be the very man for the place." Fired by this idea, he at once proceeded to Lambeth, and the result was that on November 23rd, 1856, Henry Cotterill was consecrated and then duly gazetted as Lord Bishop of Grahamstown.

The Bishop of Capetown, Dr Gray, seems at first to have feared that he and his new colleague might not work harmoniously. An

unlooked-for event brought them into thorough concord with each other. In the year 1836, that is to say the year after that of Mr Cotterill's Senior Wranglership, the place of Second Wrangler had been won by a member of St John's, who was thereupon elected Fellow of that College. His name was Colenso; and after having had experience, both in tuition as a mathematical master at Harrow, and also in pastoral work as a country vicar, he had been selected for the office of Bishop of Natal. Dr Colenso published works certainly of a startling character; and his former brother-fellow, the Bishop of Grahamstown, in November 1863, formed part of an Episcopal Court which condemned the teaching of Bishop Colenso as heretical. This sentence was not confirmed by the State, inasmuch as the Privy Council declined to recognise the legal validity of the letters-patent granted to Bishop Gray, and of his claims to act as Metropolitan. The opinion, however, delivered by Bishop Cotterill was allowed on all sides to be one of remarkable clearness and ability.

In 1867 the Archbishop of Canterbury, Dr Longley, held a gathering at Lambeth of the Bishops of the Anglican Communion; and a similar one took place in 1878, under the presidency of Archbishop Tait. On both occasions Bishop Cotterill was chosen to act, in company with an English prelate, as a general secretary; and also as sole secretary to a committee appointed to consider the constitution of the Colonial daughters of the Anglican Church, and their relations to the mother Church of England. In these situations he enjoyed an opportunity of showing to those around him his capacity for business, his judicial temper, and his largeness of view. These qualities made a special impression upon some of the Bishops who had come from Scotland and from America.

The acquaintance with his powers thus obtained greatly influenced the clerical and lay electors of the Episcopal Church in the diocese of Edinburgh, when in 1871 they were seeking for a coadjutor to Bishop Terrot. Other candidates withdrew, and on April 26th the Bishop of Grahamstown was elected by the vote of both the clerical and lay chambers, *nemine contradicente*, to the office of coadjutor Bishop. Bishop Terrot only survived this event by about eleven months; and as Dr Cotterill had been chosen *cum jure successionis*, he became full Bishop in April 1872.

With the details of his rule we are not here concerned: and it must suffice to say, that he proved himself to be active, energetic, tolerant, and accessible. If, as seems possible, these qualities were more quickly recognised by the clergy than by the laity of his communion, this may have arisen from the fact that the former, being brought into more immediate contact, saw most of him, and that his good gifts were of a character that required such intercourse to bring them out in all their fulness.

He was greatly interested in the proceedings of this Royal Society of Edinburgh, of which he was elected a Fellow soon after his arrival in Scotland. He was chosen a member of the Council, and subsequently one of the Vice-Presidents. If he did not contribute any papers, such as the valuable ones supplied by his predecessor, Bishop Terrot, it must be remembered that the clerical duties of the occupant of his post had been largely increased. Most especially during eight years (1871–1879) his mind was occupied with the erection and organisation of the Cathedral, which sprung from the munificent bequest of Barbara and Mary Walker. In this complicated task he was held to have been eminently successful. It is perhaps permissible to remark that, in his association with the *savans* of this Society and of the University he expressed himself as greatly gratified with the large amount of ability among the votaries of physical science, which was ranged upon the side of belief, in that great contest with unbelief, which Goethe in well-known words has declared to be "the proper, peculiar, and deepest theme of universal and human history."

It remains to say something concerning the indirect and the direct influence of his academical studies upon his professional life. Indirectly it taught him, as it has taught so many academical students, not to be satisfied with mere surface work in any department of study. Two illustrations, out of several that might be adduced, will serve to illustrate my meaning.

At Madras, during the tenure of his East Indian chaplaincy, he was brought into controversy with some of our Roman Catholic fellow-Christians. Not content to take the account of their tenets from hearsay and popular estimate, he made a serious study of the works of a famous champion of Roman theology, Cardinal Bellarmine; and to the close of his life he was able to cite concessions

or ingenious replies, which he had met with in the pages of that eminent controversialist. He had been brought up, and always remained, a devoted son of the Reformation. But he was not averse to the study of the schoolmen, especially Aquinas; and he recognised, not wholly without admiration, in the theology which he so firmly opposed, a system which, in his own words, "touched the human mind at a great many points."

Again, in South Africa he found a system of law very different from that to which he had been accustomed in England. Dutch law, like Scottish law, is largely based upon that of ancient Rome. Straightway he became a student of Roman law. Whether the study of that or of any other system will render a mind judicial, if it is thoroughly imbued with the advocate-temper, may be doubted; but it can hardly be questioned that where its teaching falls on a congenial soil, it has a tendency to strengthen the upgrowth of a judicial harvest by reason of its general essence of admirable clearness and common sense. In 1878 some difficulties in the will of the Misses Walker were brought before the First Division of the Court of Session in Edinburgh. Bishop Cotterill's interpretation of certain clauses had been disputed by some of the Walker Trustees, including at least one eminent lawyer. The Court entirely confirmed the Bishop's view, and rejected that of his chief opponent.

As a scholar and a divine he was naturally much interested in the Revision of the New Testament. In the main he sympathised with the Revisers, and in general preached from their version. In the field of what may perhaps be called constructive theology his most important production was probably the volume entitled *The Genesis of the Church*, an original and thoughtful work, which may, though in a quiet and comparatively unnoticed manner, considerably influence the divinity of the future.

But as was to be expected from one who kept up his acquaintance with physical science, Bishop Cotterill was especially drawn towards the field of apologetic theology. Among contemporary writers none fixed his attention more than the late Dr Mozley, whom Sir James Paget—no mean judge—has called "perhaps the most philosophic divine of our age." *The Unseen Universe* at once arrested his notice, and in 1876 he contributed to the third number of the *Church Quarterly Review* a sympathetic and able criticism of this remark-

able work. This was, I believe, his only contribution of any length to periodical literature. He was also, however, greatly struck by *Philosophic Doubts*, the work of a gentleman since known as the Right Hon. A. G. Balfour, successively Secretary of State for Scotland and for Ireland; all the more so in that he had prepared for the Victoria Institute a paper based upon a somewhat similar stratum of thought, entitled "The Relation between Science and Religion, through the principles of Unity, Order, and Causation."

This last-named paper was read in 1880. But it may be regarded as a continuation of a similar address delivered before the same Institute in 1878, "On the true Relation between Scientific Thought and Religious Belief;" which was followed by what many consider his happiest effort in this direction, namely, the small volume entitled *Does Science aid Faith?* published in 1882.

His acquaintance with colonial life not only led to his being on several occasions associated with English prelates in the choice of Anglican bishops for distant sees, but also induced him to deliver addresses bearing on the problems connected with modern civilisation. Among such papers may be named "Vital Christianity as affected by the present State of Science and Civilisation," read at Leeds during a Church Congress held there in 1873, and three lectures given at St Paul's Church, in York Place, Edinburgh, entitled "Progress." In compositions of this nature he not unfrequently avowed his obligations to several writers outside the ordinary range of theological study; such as, for instance, Fichte and Mr Herbert Spencer.

Very friendly relations, dating from the Lambeth Conference, existed between him and the Anglican prelates of the United States. He paid a visit to America, which he greatly enjoyed, in 1880; and was subsequently appointed Bedell Lecturer. These lectures, however, were read for him in 1884. Their subject was "Revealed Religion in Relation to the Moral Being of God."

There have been men of science who have combined with their gifts of knowledge that of a remarkable literary style. Such in France was the famous naturalist Buffon; and no one, whether ally or opponent, would deny this possession to Professor Huxley. It may be doubted whether, in the case of Bishop Cotterill, the gift of

expression was quite on a par with the general level of his very high and varied endowments. It was probably, as a rule, happier in friendly conversational discussion than in set and formal efforts, whether spoken or written. With diffidence it may be suggested that the little volume already named, and the article in the *Church Quarterly Review*, are favourable examples of his style, when at its best.

He met the announcement that a fatal disease had seized him with singular calmness and Christian fortitude. His illness was cheered not only by the devoted assiduity of the partner of his life, and the other members of a most united family, but also by a sympathy which extended far beyond the limits of his own communion. He left the Church, over which he had presided, very grateful to him for his work, which had not only won the affection and respect of those worshipping in the Cathedral and other Episcopal churches, but had also tended to draw into closer communion two congregations which had previously been disunited. He was probably happier in a disestablished than he would have been in an Established Church, inasmuch as the conflicts in England between Church and State were to him a source of perplexity and regret.

He used to praise his contemporary, Archbishop Tait (in company with whom he had been consecrated Bishop), for never becoming too old to learn. It was an eulogy which might be fairly applied to himself. The condition of Scotland was in many respects very different from that of South Africa. He took pains to learn those differences. From his visit to America, from events of the day, from thinkers much younger than himself, if they were adepts in any special lines of thought or study, he was most willing to learn;* thus showing that he cherished in his inmost heart a deep humility, which chastened what might have been the temptation of his great acquirements and successes. Most prominently did this and other graces shine forth during the period of his latest illness in 1885.

* The Rev. David Greig, M.A. of Aberdeen University, now Rector of Cottenham; and Dr Dowden, who has succeeded him in his Episcopate, may without invidiousness be named as illustrating this remark. The Bishop was also very sensible of the value of recent works of learned Presbyterian divines, such as those of Professors Flint and Milligan.

[*Note added December* 23, 1887.]

Since the above notice was written, Professor Tait has reminded me of the name of Dr Perry, late Bishop of Melbourne, as that of a Senior Wrangler, who, like Bishop Cotterill, had also taken a high place in the Classical Tripos; and my friend and relative, the Rev. Dr Luard, Registrary of the University of Cambridge, has, at my request, furnished me with a list of the most interesting examples of double honours taken at Cambridge between the years 1753 and 1854. I pass over those who did not win the highest place in one of the two departments (though it contains many names of students who rose to celebrity or high station in after life), and confine myself to a selection from the list of Senior Medallists, Senior Classics, or Senior Wranglers.

Among the Senior Medallists, who were also very high Wranglers, may be named Craven (afterwards Master of St John's), 1753; Halifax (Bishop of St Asaph), 1754; Law (Bishop of Elphin), 1766; Law (Bishop of Bath and Wells), 1781; (Archdeacon) Wrangham, 1790; Maltby (Bishop of Durham), 1792; Tindall (Chief Justice), 1799; Grant (Lord Glenelg), 1801; Parke (Lord Wensleydale), 1803; Blomfield (Bishop of London), 1808; Graham (Bishop of Chester), 1816; Hugh James Rose, 1817; Ollivant (Bishop of Llandaff), 1821.

Among the Senior Classics (some being also Senior Medallists) who were very high Wranglers, were (Professor) Selwyn, 1828; (Professor) Westcott, 1848; J. B. Lightfoot (Bishop of Durham), 1851.

Of the four Senior Wranglers, who have also been Senior Medallists, only two became subsequently eminent—Kaye (Bishop of Lincoln), 1804, and Alderson (Baron of the Exchequer), 1809.

The other Senior Wranglers who have been high in the Classical Tripos (since its institution in 1825) have been comparatively few; and Henry Cotterill appears to stand alone in combining with the Senior Wranglership, and a high place in the Classical Tripos, the position of First Smith's Prizeman.

William Denny, C.E. By John Henderson, Jun., F.R.S.E.

William Denny was born in Dumbarton on 25th May 1847. His education was commenced in the Academy of his native town. Shortly afterwards he was sent to Jersey, more particularly for the sake of his health, and after a residence of four years he returned to Scotland, and was placed in the Edinburgh High School, where he remained until he was seventeen years of age.

He had resolved to become a shipbuilder, and on leaving the High School in 1864 he entered his father's ship-yard in Dumbarton as an apprentice, and passed through the several departments, where he laid the groundwork of his future ability as a naval architect.

In 1868 Mr Denny became a partner in the firm of William Denny & Bros., and shortly after assumed the administrative charge of the extensive business conducted by the firm. He continued to occupy this position until his death on 17th March 1887.

He early showed great promise of becoming an eminent naval architect, and introduced a more scientific basis in all the practical work of shipbuilding. He wrote many papers, and took a prominent part in all discussions in connection with his profession.

Mr Denny had great force of character, and a wonderful gift of inspiring his staff, and those with whom he came in contact, with his own enthusiasm and earnest perseverance in carrying his investigations and experiments to a conclusion.

He was a fluent speaker, and his professional and other papers, which he read from time to time, were noted for the finished manner in which they were prepared and delivered.

He took a prominent part in the introduction of steel for shipbuilding, and did much to bring it into the general use it now obtains. He introduced an admirable system of conference between his firm and delegates from all trades represented in the works, in order to adjust and arrange all questions of wages, rules, &c., and these conferences were under his presidency during the last year of his life at home.

Such deep and active interest as he had in everything connected with the scientific part of his business would have absorbed all the energies of most men, but not so with William Denny. His nature was many-sided, and his sympathies were broad and deep. He was

well read in many subjects, and proved himself a capable lecturer on art, politics, and many other subjects, in which he took a deep interest. Possessed of a warm and affectionate nature, he will be long remembered for the interest he took in every one with whom he came in contact, and the generous, but wise, assistance he was ever ready to afford in advancing the well-being of all who had the good fortune to know him.

Among the papers which he wrote were "The Worth of Wages"; "Dimensions of Sea-going Ships"; "On the Difficulties of Speed Calculations," for the latter paper he was awarded the Marine Engineering Medal of the Institution of Engineers and Shipbuilders in Scotland; "The Speed and Carrying of Screw Steamers," being the Watt Lecture delivered before the Philosophical Society of Greenock; "On the Question of Success"; "Christianity in this Life"; of the latter he was only spared to conclude the first part.

Mr Denny was appointed by the Government a member of the Load Line Committee, and he took an active part in all its investigations. He was a member of Council of the Institution of Naval Architects, a member of the Institution of Engineers and Shipbuilders in Scotland, of which he was president at the time of his death. He was also member of the Institution of Civil Engineers and of the Iron and Steel Institute. He was elected a Fellow of this Society on 3rd February 1879.

Dr Daniel Rutherford Haldane.—From materials supplied by Dr John Smith, LL.D., and Dr Heron Watson.

Dr D. Rutherford Haldane was the son of James Alexander Haldane, who founded the Scottish Congregational body, and has sometimes been called the Whitefield of Scotland. He was of the family of the Haldanes of Gleneagles.

Our deceased Fellow was educated at the High School of Edinburgh, and during the six years of his attendance there his usual place in class was about third,—the dux for the first three years being "blind Laurie," and when that distinguished pupil left,

the next dux was one who afterwards has done good work in another profession, the Rev. John M'Laren, D.D., minister of Larbert.

On leaving the High School, he studied at the University of this city until he obtained his medical degree. When he graduated as Doctor of Medicine, a gold medal was presented to him for his thesis on Diseases of the Liver.

He subsequently went abroad for the purpose of further study at the great medical schools of Vienna and Paris. He resided at the latter capital for eighteen months, and whilst there acquired a remarkable fluency in the French language, which he ever afterwards retained. On his return to Edinburgh he was appointed House Physician to the Infirmary, in which capacity he had acted prior to going abroad, and not long after this he was elected Physician to the Royal Public Dispensary and the New Town Dispensary. He subsequently became a Lecturer on Medical Jurisprudence, Pathologist to the Royal Infirmary, and Teacher of Pathology and Morbid Anatomy in the Extra-Mural School at Surgeons' Hall. On Dr Alexander Wood retiring from his Lectureship at the College of Surgeons, Dr Haldane began to lecture on the Practice of Physic in that institution, and at the same time he gave lectures on Clinical Medicine at the Infirmary. His classes were very popular with the students, as he had the reputation of being one of the best teachers in the Edinburgh School of Medicine. In 1876, about three hundred of his former pupils, among whom were many of our best known practitioners, presented him with an address in which his powers of accurate diagnosis, the clearness and grace of his style as a lecturer, and his vivid powers of description, were adverted to in terms of grateful appreciation.

In 1852 he was elected a Fellow of the Royal College of Physicians, and he afterwards held successively the offices of Secretary and President of that body. Whilst acting as Secretary to the College of Physicians, he took an important part in promoting the scheme under which the Royal Colleges of Physicians and Surgeons in Edinburgh, with the Faculty of Physicians and Surgeons of Glasgow, arranged to grant a conjoint examination and diploma to their students, known as the Double Qualification.

For his services in instituting this diploma, a handsome service of silver plate was presented to him. He was the representative of the College of Physicians at the General Council of Medical Education, and at the Infirmary Board. The General Council, Edinburgh University, elected him their assessor at the University Court.

He was also for several years medical officer of the Scottish Equitable Life Assurance Society. In acknowledging his services in this capacity, the Directors stated in their minutes—"that his perfect mastery of his profession, and the sound judgment which characterised his opinions were so conspicuous, that the Board placed the most perfect confidence in his advice, and are certain that it has been of the utmost value to the Society."

Dr Haldane chiefly devoted himself to the teaching and consulting departments of his profession. His contributions to the science of medicine were mainly of the nature of occasional practical papers, read before various professional bodies and associations, and constituted in not a few cases interesting additions to our knowledge of diseases of the heart, the nervous system, and alimentary canal. For a number of years he edited the *Edinburgh Medical Journal* with ability and success.

His life was destined to be cut short unexpectedly. On the Christmas day of 1886, when leaving his house, his foot slipped, and in falling he broke the lower part of his right leg. Complications, leading to a complete collapse of his system, followed on this injury, and he died on the 12th of April 1886.

The death of a physician so eminent and so widely known and appreciated, was the occasion of much regret both to the profession and the public of Edinburgh; and the more so as in private life he was distinguished by the gentleness and courtesy of his manners, his straightforwardness and high sense of honour. His funeral was attended by the Presidents and Fellows of the Royal Colleges of Physicians and Surgeons, and by a large assemblage of the general public. He was elected a Fellow of this Society in 1867.

JAMES PRINGLE.

James Pringle was born in Edinburgh in 1822. He was the son of Mr Murray Pringle, who for thirty years held the office of Secretary in the Adjutant-General's Office, and a similar position in the Naval and Military Academy, which long existed in the city. He received his education at the High School of Edinburgh, where he made great proficiency in the study of the classics, and stood third among his fellow-students in the list of honours during the last year of his educational course.

On leaving the High School he became a clerk to the Edinburgh Roperie Company. His business abilities and capacity for hard work led to his being promoted to the position of cashier in the Company's office, and on the occurrence of a vacancy in the management the entire responsibility of conducting the Company's business was devolved on him.

About six years ago, he began to take a prominent part in the public business of Leith, when he was returned as representative of the ratepayers to the Leith Dock Commission. Entering the Town Council of Leith in 1881, he was elected Provost of the burgh in November of that year, and continued to occupy the position till the time of his death. Like his predecessor in the civic chair, he was an ardent supporter of the Leith improvement scheme, and he was particularly active in obtaining a Provisional Order for the better preservation of Leith Links. He displayed great tact and energy, as well as unfailing courtesy, in the discharge of his onerous duties as chief Magistrate, and he took an active interest in all associations and movements for the amelioration of the condition of the poor, and the relief of suffering.

Mr Pringle also held the honourable office of Deputy-Lieutenant of the county. He found time to take an intelligent interest in the proceedings of the Geographical Society, and he was elected a Fellow of this Society on the 6th of April 1885. He died on the 11th of December 1886. His funeral, which was a public one, called forth an expression of general sympathy and regard, such as, perhaps, had never before been witnessed in the town over which he presided.

Dr Thomas Williamson.

Dr Williamson was born at Greenock in 1815, and claimed to be the last surviving representative of the ancient family of the Napiers of Kilmahew. When quite young, he was sent to study medicine at the University of Edinburgh, where he took his degree of M.D. at the age of twenty, in the same year becoming a licentiate of the Royal College of Surgeons, an institution of which he was elected a Fellow in 1857. He was one of the oldest members of the Medico-Chirurgical Society, and of the Royal Society of Edinburgh. In early life he held several public offices. He was for fully thirty years surgeon in Leith Hospital, and during the whole of his long residence in Leith took a warm interest in the welfare of that institution. At the time of his death he held the appointments of parochial medical officer and public vaccinator for the parish of South Leith, and medical officer of health for the entire burgh. The latter office obliged him to board vessels coming from ports where any epidemic was prevalent, and to his foresight is due the readiness in which Leith has been held, during the late visitations of cholera on the Continent, to deal with any emergency which might arise. His various public duties were performed with untiring vigilance and care, and he enjoyed the unqualified respect of his fellow citizens, among whom his long residence rendered his figure one of the most familiar in the community. Dr Williamson was an occasional contributor to medical journals on topics chiefly relating to sanitation. He published various pamphlets, and delivered several popular lectures on subjects relating to public health, and read a paper on the subject at one of the meetings of the Social Science Congress at Edinburgh, which was printed in their reports. On returning from Colinton, he was seized with an attack of apoplexy which terminated fatally on the 30th of December 1885. He was elected a Fellow of this Society on 7th December 1857.

John Milroy, Assoc. Inst. C.E.

The death has just been announced of Mr John Milroy, Assoc. Inst. C.E., one of the remaining links between the present and the great days of railway construction in which the late Mr Thomas

Brassey played a prominent part. Mr Milroy died at his residence, Torsonce House, Stow, near Edinburgh. For the past two years he had been in failing health, and ten days before his decease he was seized with a shock of paralysis, from the effects of which he never recovered. Mr Milroy was very early associated with Mr Brassey in railway work, so far back, indeed, as the year 1840, in the construction of the first line between Glasgow and Greenock, now belonging to the Caledonian Railway Company, and on which he was a sub-contractor. The most serious portion of that contract was the cutting of the well-known Bishopton Tunnel, which was carried almost entirely through a dense tough whinstone. Mr Milroy subsequently acted as agent for Mr Brassey and for Messrs. Brassey & Mackenzie in the construction of lines of railway of great extent on the Continent, the first of them being the Paris and Rouen Railway, with the late Mr Joseph Locke as the engineer-in-chief. He was also engaged in the same capacity on the Rouen and Havre, the Nantes and Caen, and the Caen and Cherbourg railways. A few years later Mr Milroy likewise served Mr Brassey as his agent in the construction of well-nigh eighty miles of the Great Northern Railway; and there were various other railway undertakings with which he was connected, not only in this country, but also in France and Italy; indeed, a considerable portion of his life was spent on the Continent, where he became exceedingly well known and greatly esteemed on account of his personal character. About a quarter of a century ago, Mr Milroy settled down in his native country, in order to take charge of some large works in which he was interested, along with the eminent firm with whom he had been already associated for about twenty years. The chief of those works was the construction of the City of Glasgow Union Railway, from the plans of Mr (now Sir) John Fowler. It included some difficult pieces of constructional work to connect the Glasgow and South-Western Railway on the south of the Clyde with the North British system of railways on the north side. There was also a very important iron girder bridge across the Clyde, together with the Sighthill Railway and the Harbour Railway running past Pollokshields and under a public roadway, a canal, and two other lines of railway. While engaged in sinking the cylinders for the viaduct over the Clyde, Mr Milroy

brought into use a new excavator of his own invention, which subsequently did much excellent service in the construction of subaqueous works. While in and about Glasgow the deceased was induced to take two important contracts on his own account, two pieces of work involved in the extension of the harbour of Glasgow, namely, Plantation and Mavisbank Quays. In the former of these works, the superstructure was built on foundation piers which were formed of successive rings of brick-work, according to the plans of Mr James Deas, engineer to the Clyde Trust. In the construction of Mavisbank Quay, a marked improvement was made in the character of the subaqueous pier foundations, which were formed of concrete, the piers being most securely bound together. The Milroy excavator was here used to excellent purpose, enabling the piers to sink to depths of 50 ft. to 60 ft. or 70 ft. In these harbour works Mr Milroy was closely associated with Mr Deas, and he had as his right-hand man Mr George A. Waghorn, who also saw much service under Mr Brassey's firm. The various works which he carried out in the Glasgow district, including those for which he was the sole contractor, cost nearly $1\frac{1}{2}$ millions sterling.

After having retired from active life, Mr Milroy passed his remaining years on the estate of Torsonce, which he acquired in the year 1879, and on which he occupied his time in making considerable improvements by building, roadmaking, &c. At his death he was eighty years of age. He was of a most retiring disposition, and when not occupied in superintending the improvements on his estate, devoted the last years of his life to the study of those branches of science which were most closely connected with his profession. He was elected a Fellow of this Society in 1875.

General Sir James Edward Alexander, Kt., C.B., K.C.L.S., &c.

General Sir James Edward Alexander, of Westerton, Stirlingshire, who was born in Stirling on the 16th October 1803, and whose decease took place at the Isle of Wight 2nd April 1885, was a collateral descendant of the family of the first Baronet, William Alexander of Menstrie, afterwards Earl of Stirling.*

After passing through the College of Edinburgh and Glasgow, he proceeded at an early age to India to join his relative Sir Thomas Munro, then governor of Madras. He there devoted himself to the study of Oriental languages, passed the required examinations, and was appointed to the Madras Light Cavalry, and adjutant of the governor's body guard. He was afterwards transferred to the 13th Light Dragoons in January 1825, and volunteering for active service proceeded to Burmah, was present in the field, and took part in the first Burmah war.

After the peace his acquirements were recognised by his being appointed attaché to the Persian Mission, under Sir John Macdonald Kinneir; while acting with the Persian army on the field against the Russians, he so distinguished himself that he received the Order of the Lion and Sun.

In consequence of his proficiency in Eastern languages and general attainments, he was offered, on return to England, a professorship at the College of Heylebury, but, declining this employment, he instead joined the senior department of the Military Staff College, obtained a first-class certificate, and shortly afterwards was promoted to a lieutenancy in the 16th Lancers. He then obtained a year's leave of absence, to enable him to complete his military studies, and to join the Royal Engineers at Chatham, under Sir Charles Paisley.

He next saw service with the Russian army of Field-Marshal Diebitch, then engaged in operations against the Turks. At the conclusion of this service, and while on his return to England, a slightly untoward incident occurred to him. Proceeding in a Russian frigate by way of Sebastopol, he was there placed in quarantine, in consequence of some cases of the plague having

* He was buried in the old Logie kirkyard, near Menstrie.

appeared on board. The port chanced to be visited at the time by H.M.S. "Blonde," commanded by Captain, afterwards Sir Edmund and Lord Lyons (commander-in-chief of the fleet during the Crimean war), then cruising in the Black Sea. Captain Lyons alone was allowed to land at the quarantine station, when he naturally communicated with his fellow-countryman. On the departure of the "Blonde," Sir James Alexander was immediately arrested by the Russian authorities, on suspicion of being an emissary of the British Government. He was arbitrarily confined two months in Sebastopol with other prisoners, and finally, in the depth of winter, sent under guard to St Petersburg, where, after undergoing further confinement and hard treatment, he was at length, by the intervention of the British ambassador, released, but without compensation for the unjust usage he had received—only a slight apology from the Emperor Nicholas in person.

He returned to England by Sweden and Denmark, and in recognition of the valuable information, plans, and reports he was enabled to furnish with regard to Russia and Turkey, he was promoted to an unattached captaincy.

Sir James was now selected by the Colonial Office to undertake an important commission of inquiry into the state of slavery in North and South America, receiving from the Secretary of State letters and credentials to the various governors of provinces, &c. On the conclusion of this mission he was examined before a committee of the House of Lords, by whom his able report was highly appreciated.

He shortly afterwards returned to full pay, and joined as a captain the 42nd Highlanders (Black Watch). While serving with this regiment, he was invited by the Royal Geographical Society to make explorations in Africa, and readily accepted a duty so congenial to his active and enterprising character; but the British expedition then being in the field against Don Miguel, he took the opportunity it afforded him of seeing further active service and acquiring further geographical knowledge by joining the expeditionary force. His effective service was rewarded by receiving from Don Pedro the rank of lieut.-colonel. He then proceeded on his mission in H.M.S. "Thalia" round the west coast of Africa, visiting the different settlements.

On arrival at the Cape, finding the war with the Caffres already commenced, and the time accordingly unfavourable for explorations, he joined the troops in the field under Sir Benjamin D'Urban, by whom he was appointed aide-de-camp. At the conclusion of the war, Sir James Alexander resumed his mission of exploration, and proceeded into the interior, accompanied only by seven men—encountering successfully the dangers, difficulties, and hardships to which at that time travellers in South Africa were subjected. In one year he accomplished 4000 miles, and completed a full report of the countries of the great Namaquas, Boshmans, and Hill Damaras.

On returning to England, Sir James E. Alexander received the honour of knighthood for his services in Africa, being the first knight created by the Queen in person after Her Majesty's accession. His African duties had obliged him to go temporarily on half pay; but he returned shortly to full pay as a captain in the 14th Regiment, then serving in North America. There he was asked to undertake and accepted the arduous duty of exploring and surveying in the construction of a military road through the forests of New Brunswick and Canada from Quebec to Halifax, acting as assistant royal engineer on this most trying service during 1844–45. He received no promotion or reward, beyond a slight addition to pay while so engaged, for this arduous service which he had been invited to undertake.

On Sir Benjamin D'Urban being appointed commander of the forces in Canada, he immediately reappointed Sir James as his aide-de-camp, a post he retained until Sir Benjamin's death in Montreal. Sir William Rowen succeeding to the command, he was again offered the aide-de-campship, and served on the staff of that distinguished officer for 5½ years. He rejoined his regiment as major on the breaking out of the Crimean war, shortly became lieut.-colonel, and succeeded to its command during the siege of Sebastopol, at the fall of which stronghold he was present.

During this time of scarcity and hardship, his regiment was notorious for the beneficial arrangements for their supply and comfort that his previous experience on active service had enabled him to make for the comfort and welfare of his men.

Sir James Alexander was next appointed to the command of a

depôt battalion, but was shortly selected to raise the 2nd battalion of his old regiment, the 14th; this he so quickly did, and so rapidly brought them into an effective state, that his battalion was the first of the new 2nd battalions that proceeded on service in the field; this was to New Zealand, to engage in the Maori war. Here he commanded for some time in the province of Taranaki, and under Sir Duncan Cameron the important outposts at Waikato.

He was then promoted to major-general in 1868, lieut.-general in 1877, and general in 1881. He received during his service seven war medals, but the reception only of the C.B. (3rd Class of the Order of the Bath) will be considered but an inadequate acknowledgment of long and arduous service.

Sir James Alexander was the author of various works of travel and of a biographical and military character, such as his *Life of the Duke of Wellington*, *Canada as it is*, &c., *Passages in the Life of a Soldier*, and others relating to the various countries with which he was personally acquainted, as well as the contributor of numerous articles in periodicals on the military, scientific, and social topics of the day.

The deep interest he took in all questions relating to the interests and improvements of the army, especially as regarded its equipments, and his untiring exertions in promoting these objects to the utmost of his power, are well known. He was among the foremost and most strenuous supporters of those who called public attention to the long-neglected justice of granting medals for the Peninsular services of the army, hitherto unrecognised by distinctive decorations. This movement was eventually carried to a successful result, through the influence and advocacy of the Duke of Richmond in the House of Lords, and with the military authorities, was much indebted to the indefatigable exertions of Sir James Alexander.

Mainly also to his exertions may be truly attributed the ultimate erection of the Egyptian obelisk (called Cleopatra's Needle) in its present site on the Thames Embankment. This obelisk, presented by Egypt to England in recognition of the services of the British army in Egypt under Abercrombie, owing to untoward circumstances which prevented its shipment to England, for which arrangements had been made at the expense of the army, was allowed from that up to the present to lie neglected on the shore of Alexandria

harbour, at the point intended as that of embarkation; and the English Government having refused to incur the expense of its removal, it would finally have been broken up in 1874, had not Sir James Alexander, who had long endeavoured to call public attention to the matter, personally interfered, and undertaken at his own expense a voyage to Alexandria, and, with the aid of the British consul-general, succeeded in rescuing it from destruction. On his return he renewed his exertions, so many years unsuccessful, and obtained the munificent pecuniary assistance of Sir Erasmus Wilson, by which after great difficulties the obelisk was transported to England and erected in its present site. The country must be considered indebted to Sir James Alexander's persevering energy for its possession of this most valuable and interesting antiquity.

It would be difficult to enumerate the various other works of public and private character, whether in England or abroad, in which he was constantly engaged, and bore a leading and prominent part; these will long be gratefully remembered by those who benefited by his exertions.

Of him may truly be said, that in all countries whatever good work he found at his hand to do that he did with heart and soul; and in him those who had the privilege of his acquaintance recognised that highest type of character, the single-hearted, high-minded Christian gentleman, whose life was devoted to duty and to the promotion of the interest and welfare of his fellow countrymen, and the communities among whom his lot might be more immediately cast.

Alexander James Russell, C.S.

Mr Alexander James Russell, Clerk to Her Majesty's Signet, Edinburgh, who died recently—8th January 1887—at the age of seventy-two, was head of the firm known formerly as Russell & Nicolson, C.S., and latterly as Russell & Dunlop, and the business which he carried on was one of the oldest in Edinburgh, dating back to the end of the seventeenth century, and having descended in the direct line from father to son.

His father, Mr John Russell, was principal Clerk of Session, and

Secretary of the Royal Society of Edinburgh, and his great-great grandfather was solicitor for the sale of the forfeited estates in Scotland after the Rebellion of 1715, and one of the original shareholders of the Bank of Scotland. On his mother's side, Mr Russell was closely related to the ancient Scottish family of Murray of Polmaise, his mother having been the daughter of the late Mr Murray of Polmaise, and he was a great-grandson of Principal Robertson, the Scottish historian. He was for many years a director of the National Bank of Scotland and of the Standard Life Assurance Company, and for some time latterly solicitor to that Company. In politics he was a Conservative, but he did not take any active part in political life. Mr Russell was married—first, to Miss Magdalene Stein, daughter of the late Andrew Stein, Esq. of Wester-Greenyards, Fifeshire, and secondly, to Miss Elizabeth Anne Lancaster, daughter of the late Samuel Lancaster, Esq. of Hemborough, Devon, who now survives him. He left also an only son, Colonel J. Cecil Russell, late of the 12th Lancers, and for some time an equerry to the Prince of Wales.

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